Discussion

G. H. Tidbury (*Member*).

I should like to challenge the author's contention that it is preferable to carry out performance reduction at constant rotor speed and pressure altitude. This contention is not justified by the extra complication of his method over the equivalent altitude and r.p.m. method in view of the small change of r.p.m. involved in the latter.

The majority of methods of performance estimation depend on the most accurate available calculation of the following flight parameters, thrust coefficient, power coefficient, advance ratio, and climb ratio. Consequently the method of equivalent altitude and r.p.m. lends itself better to comparison with theoretical performance estimation.

When discussing vertical flight estimation, the author suggests that all estimated values are in error. I can assure him that this is not the case if a strip theory method assuming radial independence is used, with suitable estimation of profile drag at each section. I am convinced that only when we are able to operate helicopters safely with a truly vertical take-off and landing shall we be able to say that we really have a helicopter. For this reason, I welcome the investigation of the transition state especially in vertical flight. I believe the author has witnessed that the Air Horse has solved the problem as far as rate of near vertical climb is concerned. We are hoping that by the use of the long travel undercarriage combined with fairly low rate of vertical autorotation considering the disc loading used, the problem of vertical landing can be largely overcome even with the single engined machine, and fully with the proposed twin. It must be noted here that the transition to autorotation in the " Air Horse " is remarkably smooth under the best conditions although we have not tried the case of simulated engine failure.

Finally, it may be mentioned that air log results in autorotation on the Air Horse show the same P.E.C. as in level flight ; unfortunately climb figures are not available.

R. Lennox-Napier (*Member*)

I should like to raise a few points on instruments used in flight testing, arising from remarks made by the author in his very interesting paper.

First : *Airspeed indicators, generally.* Mr. O'Hara emphasises in no uncertain manner the unreliability of the orthodox pitot static indicator for much of the test work. I should like to hear his opinion on the possibilities of an instrument based on the principle of integrating accelerations, as this might overcome the difficulty, especially if constructed to work on air accelerations as opposed to absolute accelerations.

Second : *Low reading airspeed indicator.* The obvious suggestion here is to employ the rotating-cup anemometer, as used at meteorological stations. I am informed however, that this has been tried and found unsuitable. The hot-wire and the double venturi instruments are other possibilities.

Third : *Flight path indicator.* I think some work has been done at A.F.E.E. on a wind vane stuck out forward, clear of the downwash, with remote indication against a gyro horizontal datum. This scheme sounds very promising. Perhaps we could hear something of it.

Fourth : *Take off camera, type F.47.* Some brief details of this instrument would, I think, be interesting. In view of the fact that it costs nearly £1,500 I should like to know why the simple method used by our fixed-wing friends, of an ordinary cine camera with time marker photographing the take off through a calibrated grid framework, should not be applied to helicopter work.

Fifth and last : *Vibrographs.* It is agreed that there is a dearth of instruments capable of recording effectively the low frequency vibrations occurring in helicopter work. However, at Ciervas we have found it easy to construct a simple form of seismic vibrograph rather rudely referred to by our Chief Test Pilot as the ' clockwork mouse,' which although rather crude, has proved very useful in keeping track of 1st and 3rd rotor harmonic vibration amplitudes during development modifications on the Air Horse. Will Mr. O'Hara please give us the benefit of his experience on these points.

H. Roberts (*Founder Member*).

The first Member who spoke in this discussion gave the impression that the whole essence of helicopter flight was vertical motion. I think this is rather a restricted point of view and if carried too far may well have an adverse effect on the future development of rotary wing aircraft.

The power required for hovering is about twice the minium power in forward flight. Consequently if you set out with the point of view that you need enough power for hovering and the associated modes of vertical flight, then it seems to me that the machine is not being designed for the optimum conditions. If the product of the designer is to fly well and fly efficiently, its design must be based on operation under the optimum conditions.

It seems to me that this question of to hover or not to hover is the essential difference between the trends in this country and in America. In the U.S.A. the helicopter firms are designing for machines with larger disc loadings, and ignoring the need for vertical flight. Over here the question of safety is regarded as of over-riding importance so that vertical flight is a " must." As a result the prospects in the U.S.A. for rotary wing aircraft seem very rosy—over here they do not appear so good. We have got to decide in what way we are to treat the line of development of helicopters and how much we can allow this question of safety to interfere with progress. We must decide if we are interested in safety or if, on the other hand, we are interested in development and efficiency. It is in the achievement of a balance between these two aspects that the success of the helicopter depends.

J. S. Shapiro (*Founder Member*).

Doubts have been expressed on the need for helicopters to hover. From the point of view of getting the maximum payload off the ground a helicopter certainly does not have to hover, in fact it does not have to be a helicopter at all.

A helicopter taking off under optimum conditions for maximum take off weight at a take off speed of about 40—60 m.p.h. could be replaced by a fixed wing aircraft with a small gain in payload and cruising speed and a considerable gain in cost.

Now let us examine the effect of power loading and disc (or wing) loading on the two limits of the speed range : take off speed and maximum speed.

Increase in power loading widens the speed range ; increase in wing loading raises both take off speed and maximum speed.

However, increase in power loading reduces the payload, but increase in wing loading increases it.

An optimum wing loading and an optimum power loading exist but can only be determined rationally when a comprehensive criterion is given, such as minimum cost in commercial transport. In fact, I suspect that a comprehensive treatment of the problem would reveal two optimum combinations one resulting in a high take off speed, and the other requiring very low take off speed.

The fundamental relations are similar in both fixed wing and rotating wing aircraft but the extremes of maximum speed are denied to the helicopter as much as the extreme in slow take off (hovering) is unattainable with fixed wings.

In practice, the trend with fixed wings is towards higher maximum speeds. Take off speeds are disregarded, partly because airfields are provided for civil aviation at a fraction of their true cost.

It was my intention to emphasize that a choice is indeed available and that except for specific operational tasks the choice is likely to be a matter of opinion.

In my own view helicopters should be designed for vertical take off, for the following reasons :

(*a*) The cost and difficulty of siting of landing areas mounts very quickly with their size, more rapidly than the saving, due to increase in payload, would warrant.

(*b*) Safety and flying control are considerably improved, when helicopters can hover. A good proportion of past accidents have been due to under-powered helicopters.

(*c*) With present day disc loadings, a helicopter can hover automatically if it has enough power for adequate cruising speed. Substantial increases in disc loadings are inadvisable for a number of reasons, mainly autorotative descent and tip stall in forward flight.

(*d*) A twin engined helicopter which can maintain level speed on one engine for any length of time can automatically hover for brief periods.

(*e*) A helicopter designed to hover in air densities corresponding to moderate altitudes under temperate climatic conditions could still be operated with a take off run at very low air densities, where there are fewer restricted sites.

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(*f*) Helicopters have to fulfil tasks in which hovering is an essential part of the operation.

With reference to prevailing trends in the U.S.A. and Gt. Britain, it was in this country that the Autogiro was first developed in the belief that there was no virtue in hovering until the development of high lift devices made the Autogiro less outstanding than it appeared at first.

In the U.S.A. the " old " R.4 was considered underpowered and most of the latest machines are capable of hovering in free air. In fact, the best power loading of any type approved machine is possessed by the Sikorsky S.52. Present day helicopters and fixed wing aircraft operate in the same range of power loadings, namely 8—12 lb./H.P.

Even so, there is no need to be dogmatic once it comes to operation. In many cases, under special conditions overloads are possible and it is to be hoped that regulations will be flexible enough to take advantage of such possibilities through reduced limit load factors and favourable take off and landing techniques.

Nevertheless I remain convinced that the great bulk of helicopter operations will be performed under conditions where vertical take off and landing are the only real justification of the helicopter and the only real compensation for its slower speed and higher direct cost as compared with fixed wing aircraft.

The existence of dangerous states of flight in powered descent is by now a well known but still a puzzling phenomenon, in spite of the fact that it is usually associated with the so-called vortex ring state.

There is no evidence that dangerous conditions are encountered in all helicopters. My own feeling is that we witness a form of resonant response of blade flapping or flexing to some cyclically periodic mode of slipstream dissymmetry. In single rotor helicopters such an event could produce the symptoms of loss of control. In practice the severity depends very strongly on the type of machine. In the Air Horse a sharp increase of vibration intensity has been experienced, but no loss of control in any shape or form. My point is that, if the effects of steep powered descent depend on the characteristics of the machine as well as those of the airflow, it is futile to attempt a definition of the danger ' envelope ' in terms of flight parameters alone.

With regard to power-off descent I would like to emphasize that from the points of view of operation and safety it is not the attainment of an aerodynamically defined steady autorotation that matters but the attainment of the operationally defined steady rate of descent. Moreover, under most conditions, as shown by the lecturer and others, this rate of descent is reached rapidly but without overshooting. For these two reasons, the loss of height to steady autorotation is of academic interest only and no measure at all of the so called danger altitude.

H. B. Squire (*Member*).

I am pleased to know that Mr. O'Hara is considering the application of the maneouvre theory of Gates and Lyon to the study of helicopter stability. This theory is probably sufficiently general for it to be adaptable to include the helicopter as a special case. For example, the dynamical instability at the higher speeds observed, for example, on the Hoverfly, in the form of increasing oscillations, may reasonably be described as a consequence or an indication of a positive static margin and a negative manoeuvre margin. However, the special form of longitudinal control of the helicopter and the freedom of the blades to flap makes the relation between the trim curve and the static margin less direct than in the case of the aeroplane.

An interesting theoretical study of manoeuvrability with the title " On the pull-up of the helicopter " is given by HOHENEMSER in the German war-time report FB 1989/8. (I do not know if this has been translated). This investigation shows that stability with respect to angle of attack is more important than stability with respect to speed in reducing the acceleration due to small control movements.

Mr. O'Hara (*In reply to points raised in the Discussion*).

In reply to the query by Mr. Tidbury about the reasons for reduction at constant rotor speed, we are normally required in making type trials to determine the performance at given engine conditions which determine the rotor speed ; it is therefore more convenient to adopt the constant rotor speed condition in the reduction methods.

As to the remarks on vertical flight measurements it is not possible to make accurate estimates of helicopter performance at low speed using the simple momentum theory, but reasonably accurate estimates can in fact be made for vertical flight using the well-known curve of rotor coefficients, now established by analysis of flight data.

The remarks on the transitional performance of the Air Horse are interesting. So far we have not had the opportunity of investigating the transitional performance of multi-rotor helicopters, but hope that we may shortly get some practical experience on the Air Horse itself.

Mr. Lennox-Napier mentioned several possible methods of measuring low air speeds. Generally speaking I am in favour of development along any line which might possibly produce a satisfactory instrument for this purpose. The air log is the most satisfactory instrument we have at present for measuring low speed in test flights.

As for flight path to the horizontal, the instrument he mentions has been developed for use in connection with an investigation of instrument flying, but it has not reached a satisfactory form.

With reference to the use of the take-off camera we use the F.47 camera because one is available, but any other type could be used for this purpose in conjunction with a grid of the type suggested.

The discussion started by Mr. Roberts and taken up by Mr. Shapiro on the line of development to be followed to obtain economic helicopters raises a problem to which there is not a simple answer. It seems to me, however, that if helicopters are to operate economically in normal transport roles, less emphasis may have to be placed on the requirement for a vertical climb performance. On the other hand less economic operation will be tolerable in roles in which only the helicopter can operate.

With reference to the vortex ring state of operation mentioned by Mr. Shapiro, it does seem probable that the possibility of roughness of control and control disturbance is considerably less on multi-rotor machines than on single-rotor machines.

On the comment he makes with regard to the transition to steady autorotation, the only reason for considering steady descent as the final state is that we measure the performance from one steady state to another. So far as application to helicopter performance is concerned more importance may have to be given to an interim stage of the transition.

As Mr. Squire indicates, the static stability and manoeuvrability characteristics of a helicopter can be related to the dynamic stability in a way similar to that for fixed wing aircraft ; the theory has not yet been developed, however, in a form suitable for application in flight testing. From the point of view of flight testing it is necessary to relate the different aspects of stability and manoeuvrability to the pilot's impressions of the general handling characteristics of the helicopter. In this connection it may be noted that there are grounds for believing that the steady flying qualities of the helicopter are more affected by manoeuvrability characteristics, in the ordinary stability and control sense, than on a fixed wing aircraft.

It has been pointed out to me by Mr. Stewart that it is important to make clear that it is at constant speed that the rotor is unstable with respect to angle of attack. This instability is of significance therefore not in the field of static stability, but in that of manoeuvrability. For the conditions of static stability with the rotor thrust equal to the weight, the rotor is in fact approximately neutrally stable with respect to angle of attack.

Vote of Thanks by Mr. W. Gordon.

I am agreeably surprised to find out this afternoon how many are willing to forego the other attractions of such a splendid afternoon and hear Mr. O'Hara's lecture on the Flight Testing of Helicopters. It seems to me that all goes well with the future of the Society when so many are willing to come here today.

In my position at M.O.S. I know the work done at A.F.E.E.—apart from pure research—in the evaluation of particular helicopters. Our work commences when A.F.E.E. present their reports. I may say that this is a very important part of the work of A.F.E.E.

With regard to the remarks which have been made on instrument development, we have been considering this subject in M.O.S. in order to find what instruments could be developed for helicopters. I am thinking of those instruments which can be fitted to the normal helicopter and not those instruments required specifically for test work. From the remarks which have been made this afternoon, I think it might be a good plan to cast the net over the contractors and obtain useful suggestions from them.

Those of you who read this lecture in our journal will be able to read the part which Mr. O'Hara has left out in his talk, and you will see there the large amount of work which he has put into it.

I would like you to join with me in thanking Mr. O'Hara in the usual way.