Thus K_F (Unloaded Hole) = 1 55 (Standard Data)

$\bar{K}_F$ (Loaded Hole) = 1 05 (From Fig 11)

f_u = 27,000 lb /sq in

f_l = 3,720 lb /sq in

$f_u + f_l$ = 30,720 lb /sq in

Thus from Fig (10)

N = 1 5 × 10^6 cycles mean curve

= 0 35 × 10^6 cycles —5% scatter curve

Actual failure of specimen 2 58 × 10^6 *cycles*

Example 2 *Typical Skin/Stringer Joint*

(*Low sheet stress—High rivet load*)

Data

Sheet	20G
Sheet Stress	± 11,200 lb /sq in
Rivet Load	± 80 lb
t/D	0 288

Thus (as example 1)

f_u = 17,300 lb /sq in

f_l = 18,700 lb sq in

$f_u + f_l$ = 36,000 lb /sq in

Thus from Fig 10 N = 0 17 × 10^6 cycles mean line

= 0 12 × 10^6 cycles —5% scatter line

Actual failure of specimen 0 21 × 10^6 *cycles*

NOTE

In example 2, the value of f_l is of the same order as f_u, consequently any increase in the rivet load would substantially reduce the general stress level allowable for a given life

Discussion

Mr P E Q Shunker (*Westland Aircraft Ltd*), who opened the discussion, congratulated the Author on a most interesting paper on the very difficult subject of rotor blade design and stressing Like any other structure, the rotor blade must be analysed in two parts

(i) the critical external loading which must be derived, and

(ii) the strength assessment of the blade under that loading

Since the blades were part of a dynamical system, the external load analysis was concerned not only with the air loads themselves, but also with their associated inertia loads

The paper demonstrated a method for the solution of the dynamic problem, using the elegant devices of the matrix algebra The Author had made a valuable contribution on this particular aspect For example, he had placed the so-called "ground resonance" phenomenon in its correct perspective as being part, albeit extremely important, of the general dynamic picture In discussing the effects of dynamic phenomena he had pointed out the pitfalls to which the unwary were prone and in his analysis he had demonstrated the importance of the "body freedoms" One would imagine that this was more important in the case of the ultra-light than in heavier aircraft

The first difficulty with which one was faced, however, in blade analysis was the question of the aerodynamic loading While the Author acknowledged that much remained to be done on this point, the paper could have been considerably enhanced

https://doi.org/10.1017/S2753447200003437 Published online by Cambridge University Press

if more attention had been given to this subject The effects of compressibility and blade stall on the blades, and consequently on the helicopter itself, played a significant part in the success of a high speed helicopter

Mr SHUNKER quoted two examples of the effect upon the design of a helicopter On one particular helicopter, the effect of blade stall was to increase the measured vibratory loads in a member of the control system by something like 300 per cent when the tip speed of the retreating blade decreased by about 10 per cent Covering a range of rotor r p m and forward speed, any question of resonance for a given r p m was eliminated On another helicopter, the control loads were increased by 200 per cent for an increase of approximately 60 per cent in the forward speed , and this was attributed to the effects of compressibility

These illustrations confirmed the Author's plea on the need for a closer study of the aerodynamic loadings on the blades and, hence, on the rotor, so that high cruising speeds—certainly for those interested in the pure helicopter—were not placed in jeopardy

The Author had quoted the lag plane stresses shown in Fig 6 as being of the order of ± 1 ton/sq in To what portions of the blade did this refer, chordwise ? Secondly, was there any flap for trim during the measurement ? Only lag plane bending stresses were quoted, even in the case of hovering

One would also like to have seen some comparison between the estimated and measured values of the stresses on these helicopters, as such figures would have given greater significance to the factor of 2 which the Author suggested should be applied to the first and second harmonic calculated values, to bring them into line with practical results

Reverting to the discussion on the undercarriage design loads, Mr SHUNKER said that the remarks made with respect to blades could, of course, be generalised to the whole helicopter He asked that more rationalised undercarriage design cases be formulated relating to the overall behaviour of the helicopter during taxying, take-off and landing

Concerning blade life, he wished to add some comment on the recommendation that the achievement of 10^7 cycles of stress reversal at first harmonic implied a life of about 1,000 hours on most helicopters It was possible to estimate the stresses as indicated in the paper, with empirical corrections where necessary Designwise, these stresses were realistic In view of the limitations of the estimates, the effect of higher harmonic stresses on blade life, particularly if amplification occurred at some higher harmonic, was not at this stage determinable since the empirical factor reputedly masked the truth

If, however, it was assumed that the *nth* harmonic amplitude of stress was the only damaging term, and it was arranged that the estimated stress was less than the value at 5×10^8 cycles, say, on the appropriate Fisher-corrected S-N curve of the material (which was assumed not to have a so-called endurance limit), the life of the blade on this score would be given by

$$\frac{5 \times 10^8}{\text{nth harmonic} \times N_1} \text{ hours}$$

where n was the harmonic and N_1 the first rotor cycles/hour

Using 10^7 cycles at 1st rotor cycles/hour as the blade life criterion, the life was

$$\frac{10^7}{N_1} \text{ hours}$$

For an nth harmonic to cause embarrassment,

$$\frac{5 \times 10^8}{n \times N_1} < \frac{10^7}{N_1} \qquad \text{or } 50 < n$$

https://doi.org/10.1017/S2753447200003437 Published online by Cambridge University Press

i e, the damaging harmonic must be greater than 50 It was unlikely, therefore, bearing in mind the effects of damping at higher harmonics and even allowing for amplification of these stresses, that the 50th harmonic term could be significant The figures quoted could, of course, be varied to achieve higher lives, with, no doubt, low harmonic orders

He was not quite clear concerning the point made by the Author in reference to the analysis of redundant structures The Author stated " that the 'calculated' stress at a given point results from assumptions which can usually be justified only on arguments for 'Ultimate Case' stressing " His own impression had always been that the normal methods of redundant structural analysis had depended on the assumption that the structure behaved as an elastic medium—*i e*, load was proportional to deflection Under the low stresses that must obtain for fatigue integrity, he would have thought that this analysis was more relevant to the fatigue cases than the usual " Ultimate Case "

The examples quoted by the Author on the effect of surface finish and capri-honing were extremely instructive A large number of critical stress conditions arose at locations where surface finish (as defined by a specified number of micro-ins) was hard to obtain, and even harder to measure (*e g*, radii at the root of lugs, etc) The practical process of, say, capri-honing tended to reduce the effect of so-called " bad " surface on fatigue strength and thereby tended to eliminate some of the doubtful terms in the design-manufacturing equation

Mr Rogers, in reply, said that the reason he had not had more to say concerning the aerodynamic loading was that the lecture was intended to occupy 50 minutes but had, in fact, taken over an hour, and, even so, it could be criticised as being sketchy Something had had to suffer, and to try to keep everything in perspective he had mainly wanted to indicate that everything in the garden was not lovely as far as the aerodynamic loading was concerned The more that was known about the other aspects which could affect the aerodynamic loading, the better If Mr Shunker or his company could let the Helicopter Association know more about these problems, everyone would be very interested

In reply to the question of whether any rotor flap occurred at the time hovering stresses were taken—*i e*, flap due to trim—he could not give exact figures, but he imagined that there must obviously have been a small amount of flap for trim on the Gyrodyne, and this would in fact affect the lag plane bending stresses On the Ultra Light helicopter the strain gauge results in Fig 6, were for a " tilting head " rotor in which there was zero mechanical flap in hovering

The point he was trying to make was that the order of the stress which was occurring was much greater than one would ever calculate in a systematic way for a hovering case with, say, a small flap angle It was of the same order as that which would be expected (with the same flap angle) at a reasonable forward speed

The points at which the stresses were measured were at the leading edge of the Ultra Light blade, and on the Gyrodyne blade, the construction of which Mr Shunker knew quite well, did in fact, occur on the leading edge of the elliptical spar

With regard to measurements against calculations, one could only compare measured results and flight results, and he agreed that the examples of this could be given to make the lecture complete Here again, in the interest of cutting out examples which would tend to make the lecture longer, all he could say was that this was the order of the measured against the calculated It must be left at that

Concerning blade life, he did not quite see the point in the equations Mr Shunker had given, and he would prefer to give his answer to this in writing The point he was trying to make was not necessarily that the stress level at $N = 10^7$ itself was significant, but that the stress levels which were calculated or measured ought to be inside the infinite life allowable It was necessary to be careful when dealing with the measured stress level, because the complicated pattern which was obtained contained many little wiggles Clarity was necessary in defining what were the cycles associated with a given stress Provided the whole lot was kept below the endurance limit, there should not necessarily be any failure

He was in entire agreement with Mr Shunker concerning reduntant structures If the redundant structure was analysed properly, he would fully agree What he had in mind was the case which perhaps criticised stress men as such When there was a problem which did not necessarily warrant a proper redundant structure analysis, one was tempted to make rather arbitrarily simplified assumptions He had merely drawn attention to the fallacy of such assumptions In such a case, one might not get the load going in the assumed path and consequently, unless one was very careful, the result of the load going in different paths might break both of them in turn

Author's Written Comment

In Mr Shunker's reference to blade life the reason for his obtaining n greater than 50 was simply because he had taken a value of N, 50 times greater than 10^7

Also one could not equate lives at different stress levels, *i e*, the stress level on an S/N curve at $N = 10^7$ is obviously different to that at $N = 5 \times 10^7$ unless the material had an endurance limit at 10^7

There was obviously a misunderstanding of the point the Author was trying to make Firstly, reference was only made to a steel rotor in which the endurance limit was assumed to occur at $N = 10^7$ This implied 1,000 hours approx at first harmonic, or 1,000/n hours for the nth harmonic

For a stress level slightly less than the endurance limit (in steel) infinite life would result for any harmonic

Mr F S Wood (*Royal Aircraft Establishment*), said that the views of R A E on fatigue substantiation were very close to those expressed by the Author, whose concluding remarks he heartily endorsed

He was very much in favour of the rational approach advocated by the Author towards the dynamic problem By looking at the problem in this general way, thereby seeing the various facets as particular cases of the general solution, one would go much nearer towards seeing what was important than by tackling each particular problem as one in itself If it was practicable to deal with it in this way, it was greatly to be favoured

He wished to cross swords with the Author in dismissing so briefly the question of ultimate design cases Without detracting from what was said in the paper concerning fatigue cases, which was of importance, Mr Wood felt that what happened in the general design approach was as follows The blade, or whatever the component was, was designed originally on a fairly simple basic assumption, which was usually an ultimate case It was then checked against other ultimate cases and against the fatigue cases As the Author had rightly pointed out, those other cases could only be properly assessed after a fair amount was known about the configuration of the aircraft Therefore, by the time that this was done, the blade was already being basically designed to the simple ultimate case and all that these other cases could do, unless something serious arose, was to make slight modifications perhaps in the course of the design and the testing

It seemed that one was still dependent to a large extent on the ultimate case for the initial design That being so, he wished to emphasise the Author's plea for more rational and realistic specification of ultimate cases of that kind

To the cases mentioned by the Author should be added that of jacking, which, as it stood at the moment, might be more critical than towing

On the general dynamical problem, it would be interesting if the Author could give some idea of what, in some particular case, had been the effect of body freedoms on the blade motions and stresses in flight

The **Author,** in reply, said that Mr Wood was quite right about the ultimate cases It should be realised however, that the substance of his lecture was not how to obtain the stresses in rotor blades in detail, but a logical approach to the problems which had to be considered When coming down to the actual detailed process of stressing a rotor blade, it was quite true that one must have a structure which could be analysed

A very simplified approach was that one obviously began with ground cases, which could be analysed to a certain extent, because at least the rotor was stationary and it could be treated as a static structure One, therefore, began by thinking about the structure in that aspect and apportioning certain sizes to the spars and the skin thicknesses, and so on, to meet this static case This was the point he had tried to bring out concerning the dynamic "towing" problem—dynamic inasmuch as the blade was an elastic body, and also that one must decide what one's case was to start with When one knew the case, one could have an initial stab at one of the basic ultimate cases

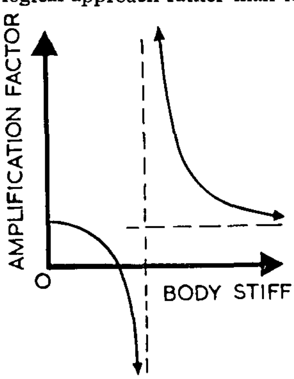
It did not, however, end there Having got a rough idea of the order of sizes that were needed, one must then consider other ultimate cases, which were, in fact, tied up with the dynamical amplification obtained in the associated ultimate case

The Author said that he was not necessarily considering fatigue cases when talking about the general problems The important point was that one began with a simple representation of the blade, and a simplified dynamical technique was applied to it It was possible to begin by assuming that the axis was fixed against translation One could do simple calculations, which were far from exact, which would help to put one in the picture One tended to go round and round until achieving something in the way of a rotor blade that was considered to be worth analysing

The main point about the business of blade design, which the paper had not mentioned but which was very important, and was probably not realised by those who were more concerned with stressing fixed-wing aircraft, was that on a fixed wing aircraft, it was possible to start with a case—say, the design of a fuselage or the design of a wing—and to a large extent the two people concerned with the different parts could work in isolation provided they balanced up when they met Generally, the design of the one part was not too much influenced by the design of the other part

In the case of a rotor blade, however, the design of one part considerably affected that of another and it was not possible to have a change of mind half way, as the blade was so fatigue conscious that bits could not be tacked on afterwards If that were to be done, the effect would be to start inducing large local loads at the attachment rivet holes, with the result of considerably reducing the fatigue allowable in those particular places

The net effect, therefore, was that one had to design the blade as a whole, and this resulted in a rather slow approach This fact was generally realised in a helicopter design office, and what usually happened was that the designer had a very fluid idea of what comprised the actual blade until almost the last moment, when the whole lot would knit into place and was finished The lecture was intended mainly to show a logical approach rather than to consider the actual design problem



Speaking of the body freedoms and their effect on the amplification, the Author said that earlier Mr Shunker had made a point which was quite true in that in terms of a large helicopter or a reasonable sized mass in relation to the rotor system, when approaching the question of forced oscillation in the air one did not, generally speaking, have to make allowance for the body freedom The effect of the body was usually quite negligible In the case of something like the Ultra-Light, however, in which a body freedom, and quite a flexible freedom, was intentionally put in, the picture could be altered, and there was no unique answer to it One could alter the pictures to any desired extent

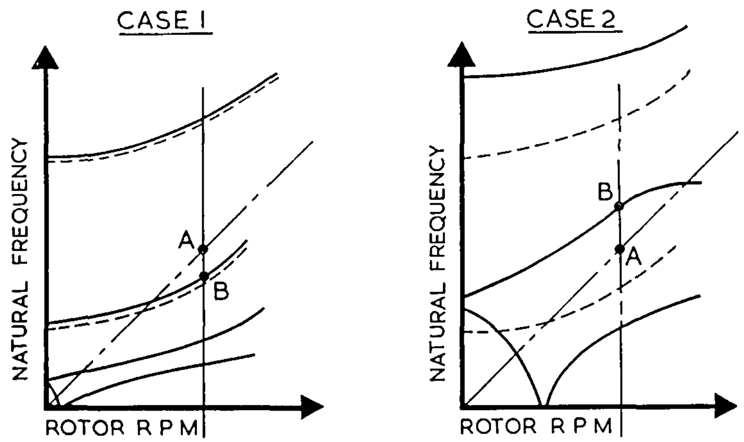
By doing either the complete problem, which was rather difficult, or the little simple sums, in which one played with the one-mass blade and possibly a one-mass body system, or, which was more complicated, the two-mass body system, it was possible to show the effect on the one-mass blade system of body stiffness and to plot a graph of body stiffness against the associated amplification factor (Say, ratio of an applied force at blade tip to associated force at rotor head)

The amplification factor associated with infinite body stiffness is that obtained from the normal assumption of zero translation of the body, and it can be seen that for a body stiffness in the region of the vertical asymptote the "zero translation" result would be very misleading

The same thing can be seen in a different way by considering the body frequency curves (Figs 5a and 5b) These illustrative curves although not disclosing the magnitude of the amplification factors associated with a given rotor R P M show the effect of the body on the resonance frequency in, say, an " air " case For example, by considering the diagrams below it can be seen that in one case the body effect has a negligible effect on the amplification factors obtained assuming zero translation of the rotor head, whereas in the other it has been considerably affected

It was therefore necessary to assess by simple means which of the above cases was relevant to the aircraft under consideration, since, if " Case 1 " was indicated it was not necessary to go to all the complication of taking the extra body freedoms into account

NOTE The distance AB is a qualitative measure of the amplification factor



-------- BLADE NATURAL FREQUENCY (ROTATING CO ORDS) "ZERO BODY TRANSLATION"

(*NOTE*—The dotted curves in both cases are the same)

Dr H F Winny (*Fairey Aviation Co*), who described the paper as a very useful survey of the problem of strength assessment of rotors, referred to the question of ground towing in the rotor static condition and said he wished to put in a plea that although it did not sound an important case, on large rotors it was bound to be quite a design case for the bottom skin of the blade, especially when there were heavy units at the tips

A factor of 2½ total had been suggested What was wanted was more data on this The analysis given by the Author with the one-mass system showed that the actual factor decreased with the time of application of the load Presumably, since it was a one-mass system, as the time of application was reduced the assumptions would finally become inapplicable, and with short times it would be necessary to bring in a two-mass system, to allow for the amplification of the first overtone

Presumably, the characteristic of the undercarriage would become very important, because it would not be possible to transmit the impulse, if it was of a very short duration, through the undercarriage system It seemed, therefore, that although the expressions gave some idea of what might be expected, it was a case of getting actual measurements to justify any such factor of $2\frac{1}{2}$ on the ground towing with the rotor stationary

Figs 5A and 5B of the paper clearly showed up the point of resonant frequency and of actual unstable conditions when the complementary function had no damped solution In the case of the instability ranges which took place at the higher frequency —those high up on the curve—presumably one could assume that they had more damping than those at the lower frequency This was very important, because with the picture that was shown a large amount of the rotor r p m range was being filled up with unstable regions One would like to know that those at the higher frequency were less likely to cause trouble

The recent report by Coleman and Feingold produced somewhat similar curves for the two-blade and the three-blade rotor, those for the two-blade rotor agreeing very closely with those shown by the Author

Referring to the question of fatigue, he wished to make a plea for adopting, as the Author had suggested, the factor on stress and not on life It seemed that the factor on life caused a lot of trouble, as the paper had shown, but it was not generally accepted by everybody that the factor should be one on stress

Fig 11 showed a reduction in the stress concentration factor as the value of t/D was reduced It is suggested that this result should be taken with caution until more tests had been carried out to establish this trend

One aspect of fatigue which was not mentioned by the Author but which might become more important was the question of size on the fatigue strength Some recent American reports had shown that size could make a very large difference to the fatigue life In one test, by increasing the size, the fatigue strength was reduced to one-third This should not necessarily be taken as being in any way general, but it might be that with the thin sheet (used for the results of low values of t/D in Fig 11) an appreciable size effect would be present The smaller the thickness, of course, the higher would be the allowable stresses from the tests and this might account for the reduction in K_f for low values of t/D

Mr Rogers, in reply, said that in the paper he had given two examples of towing, one for a rectangular distribution and the other for a triangular distribution, not to suggest that those were the types of acceleration necessarily obtained in towing, but in order to show that because different distributions of acceleration could exist, the results obtained were very dependent on the actual distribution

In the rectangular case, however long the time, it would not have made any difference There could still be an amplification of 2—*i e*, provided that the actual time in the cycle was long enough The meaning of the paper might not have been clear, since only maximums were quoted In these cases, there was a double maximum If the time of application was fixed, there would be a maximum condition associated with the time during the oscillation

For the rectangular distribution, it could be said that a time could occur in which a maximum was reached and that the maximum was independent of e, but provided that a suitable e was chosen, it would eventually pass through a value of 2 It could, however, be less than 2

In the triangular example, that was not the case For some t, one could start with a small e and then get a small amplification With some e—the e which gave the absolute maximum for which one was looking—it was possible to get a maximum amplification of 1 26 When e became large, the blade would oscillate up or down and one could obtain other values less than 1, dependent on the value of t

The point was to show, independently of the example, that there was here a little dynamical problem which had to be considered and that one could not simply arbitrarily dismiss a towing case as an ordinary ultimate case without thinking of the dynamical amplifications which occurred

If, as Dr Winny had suggested, the undercarriage did not allow all the applied acceleration to be imparted to the blade system then the type of blade acceleration obtained from a typical undercarriage, might be reduced from a large pulse to one tapering off to nothing This is very important since every ounce that one put on a rotor blade to look after the static case considerably affected its strength when considering the moving case

https://doi.org/10.1017/S2753447200003437 Published online by Cambridge University Press

Regarding damping effects on the ground resonance curve, obviously the whole range was not cluttered up with unstable ranges The main point of the lecture was to show how one could use the general dynamical problem to cover more degrees of freedom for the body One of the next things on which work should be done was the making of an attempt to put in damping characteristics of the body There had been " fun " in working out the equations as they stood at the moment, and there was likely to be a lot more " fun " when starting to put in the damping with these higher body modes

Here again, it was not unreasonable to see where trouble could arise even if no allowance was made for damping It must, however, be realised that damping occurred and that things were not as bad as they may seem Initially, it was better to show that it was possible to have an unstable range because of a combination of body modes It was better to know that other unstable ranges could occur by allowing for the extra body freedom than to ignore them, as in more simplified theories where, as a result of their neglect nothing would have been predicted

The Author had not seen the article by Coleman and Feingold and was pleased that it had been called to his attention The more data that could be obtained in agreement with basic theory, the more confidence one had in the results obtained Coleman and Feingold had done their work some years ago, in about 1947 It was because nothing had appeared on the scene for about ten years subsequently that it had been found necessary to do some work to introduce these effects

In this direction, too, a plea should be made In America, quite a lot of this basic work was sponsored by Government bodies One could imagine, even without knowing the details, that the new Coleman and Feingold report was prepared either at a University or by a Government body In England, however, when information of this sort was needed, the work must be done in the aircraft company itself One was usually busy enough solving all the day-to-day problems without trying to sit down and sort out the mathematics of these complicated problems

He hoped he had made it clear that the $\bar{K}f$ curves were only tentative His point was that a lot of test data—although no one should take this unduly to heart—in many periodicals related simply to *ad hoc* tests Such tests did not help the stress man very much What was needed was a method of applying these results to the problem in hand

In the case instanced in the paper, the concentration factors varied with sheet width Such results were rather useless because in practice one never had a sheet 1 0 in wide with a hole in the middle It was necessary to find some means of turning the coupon test data into a useful tool for the stress man The sooner people realised this, the quicker would the test data that was obtained in many *ad hoc* tests be of use to the normal stress office

(WRITTEN COMMENT)

The new Coleman and Feingold report mentioned by Dr Winny was N A C A T N 3844 This report was in fact a reprint of the earlier Coleman and Feingold's reports Refs 4 and 5, and consequently still only gave a solution for a body with one degree of freedom only

Mr J S Shapiro (*Consulting Engineer*) (*Founder Member*), said that the very remarkable edifice for analysing problems of blade stress which the Author had given would take a long time to digest and it was therefore difficult at this stage to form any judgment He thanked the Author particularly for producing his paper in such good time

He was not being unkind in saying that this magnificent edifice which had been presented was rather like a very elaborate coffee machine it made coffee but did not teach one how to make it He was, therefore, left with quite a number of problems which had been exercising his mind about blade stressing Probably the biggest of them all was one to which the Author had just referred when saying that he looked forward to a great deal more fun in introducing damping—not only the damping in the blade root, but also the blade damping in the whole blade, particularly due to aerodynamic forces If damping was neglected in the first harmonic, one would get no sensible result at all There was damping even in the higher harmonics

Mr SHAPIRO believed that the whole edifice would have to be changed somewhat, because there were no longer equations with constant coefficients One was, therefore, entering a completely new field of equations, and a lot of brushing up would be necessary

In the stressing of blades, in what directions—apart from the one which had been emphasised, of the introduction of body freedoms—had the Author's method of approach improved the correlation with practical tests and the ability to see through the problem ? For his own part, he had always found—at least, so it appeared from the older theories—that one could not disregard either internal damping in the blade or aerodynamic damping in the higher harmonics This was simply another plea for a little greater clarity in what appeared to be a very imposing rational approach

Some time ago, added Mr SHAPIRO, he had been engaged in a discussion on fatigue strength in the pages of a periodical, particularly with Mr Fisher When the R A E lug tests were mentioned to-night, he could only say that he had once been able to prove to his own satisfaction that he could design a lug with a fatigue strength even worse than 4½ per cent of the ultimate strength He did not know whether R A E had yet succeeded, but they might do

He asked whether the Author could give the reference to the dangerous effect of pickling in cadmium plating

The **Author,** in reply, said he did not think anyone at Fairey's would describe the little tome which they had produced as an edifice

First, it was necessary to get the perspective right The reason for including everything possible in the lecture was that it was extremely difficult to try to write a general lecture bringing out particular points and approaches without having some decent theoretical background Since he has found it very difficult, if not virtually impossible, to find a consistent background anywhere in the existing literature, it was necessary to introduce the Appendices, which, although they might be considered as an edifice as written they still formed only a small part of the true theory, which, for the lecture, had been simplified to a " one-mass Blade " system

It was not for one moment suggested that the solutions which had been reached were final or were in any way the complete answer They were merely a step on the way to producing a basis for development

He had not yet given much consideration to the effects of damping on aerodynamic loading The next idea was to develop damping in the body, perhaps leaving out damping at the blade root, because the existing Fairey blades did not have blade dampers as such So far, therefore, he had rather skirted round the problem of damping It was simply a matter of time how one built up one's problem

As to the help which this had given in the solution of the real problem, he could give examples with regard to the Fairey Ultra-Light Calculations for the two blade rotor similar to slide, Fig 5A, were done assuming zero damping, and showed that at the first body frequency where there was virtually little damping in the system an unstable oscillating was expected at a frequency associated with the skid undercarriage During the testing a certain amount of structural damping occurred and although nothing catastrophic was anticipated, because the range was extremely narrow, the aircraft responded in a noticeable way It wobbled round quite safely in the vicinity of the predicted R P M

In the next degree of freedom there was still a calculated, narrow unstable band, below flight R P M , and since when going farther up the R P M range, these bands became more important, gave concern There was, however, considerable damping in the mode inasmuch as this oscillation was associated with an actual rubber spring Those undertaking the test work did not exactly put their hands on their hearts when approaching this band (since the calculated band was not really wide), as they considered that with the damping included, there was no reason to anticipate a dangerous instability This in fact was the case

Had there not been a rubber spring in that particular mode, the calculation would have given cause for concern and it might then have been necessary to think of putting in damping much earlier in the equations in order to see whether trouble would arise in that particular case

The experience with these curves was that, although they were preliminary, they tended to show up more systematically, conditions of instability, although they were not critical enough in saying exactly whether or not there would be a dangerous type of instability or an oscillation that was basically self-excited but at the same time damped

The Author suggested that Mr Shapiro should contact Dr Winny on his question concerning cadmium plating The report in question was at present a purely internal Fairey report

Dr H Roberts (*Fairey Aviation Co Ltd*) (*Founder Member*), said that some very learned statements had been made, some of them being too learned for him to follow On looking through the file of an unnamed Company, some years ago, to discover the length of time taken to do the performance on a particular helicopter he found to his horror that by working very hard, doing the job properly and making no mistakes, he could get an answer out in a month But the answer was required in a day or two, and not in a month's time He received the same impression when listening to the Author outlining his stressing methods

After listening to the Author, his feeling was that the aircraft industry was trying very hard indeed to make considerable increases in its technical overheads It was no use providing a method which would take a long time when quick answers were wanted He could not wait six months for the Author to come back with a nice answer accurate to 1 per cent Delays of this kind were fatal to a designer and of serious concern to anyone desperately trying to meet a target date

Speaking from the project designer viewpoint, he said that at the beginning of a project one was faced first with the selection of the size of the rotor In the past, this had been done somewhat tongue in cheek rather than on technical grounds, but lip service had been paid to the idea by saying that it was not structurally feasible to make a single rotor of more than a certain diameter This statement was based on accumulated experience which was now somewhat obsolete

On the basis of all the work he had done, did the Author have any views as to how big a rotor could now be made ? Was there still a restriction to 80 or 100 ft ? Could one go on indefinitely ? This was the question to which an answer was needed quickly without doing too much work at the project stage

Going one stage further, a diameter having been chosen, the next step was to choose a tip speed In the past, this had been done nicely by aerodynamic reasoning, but the Author now said that unless great care was exercised, an awful lot of resonance problems would be encountered

The designers wanted to know quickly what the limitations were As the forward speed increased, was it necessary to go into the compressibility region because the r p m could not be brought down by reason of resonance troubles ? This was another answer that was wanted quickly

One wanted to know the answer also concerning the magnitude of the offset of the hinge at the centre In the past, it had been selected on somewhat erroneous or at best doubtful stability grounds Could the Author say whether the stressing was severely affected by this parameter and whether, in fact, the hinge position should be chosen on purely structural grounds ? These were all-important questions which needed to be answered in a hurry, and of course in a general manner

The big question was whether it was really necessary to follow the Author's accurate though somewhat cumbersome long-term methods, or whether some short cuts could be taken If there were short cuts, where were they to be found ? In particular the speaker would be extremely interested to hear Mr Rogers' views on the three specific points mentioned

Mr Rogers replied that the subject of his lecture was "Problems associated with the Strength Assessment of Rotor Blades" and not methods for the assistance

https://doi.org/10.1017/S2753447200003437 Published online by Cambridge University Press

of basic designers, as they should have their own ideas on obtaining short-cut methods for the project design of helicopters The point which should be brought out was that although the theory might in itself appear somewhat cumbersome and complicated, it was not impossible to do short and speedy calculations by using it

Without wishing to advertise in any way, he pointed out that the Fairey Ultra-Light, which started life as a basic concept in one year, was completed and flown on approximately the same date the following year , and these same methods, in a simplified way, had been applied It was during that time, in fact, that some of these methods were developed

It did not follow that one had to take an extremely long time to use the technique in order to analyse the rotor system What did matter was the degree of approximation and the chance that one wanted to take in the early stages of design After all, this was typical of all problems

A point to remember was that one should not be asked to decide in five minutes on many different fundamental points which were controversial in helicopter design The basis of all design was that one built up a background on a type as well as using experience

In the fixed-wing field, it was possible to draw on quite a lot of experience, and because there was a lot of experience, one was able to take chances that would not normally be taken if the whole thing had to be done *ab initio*

All he had been suggesting this evening, continued the Author, was that where there was no great background of experience, one must fall back on some systematic and scientific way of choosing one's parameters The question of whether one decided to analyse them all at once and to take a long time, or whether one was prepared to take a chance, as in the design of any aircraft, and analyse a few of them to see that one was on the right path, was the job of the basic design office Experience in this sort of thing would be gained only by building a type and continuing to develop it No one could be expected one minute to design a baby helicopter and the next minute to be asked his opinion on a giant helicopter with a rotor double the size of the 'Rotodyne,' because the answer would be that he did not know If anybody wanted to know the answer to this, he should ask whether anybody else could suggest any other method

That was not to say that the methods considered tonight were the best, but, obviously, some method must be applied to determine these things One did not get the answer simply out of the blue They were questions which must be given thought

If a consistent theory was available, one would get the consistent answer that could then be followed into the more detailed design stage , but if when beginning to apply the more refined theory one fell into trouble, that was where the experience presented itself, in that one would have learned something and would not do it again

Mr I Chichester-Miles (*Blackburn and General Aircraft Ltd*), said he would like to ask questions concerning testing and about the effect of surface finish on fatigue Also about control forces As regards testing, this was, after all, the proof of the pudding Would the Author agree that the aim of testing should be substantiation of serviceability rather than destruction ? One of the great difficulties in the industry at the moment was the question of expense There were so many cases to be dealt with, on helicopter blades in particular He wondered if the Author would agree that, instead of a number of tests to destruction, a whole series of tests on one specimen, consisting of a variety of fatigue tests, repeated loading tests and static proof tests (in those cases where this was of importance) would fill the need Was, for instance, a ground ultimate case of destruction or an overspeed case to destruction necessary ?

Concerning surface finish and its effect on fatigue, had the position yet been reached in which laboratory tests with various surface finishes were sufficiently accurate for service conditions ? Was there not an advantage in trying to devise surfaces which were not so much efficient in the laboratory, as efficient in service insofar as they were insensitive to surface scratches and damage ? He had in mind some types of electrolytic and chemical methods which looked as if they might produce a type of roughness which, although giving a lower laboratory performance, produced a consistent figure in service even when scratched

His last question was about control forces It seemed that the failure of controls in flight due to fatigue was one of the major bugbears in helicopter design It might be that he was somewhat ignorant, but he had rather got the impression that there was very wide scope for further knowledge in the derivation of control forces Had the Author found that he could theoretically derive control forces with any degree of accuracy ? If so, where did they come from ?

The **Author** replied that here, there were two ideas, one concerning serviceability testing and the other fatigue testing Before coming to serviceability, one must establish safety For safety, one must obviously break specimens to establish scatter and to be confident that the measured stresses would never break the part

In the case of the rotor blade specimen that was troublesome from the fatigue point of view, there was not generally much point in doing ultimate tests on it It was quite possible that serviceability testing might be a necessary adjunct to fatigue testing, but one should not put the serviceability testing first The first step was to establish the strength of the basic aircraft, and any serviceability testing that was necessary could then follow on in the normal way This was, however, a problem which was rather divorced from the actual strength test in itself

While it was realised that conditions of service could affect the fatigue strength, much depended on what was meant by " basic fatigue strength tests " They were, after all, supposed to be representative of flight conditions, and the elaborations that could be resorted to in these tests was always a compromise between the difficulty of doing the test and the useful data that it could produce There was a very big " it depends " on this question

What Mr Chichester-Miles had said concerning surface finish was quite true This was one reason why the paper had tried to bring out the fact that it was possible to get improved fatigue allowables by means other than fine surface finishes After a fitter had spent many hours in producing a fine surface finish, it was disheartening to see the result when somebody dropped a spanner on it Although these things should not happen, the fact remained that they could happen , and, obviously, it was quite right that they should be brought to notice

The short answer, therefore, was that one must establish a finish that would not be too sensitive to local imperfections One possible means was by a more extensive use of the Capri-honing technique, by which one tended to toughen up the skin by bombarding it with, not little pieces of grit, but rather hard spherical balls This was the more logical approach in seeking a surface which could be relied upon for improved fatigue properties since it should be less prone to damage

The Author said, that the whole of the Fairey Company knew his view on control forces If asked how control forces were calculated, his reply would be that anybody else's guess was as good as his own It was very difficult to calculate control forces This did not mean, however, that one should not attempt to do so or could not do so

One could get any answer depending on the assumptions that were made To assume a naive approach, one could take a blade bending under its first and second harmonics of forcing load and obtain the associated flapping and lag plane deflections

One then had a set of loads which were offset from a Tangent drawn through the blade feathering hinge, and consequently it was possible to calculate the torque about this hinge Since the torque was obtained from the product of harmonically varying loads and displacements it was also of harmonic form

If it was said that allowance must be made for the effects of higher harmonics, then the magnitude of loads and displacements being considered should be increased It might then be asked what higher harmonic loads and deflection should be taken, because it was not possible to calculate these higher harmonic effects with any degree of accuracy If it was said, " We will just double up the forces and displacement "

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—he was talking about the calculated first and second harmonic terms—one could get another answer By such assumptions, it was possible to get a torque ten or fifteen times greater than that obtained by calculating it on the basis of simple first and second harmonic distribution only Since the size of the control rod was rather different if designed on one load and then on another fifteen times as big, it made one wonder what was being done

His general experience was that by using the assumption of doubled displacements and loads, there was a tendency to over-estimate the control load On the Gyrodyne, results had been measured of the order of 65—70 per cent of those calculated on the " doubled-up " distribution, which meant that they were seven times as great as those calculated on the more naive distribution, which was simply a straight first and second simple harmonic displacement with its associated load Unless great care was taken, it was easy to under-estimate control loads considerably and constantly to have trouble with them

The answer was to make sure that one was not too pessimistic and to put on strain gauges in the control circuits to find out quickly, exactly what the loads really were

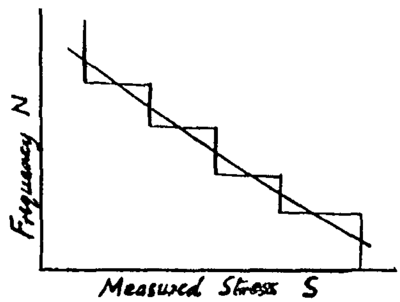
Mr A F Chalmers (*Fairey Aviation Co*), said he wished to enlarge on the point made by Mr Chichester-Miles concerning programme testing

The stressman on fixed-wing aircraft is in a very enviable position as he has a definite fatigue criterion of 1g $\pm$ 7½% of the ultimate loads, this being a typical gust case If this is applied to a component on test and 2 $\times$ 10^6 cycles is reached without failure everyone concerned is satisfied For the helicopter stressman however, there is no such criterion

If an analysis could be made of the strain gauge results from all helicopters which have flown, say on a national basis at R A E it should be possible to obtain a generalised stress frequency plot

Component testing would then be done on a programme form, replacing the stress spectrum by a step approximation

From such tests, it should be possible to obtain a far more reliable estimate of the safe life for the aircraft than by choosing a stress at a certain flight condition, testing at that stress, and basing flight clearance on the resulting life



The **Author,** in reply, said that knowing the work on which Mr Chalmers was engaged and having a good idea of what he was getting at, he would answer accordingly In passing, he said, it should not be thought that because so many questions had been asked by people from Fairey's, he had been briefed beforehand For the past fortnight he had been away from Fairey's and had been engaged upon the R Ae S Air Transport Course, and was only just beginning to project himself back into the helicopter atmosphere

In the case of the fixed wing aircraft, there was quite a lot of statistical evidence available to show what the fatigue criterion should be With helicopters, however, when assessing fatigue effects, there was no statistical background to say how often a semi-ultimate case occurred, how often a manoeuvre case occurred, and how often only normal flight cases occurred

To get a true life indication of the component, one should make some estimate of those other conditions that could occur in addition to what was normally accepted as the normal condition for life assessment It meant that a similar analysis must be done for helicopters if one wanted to make a realistic life assessment without going through the laborious process of finding all the order of stress levels which could be experienced

It was necessary to emphasize the fact that before the problem could begin to be tackled at all, there must be a lot more background data There were two massive tomes (A P 970, Vols 1 and 2) to assist the fixed wing stress man, but in rotary wing stressing there was simply a great big cover (A P 970, Vol 3) containing only three or four very thin pages It was to be hoped that the size of the cover was an indication that one day it would be filled up

Quite a lot more could be done on this serious topic to assist people who were concerned with stressing helicopters The stressman was reasonably intelligent and did not need to have the relevant parts of A P 970 translated into Helicopter terms He could quite easily do this for himself It certainly did not contain many things which he wanted advice on

The Author added that he always looked through all the amendments as they came out, but, so far he had not found anything which could not be obtained from Vols 1 and 2

The **Chairman** referred to Fig 6 in the paper, that showed higher harmonics present when the aircraft was hovering Presumably it could only be checked on a model without a body, but was the presence of the body inducing these harmonics in any way ?

On the general level of the higher harmonics, he remembered hearing M Petit (of Bretignz) in Paris, claiming that the French had had stress measurements in flight showing that the level of all harmonics above 3rd rotor was greater than the sum of all harmonics below 3rd rotor This was a worse state of affairs than people thought He could not remember whether that assertion was related to any particular helicopter

He was surprised that nobody had used the magic words " cumulative damage " in discussing fatigue Perhaps it was such a ' hot potato ' that nobody cared to pick it up !

The Chairman said there was another point which he would like people to think about He had been engaged on a little sum which had given an interesting answer Mention had been made of a desirable blade life of 1,000 hours This sounded like a nice round, large sum stretching out nearly into infinity It should, however, be remembered that utilisations by commercial aircraft easily reached—and helicopter transports must reach the same level if they were to be economic—what could be described as a very modest figure of 2,000 hours a year Some aircraft reached over 3,000 hours per annum Sabena, with their helicopters, were already running at about 1,500 hours on daylight contact operations

A blade retirement life of 1,000 hours with a 2,000 hours a year utilisation would mean two sets of blades per year The cost of blades, and particularly metal blades, which were rather expensive, worked out, on average, at about one-eighth of the cost of an aircraft This meant that an operator with even a relatively small fleet of, say, 16 aircraft would be buying each year the equivalent of four aircraft in blades

That little sum served to focus attention on the necessity to push up the blade life far beyond 1,000 hours , otherwise the element due to blade replacement became extremely expensive and was one of the reasons why operators tended to look askance at the operating costs of helicopters, of which this was merely one aspect

Mr Rogers, replying to the Chairman's reference to the Fairey Company's Wind Tunnel model blade, said that they had a four-bladed model rotor, which had been designed in the T O at quite short notice and which, apparently, was now doing a useful job in helping Dr Roberts with many of the aerodynamic problems which he had to solve in regard to the aircraft body

The model blade had been strain gauged to ensure that there would be no trouble with it, and measurements had shown that for the condition of zero cyclic pitch zero forward speed and no body at hovering R P M small oscillating stresses occurred

They were, however, of much lower order than the stresses which were obtained for the forward speed conditions It would have been useful to have obtained similar results for a two-bladed model rotor Perhaps the reason for the much reduced hovering oscillating stress level in this case was that they were dealing with a four-bladed rotor and there might be a greater tendency to uniformity in the induced velocity distribution than with a two blade rotor The full scale strain gauge results previously quoted were particularly associated with a two-bladed rotor, a rotor with which one could hardly imagine a uniform distribution of induced velocity

It should be noted however, that even with zero wind speed in a tunnel where the conditions were known, there was still an oscillatory stress in the hovering condition

It was not surprising that the question of utilisation had been raised One could imagine being the sole survivor of the helicopter fraternity, during the last two weeks, among people who operated Viscounts, D C 6's etc , that when they began to talk about utilisation, it made one feel that a mere 1,000 hours was not worth anything To an operator with a fleet of 20 aircraft, it was a week's flying It meant that one must take, not so many blades for so many aircraft, but that one must go along every week and put on one new blade on one aircraft It was a rather troublesome thought

In connection with fatigue certain estimates had to be made based on cumulative damage, because there was nothing else, but when these estimates were made nobody believed them Considerations of this sort always made blade life assessment very difficult

To overcome some of these difficulties it was necessary to aim for at least a 1,000 hours and for steel blades this implied infinite life, and if infinite life could be achieved, the 1,000 hours did not apply

The question of life must be interpreted carefully The finite life calculation was not really worth anything One must have infinite life, for the normal flight condition, for the manoeuvre condition and perhaps in semi ultimate conditions as well

Concerning the stresses which occurred above the endurance limit, someone must either do a lot of testing from the point of view of sequence testing, or apply cumulative damage It was no good looking at the problem and saying that it might break or it might not

The person who was asked " Can we fly the aircraft " must make up his mind and give a decision The stress man was sometimes asked to give design clearance on a rather delicate premise

The **Chairman,** in closing the meeting, said that the plea which had been made twice this evening and on many occasions in the past was usually directed at some unfortunate member of the Government Technical Service who might happen to be present All that they could say was that they wanted more research, more test work, more experiment and more theory The lack of it had been felt acutely over a number of years It was good to see that the situation was improving, however, and Mr Wood, of R A E , was giving a great deal of help It was a pity that there were not several more Mr Woods who could help people out

There were several areas of vital research and development where nothing at all was being done Anybody who required information had to turn to American reports and see what could be picked out of them, but they never quite covered what was needed All this was very disquieting

The Chairman said that he was closely associated with the Author and had always had a great deal of comfort from the close day-to-day contact with him in the very difficult arena of rotor stressing and rotor dynamics

With Mr Rogers in the team and with the class of work which he had described, not only the Fairey Company, but other companies also, with their own high-quality technical staff, must feel that when the detailed calculations had been done, according to the theories which had been explained tonight, at least they had a better understanding of the problems and this trouble might be beaten Knowing one's enemy was very important In this vitally important area, the Author's contribution tonight had been of great and lasting significance

The vote of thanks to the Author was accorded by acclamation