Discussion

Mr O L L Fitzwilliams (*Founder Member—Westland Aircraft Ltd*) I have been particularly interested in the second half of Mr STEWART's paper which deals all too briefly with a subject on which we have ourselves done a considerable amount of work, the first results of which will I hope shortly take the form of some experimental airborne ironmongery Unfortunately I am not authorised to speak in detail of our work but in skirting round the subject I hope to make a contribution of some interest by referring to some confirmatory flight test data from the distant past, by indulging in a small flight of fancy to illustrate a particular point, and finally by indicating the sort of helicopter which might well result in the relatively near future from progress along this particular line of investigation

When a novel theoretical prediction is made it is natural to expect a considerable time to elapse before confirmation is available from flight test data, but this is an exceptional case and I think you will find the curve shown in Fig "A" to be of considerable interest First of all I think you may be interested to notice the date of these experiments (1933) which were carried out with the Pitcairn PCA-2 Autogiro, and you will also notice that these curves represent the results of actual flight tests carried out in conditions in which the tip speed ratio was as high as 0 7 This Graph has always fascinated me because when it first came to my notice, in 1937 or 38—that is, about five years after it was published—it was still taken for granted in the design of Autogiros that successful operation could not be expected at tip speed ratios

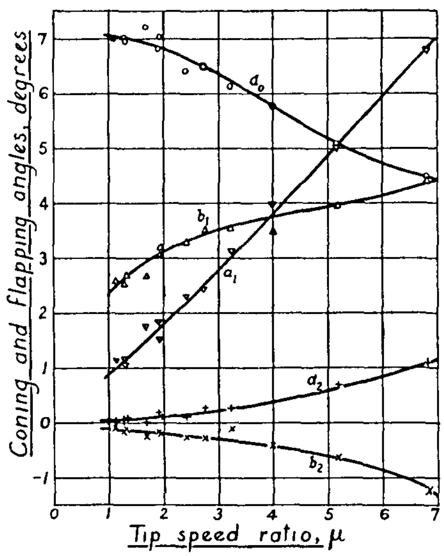

FIG A

CONING & FLAPPING ANGLES – GLIDING FLIGHT (P C A-2 AUTOGIRO)

N A C A REPORT No 475 1933

higher than 0 5, and the value of 0 7 actually achieved with the Pitcairn is more than double the tip speed ratio which most helicopter designers would dare to offer at the present time

Especially in view of this date, the Fig provides a most interesting confirmation of the results found by Mr STEWART In studying it from this point of view we can ignore the lines which slope upward toward the right, since these deal only with the tilt of the rotor, and the quantities which relate to the distribution of lift over the rotor disc are concerned only with the coning angle and with the Fourier coefficients for the second harmonic of flapping You will notice how the coning angle reduces with increasing speed because the PCA-2 Autogiro was fitted with a fixed wing which at high speeds supported approximately 40% of the weight of the aircraft You will also notice that over practically the whole of the speed range the Fourier coefficients for the second harmonic of flapping are numerically equal above and below the base line This constant relationship between the coefficients indicates a constant phase angle which confirms Mr STEWART's deduction that the phase angle for the second harmonic of flapping is substantially independent of the tip speed ratio

It can also be shown that the equality of the coefficients corresponds to a phase angle of $67\frac{1}{2}°$ and by reference to one of Mr STEWART'S charts, we find that this would correspond to a blade inertia number of the order of 13, which seems to be about right I do not know the actual blade inertia number for this case but it should be quite easy to establish as a check on Mr STEWART'S calculations, since the characteristics of the PCA-2 Autogiro were recorded by WHEATLEY with very great care in this series of N A C A Reports

Another of Mr STEWART'S charts indicates that the ratio of the second harmonic of pitch change angle to the resulting flapping angle decreases with increasing speed and this again is borne out by the flight test data recorded in Fig "A" which shows that in order to induce a given second harmonic of the flapping angle at low speeds a considerable amplitude of the second harmonic pitch change angle would be required, whereas at high speeds the same amplitude of the second harmonic of flapping may result from the automatic motions of the blades, without the necessity for a deliberate excitation

Although these flight test results have been available for nearly twenty years, their significance seems to have escaped the notice of workers in the helicopter field and it seems advisable to explain why it is possible to test results obtained on an Autogiro fitted with a fixed wing to be used as a check on the behaviour of helicopter rotors I must admit that phrases such as " the second harmonic of flapping " did not at first convey to my mind any significant physical picture, but there is actually nothing mysterious about these blade motions and their origin is really quite easy to understand For instance when a rotor is operating, as in this case, at a tip speed ratio of 0 7, this means that the point on the retreating blade which is instantaneously stationary is at 0 7 of the blade radius so that portions of the blade, outboard as far as the extreme tip and inboard over most of the working section of the blade, are operating at relative wind velocities which are so low that the blade is simply incapable (regardless of stalling) of developing sufficient lift to maintain its normal conical motion

It is this inability to produce sufficient lift which gives rise to the second harmonic of flapping and which causes the rotor blades to depart from their conical motion in such a way that the blade tips no longer describe a circle but travel along a path resembling the brim of a Stetson hat Since the setting up of this motion results primarily from the distribution of relative air speed, its occurrence has little to do with whether the rotor is fitted to an Autogiro, a Gyrodyne, or a Helicopter

Moreover by examining the nature of this motion it can be shown that the vibration which is felt by the occupants of a conventional helicopter in the conditions in which blade stalling sometimes occur, is not primarily due to the discontinuities associated with the stall itself but results primarily from the vibrational characteristics of the conventional two or three-blade rotor in the presence of the second harmonic of flapping, the setting up of which is the most significant consequence of the blade stall This means that in respect of vibration at high speeds it can be shown that Cierva made a very serious mistake, at about the time this N A C A Report was issued, in going from the four-blade to the three-blade rotor

Now I would like to quarrel with Mr STEWART'S statement that the kind of development he has in mind may add not more than 40 or 50 m p h to the speed of helicopters Although I cannot speak with full assurance on this point I feel that the possible advance along these lines will almost certainly be found to be very much greater and, if this proves to be the case, the discrepancy in our views may well be found to be due to the fact that Mr STEWART has possibly not made sufficient allowance for the radial component of the air flow over the blades

Although I cannot present a properly detailed argument in support of this impression, it may help as an example to consider the extreme case of a rotor, having my favourite tip speed of 550 ft /sec and operating at a tip speed ratio of 3 In this case the rotorcraft would have a forward speed Mach Number of 1 5 Also the tip of the advancing blade would be operating at a Mach Mumber of 2, while even the tip of the retreating blade would be moving forward through the air at the speed of sound To suit these conditions the blades would have the bi-convex Ackeret type of supersonic aerofoil section which is equally suitable for either direction of airflow If we now regard, in this example, the conditions in which the blades are operating at the ψ — 225° and ψ — 315° positions, we can see that whatever peculiar things may be happening to the blades, the large radial component of the airflow makes it extremely unlikely that these will include the stalling which Mr STEWART foresees as a limit to forward speed

I do not know whether a rotorcraft capable of operating in these conditions would be of any practical use but I do find that a rotor designed for these conditions has many extremely interesting and even attractive properties However, for the present, it may be sufficient to emphasise again that the behaviour of the rotor in respect of limiting forward speeds is not primarily dependent on whether the rotorcraft is a helicopter, a Gyrodyne or an Autogiro

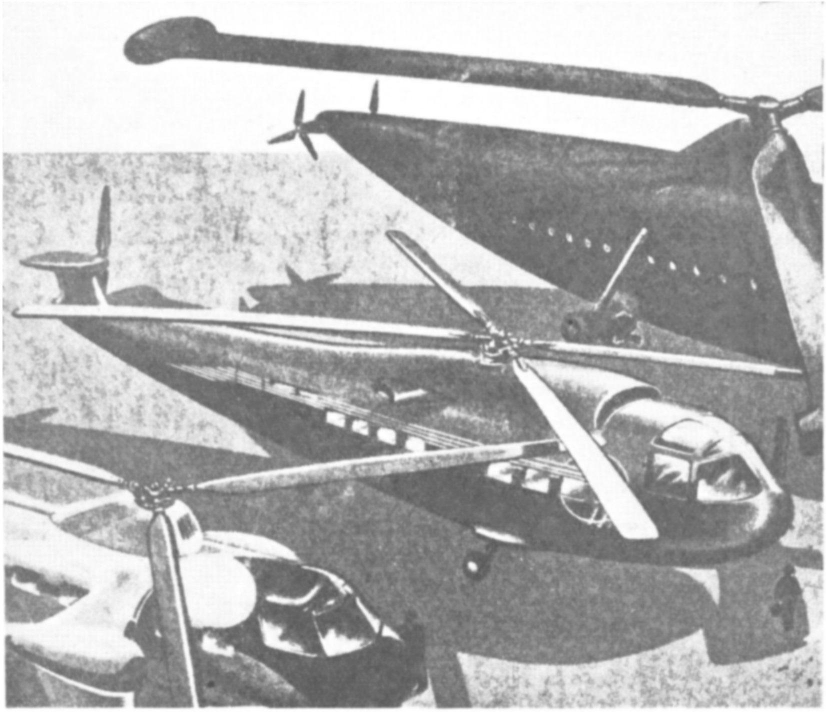

Fig B Westland W 81

One might expect, for instance, that such a rotorcraft might be a jet propelled Autogiro or a type of Gyrodyne deriving its forward propulsion largely from the exhaust thrust of a turbine engine, but it may be of interest to note that such a machine might also be a helicopter powered by ram jets at the blade tips, and that it might even be found possible to obtain a positive forward thrust from the ram jets on the retreating side of the rotor disc (rotation of the rotor would of course be maintained by the large difference in the thrust of the ram jets on the advancing and retreating sides)

Whatever system of propulsion might be adopted for such an extreme example it seems probable that a shaft driven helicopter, having what might be termed a medium forward speed of the order of 200 m p h , would probably benefit from a certain amount of forwardly directed airscrew or jet thrust as a means of avoiding an excessive forward inclination of the rotor at high speeds

As an indication of the sort of helicopter which may result in the near future from these various lines of development, you may be interested to consider Fig "B," which is a reproduction of part of a well-known Westland advertisement (this sketch has received wide publicity during the past year and has been clearly labelled with a cruising speed of 190 m p h so that it seemed to me rather curious that hardly anyone has asked how it works) Such a project would require two jet engines incorporating free power turbines but for the present the sketch has been based on the assumption that a suitable engine might become available having approximately the power characteristics of the Mamba or Dart I show the sketch here to illustrate how the introduction of a four-blade rotor, with second harmonic blade control, would match in an almost ideal manner with the exhaust thrust characteristics of the normal turbo-prop engine, to produce a fast helicopter which would be very attractive for inter-city transport

In a written submission Mr FITZWILLIAMS adds

I wish to take this opportunity of defending my use of the Stetson hat analogy since these hats have wide gently upswept brims, turned up more at the sides than fore and aft Actually this gives a very accurate representation, including the possibility of azimuthal adjustment of the hat to indicate phase displacement ! For example, the Pitcairn rotor at top speed was "coned" at 6 3° athwartships and at only 2 7° along the fore and aft axis of the "hat"

Also, where Mr STEWART, in his reply, refers to the low disc loading of the PCA-2 Autogiro as distinguishing it from modern helicopter practice, he should have referred to the blade loading The fact is that in order to retain a reasonable margin of manoeuvrability at high speeds when the lift is concentrated on the fore and aft quadrants of the rotor disc, it is necessary to provide a blade area which, though much less than would appear necessary on the assumption of conical blade motion, is nevertheless probably sufficient to take the rotor, without blade stalling, through the region of low tip speed ratios and into the region of high tip speed ratios where stalling is less critical, without the necessity to use the second harmonic of blade pitch change as a means of stall relief So far as one can see, the second harmonic of blade pitch change is actually required mainly for other reasons

Mr J S Shapiro (*Founder Member—Consultant*) I believe that the quasi-static theory gives a lateral lag in uniform circular motion, and therefore I do not know whether it is worth while going on to the new frequency response theory until they get a little further along the circle than the lecturer has shown I would also like to know whether the frequency response diagram is a precise circle on theoretical grounds, because I am under the impression that it is a near circle but not quite An exact circle would be very similar to the diagram of an electrical machine, an interesting analogy, first introduced by Kuessner, for a rotor which combined flapping and pitch change

I would like to ask, as a point of experimental technique, how was the tip path plane established in the wind tunnel experiments ? Was a photographic technique employed ?

Coming to the question of translational speed, of the four fundamental methods of delaying tip stall, which we assume is one of the limitations of speed, the lecturer has not mentioned the 'gyrodyne' principle , that principle does not, perhaps, get us very far but after all Mr Stewart's method has reduced the retreating tip incidence by only 2° If the power input into the rotor drops the flapping goes down and you get a lower incidence on the retreating tip

There are blade sections which allow a much greater increase in stalling incidence than 2° above that of symmetrical profiles The use of these sections would probably be a simpler way of achieving the same thing It is useful to recall the essential physical reason for the second harmonic of flapping It can be more easily explained if we equate flapping and feathering feathering alters the lift proportionally to the angle of feathering The increase of lift with velocity is quadratic It is this *quadratic* alteration of lift with velocity compensated by a *proportional* alteration of lift with pitch which causes the second harmonic of flapping We cannot look upon the second harmonic as a kind of secondary disturbance, it must be there because we have a square relation between lift and speed

The N A C A Reports which showed tests at very high tip speed ratios were taken into account in a lengthy but very lucid Report by Hohenemser on the question of stalled operation of a rotor It appears that we know so little about it that we should not make any hasty predictions It is quite possible that with proper isolation of rotors against vibrations we may not need large increases of power, and may not lose a lot of efficiency if we operate in the region of tip speed ratios above 0 5

Mr A H Yates (*Member—College of Aeronautics*) I am interested in Mr Stewart's suggestion of introducing second order pitch control When we put a flapping hinge on a blade the tip path is approximately a plane but there are second and higher order terms which mean that the tip path deviates slightly from a plane I have always understood that that, in itself, was one cause of vibration, and that if people could supply the second order pitch variation they would use it to suppress that deviation of the tip path from the plane I am not at all sure whether Mr Stewart's use of it to change the loading over the disc is in fact the same thing I quite understand that variation in the loading over the disc may reduce the vibration by reducing stalling, but it may make the other motion worse

With regard to the calculations of blade incidence over the disc, is it not right that as one comes inboard along the retreating blade the incidence gets less, and before the region of reverse flow is reached it actually becomes negative? It seems to be an oversimplification to think of the flow as coming to the aerofoil in either normal or reversed flow because on parts of the blade the flow is broadside on to the aerofoil

I would like to ask if, in the written paper, the author will make it clear what assumptions he made about the variation in the induced flow across the disc was it assumed uniform or calculated from his loading distributions?

Mr P E Q Shunker (*Fairey Aviation Co, Ltd*) With reference to Mr Stewart's remarks with regard to second harmonic control and considering oscillations in the azimuth plane if you sum up the aerodynamic forces on the rotor for n blades you will get the first n harmonics vanishing if all the blades are evenly spaced, for a 4-blader, for instance, if they are not at 90° with respect to one another, you are likely to get residual forces, which presumably affect the vibration Since it appears that even with second harmonic control, second and higher harmonics of flap increase with forward speed, azimuthal displacements which are considerably influenced by the Coriolus forces would also increase and so result in the above-mentioned residual forces Presumably these forces are contributory to the vibration experienced by helicopters at high forward speed

Dr G S Hislop (*Member—British European Airways*) I am delighted with Mr Stewart this evening because he has brought out into the open the clear possibility that higher speeds of helicopters are coming along, just as we had hoped they would In the application of helicopters to civil air transport a high cruising speed is important because it has a very important effect on earning capacity of the fleet Further, in order to be popular as an airline, punctuality is important and higher cruising speeds enable one to deal more effectively with adverse winds and yet still offer an attractive schedule Up to about three years ago the feeling that helicopters could ever cruise at a speed of say upwards of 150 m p h was not very great, but the last two or three years have seen a transformation in that picture and there is not only one idea of how that might be achieved, but several ideas In itself this state of affairs is one of the most promising features of present-day thinking

I am not going into the merits or demerits of the gyrodyne principle as compared with the use of the second harmonic of flapping to achieve higher speeds, but there is one point on the use of the second harmonic upon which I would like to have the author's views Is the so-called Wagner effect which is of importance in certain conditions of fixed wing flight ever likely to become of any importance when second harmonic flapping of rotary wings is considered?

Dr H Roberts (*Founder Member—Saunders-Roe, Ltd*) As a member of the old school, in which familiarity with vector methods is not always achieved, I should be glad if Mr Stewart would tell me in which way his new technique for determining blade response differs from the old method based on acceleration and higher derivatives I feel that we are doing the same job in a different way, and if that is true it seems probable that for most design purposes one could get a solution which is more amenable to simple treatment in so far as one could write down all the acceleration derivatives and immediately get out the answers

Something in my blood makes me believe that one never gets something for nothing I have heard of various ways of getting higher speed, by adding wings and other methods, and it would be interesting to know which of those methods is really going to win After all, a wing which carries something like forty per cent of the load is going to weigh quite a bit, and if one can simplify it and get something for nothing by having a fairly simple gear at the top it is not worth while playing around and wasting a couple of thousand pounds on a wing which is not really necessary

As far as the bi-harmonic variation is concerned, it seems there are two functions for which it can be used it can be used, as has been suggested, to push up the maximum forward speed but it also seems that it can be used for smoothing out vibration, which rather opens the door to the question as to whether one should think in terms not only of be-harmonics but multi-harmonics Has that been considered, and if so is there any future in it?

I was very interested in Mr FITZWILLIAM'S supersonic rotor, which is all right when he gets there but how does he get from the subsonic to the supersonic condition?

The Chairman When we were making rotor tests in the 24 ft tunnel at Farnborough we wanted to determine the actual load distribution over the disk, and tried to apply the pitot-traverse method, which has been used successfully to measure the thrust distribution on airscrews At a small disk incidence (about 15°) we found a loss of head behind the advancing blade and a gain of head behind the retreating blade Now we rely on the total head rise to pull the fuselage through the air, and hence the part played by the retreating blade is vital, if our tests show up a general feature of the flow through a rotor But at tip speed ratios of order 0 7 and above, the retreating blade will be almost ineffective Thus, before discussion of the high tip speed rotor goes further, more wind tunnel tests are required to clarify this matter

Mr W Stewart (*Member*) (in reply) I started with some general problems of helicopter research but I now seem to have a lot more I can only briefly reply to one or two particular comments, starting with Mr FITZWILLIAMS, who I know indulges in quite a lot of flights of fancy but this time I tend to agree with him because his ideas are not quite so fanciful but are a logical combination of a number of different methods His only flight of fancy was when he said that if we could get so much out of each method and add them all together we should get something It is a logical approach to the particular problem for the simple reason that the items which he mentioned were largely independent, in other words if we could have a higher tip speed ratio that would give an increase in speed quite independent of the second harmonic control

I am, of course, very familiar with the Report on the Pitcairn autogyro and every so often when I feel disappointed with helicopter performance I look at that report and think that I will find something wrong, but it is right enough and it actually did go at that speed There is nothing very strange in the actual results the autogyro, as it was then, had a very low disc loading—about forty per cent of the weight was carried on the wing at high speed, so that the disc loading apparently was actually ¾lb per square foot, and with the autogyro type of airflow flight was possible under those conditions and the results were extremely valuable, both in giving a check on flapping theory and for various other reasons, and it did in fact provide one of the earlier examples of flight evidence to back up the theory about which I have been talking

I might mention that the idea of second harmonic control was tried by Sikorsky for a very different reason he wanted to simulate forward flight conditions on the ground and thought of the second and higher harmonics as a means of pushing stresses into the blades to simulate what had been happening in high speed forward flight Some information was available in a paper by Jensen, which gave a description of the tower and some of the tests, and the example of their second harmonic control gave absolute agreement with the theory which I have talked about and did not produce any serious stresses on the blades

On the question of the second harmonic flapping occurring in any case it is quite true that a_2 and b_2 terms did occur in the tests, due to the forward speed, and these do re-distribute the loading to a certain extent and in an advantageous way—that is decrease it on the lateral sectors and increase it fore and aft—but unfortunately it does not go far enough in reducing the high incidence on the retreating side and we have to add a bit more to it

With reference to Mr SHAPIRO's questions I would like to point out that the particular example which I selected was rather a simple one and that is precisely why the locus fell into an ordinary semi-circle With a constant amplitude sinusoidal motion the locus was in fact a perfect semi-circle, and the tangent to the curve which gave the quasistatic theory was in fact 90° There is a lateral effect which can be calculated separately and we can also go on after that, and calculate what will happen if the oscillation is still sinusoidal but either increasing or decreasing It is also possible to take other cases which are not purely sinusoidal and to get another form of locus, which is exactly why I resorted to the vector diagram in the first place, in that vectors were the only way of doing it If you take an increasing oscillation which is not purely sinusoidal what you get is a spiral curve and a series of increasing or decreasing spirals according to whether the oscillations are increasing or decreasing, and the real use of the system is that it allows you to plot a form of locus for any type of initial displacement conditions In fact it comes back to a point made by Mr Roberts if 'a' represents displacement and 'b' represents the angular velocity then 'c' should represent the angular acceleration, so why should we not say that amount is an acceleration derivative and leave it at that? In some cases it may in fact correspond exactly with what may be an acceleration derivative but it is arising from a very different cause and as such it is better fundamentally to realise where it is coming from If it is just treated as an acceleration derivative which may be put into the equations and worked out simply it may in fact lead to erroneous conclusions

For the tip path plane and wind tunnel tests a cine camera was used The question of the natural frequency of the blade in operation is an extremely important point If you take an ordinary first harmonic control as we know it when the blades have shifted to their new plane you have no control forces left, but in this second harmonic control you must supply control forces to change the pitch by what you require The question then arises as to how much that may be, and taking the particular example of the ordinary types of helicopter in terms of moment of inertia, angular velocity, etc, it works out that the control moment coefficient of the blade corresponds to something like 01 That is fairly small but it would be a good thing to get rid of it, and if you do happen to get near the actual frequency it may keep going on its own, on the other hand you may build up a resonance which would be worse than what you started with

In reply to Mr YATES, when you take lambda you must make quite wide assumptions and it is assumed that lambda is always perpendicular to the mean tip path plane and is constant over the disc The question of assuming lambda constant largely affects what happens at the centre of the disc

Dr HISLOP mentioned the possible Wagner effect but I do not think we need worry about that here, for the simple reason that on the ordinary helicopter the lift distribution as the blade goes round is changing so much in any case that if it is present it does not seem to affect the ordinary helicopter, so it certainly should not affect this one In fact the incidence was much more evenly distributed with this form of control so it should make the Wagner effect less

As far as applying the general theory to other helicopters is concerned, the tandem helicopter does not have any real significance The question was quite fundamental in the ratio of the frequency of the imposed oscillation to the frequency of the rotor itself, and it is merely a case of evaluating for any type of application For example, I did discuss the long period oscillations of a helicoper It is possible to get a short period oscillation which is important but it is a fundamental problem of the rotor and has no relation to the configuration of the helicopter

I have in fact thought of multi-harmonic controls and have worked out the third harmonic control in addition to the second, but it does not really help very much You can go on adding the harmonics but I have found in fact that the third harmonic control does not give very much improvement on what we already have, so I do not think there will in fact be any real point in multi-harmonic controls for this particular application, though there may be a very good point for the reproduction of flight stresses in ground testing, and so on

Finally, there is the point raised by the Chairman, which is very important and I do not want to deal with it because it is extremely complicated and the more you think about it the more complications you find, and one could argue for a complete lecture on it It is a matter of distribution of forces and moments , we are pushing a certain amount of power into the rotor which must produce a given torque about the axis and produce forces along and at right angles to that particular axis, the one producing thrust and the other forward speed, but the whole point is that everything must be taken in The particular case mentioned by the Chairman is one example of a complete problem where you must take everything into account before you have a complete solution, and while I have tried this particular example I have not allowed for everything yet, and until I have done that I do not in fact know the answer myself

The Chairman I am sure we have all enjoyed Mr STEWART's talk very much it has raised a large number of important and interesting points and we are most grateful to him for preparing the lecture and presenting it (*Acclamation*)

CANCELLATION OF LECTURE ON 22ND FEBRUARY, 1952

The last moment cancellation of Captain Forsyth's paper on ' Helicopter Power Plants ' is very much regretted The lecturer was unable to complete the paper until a few days before the date on which it was to be read before the Association As a result, there was insufficient time to obtain the necessary authorisations for its publication from all the interested parties, although we were able to obtain its clearance by the Ministry of Supply and the Patents Office To obtain approval from all concerned for this class of paper takes time and is not always possible without considerable revision The Executive Council is well aware of the difficulties and has great sympathy for lecturers who are most likely to be under considerable pressure of other work Every endeavour is made to avoid such an occurrence and to this end future lecturers are particularly asked to have their papers completed at least one month before the date of the lecture

Fortunately, on this occasion it was possible to warn nearly everyone and for the few who came, mostly Council Members who had attended a meeting, there was a showing of films, time being too short to arrange a substitute lecture or discussion