Discussion

The **Chairman,** who congratulated the author on his excellent and concise presentation of a highly specialised subject, said that more wind tunnel testing of helicopter models might have been undertaken in the past but for the fact that many of the problems were related to near-zero speed, *i e*, to the near-hovering condition, in which there was considerable interference from the walls of the tunnel A similar interference near obstructions had been observed even in flight

Members might have heard of the so-called " Senger ' effect discovered by Mr Senger, the first Dutch pilot to learn to fly a Sikorsky helicopter When flying over long grass, he found that the machine sank into the grass and there was substantially no ground cushion The test pilot, to whom the incident was reported, had not previously experienced such a condition, but on making a test he, too, found that the machine sank into the grass and there was, in fact, little if any ground cushion What apparently was happening was that the grass was deflecting the flow which was thereby being recirculated through the rotor A vortex ring state was being established and the ground cushion was being lost

A similar effect tended to happen in the wind tunnel Not only was there the problem of folding up of the vortex sheet in translational flight as mentioned by the author, but there was the wall effect causing folding up of the flow itself, and the recirculation through the rotor probably accounted for some of the results that were obtained at low forward speeds

A paper* had recently been published concerning the wind tunnel testing of VTOL and STOL models in the United States by Mr J P Campbell, of the Free-Flight Tunnel at Langley Field, whose new techniques for testing such models were being developed The conventional force-testing techniques were appropriate for an accurate and quantitative analysis such as had been discussed this evening but the free-flight techniques were helpful in providing a qualitative investigation into the stability of helicopters This had been shown to be the case even in the early experiments of Arthur Young, who had made one of the first approaches towards stabilisation of the helicopter When he introduced his stabilising bar in the little model that the Chairman had seen demonstrated when visiting him at his office in Buffalo, in 1942, the model was controlled by electrical signals through an attached cable Mr Young had controlled it from his room out through the window and outside the window for a considerable distance, and then controlled it right back again through the window and landed it in his office It was a dramatic demonstration that stabilized flight with the helicopter was a possibility, and this free-flight technique was now being developed at Langley Field for the study of powered-lift systems

Several different techniques were employed In one of them, three pilots were used one to control the aircraft in roll, one for control in yaw and the other for controlling pitch With three separate pilots to control these several factors, the stability of the aircraft free from coupling effects could be investigated because each motion was controlled independently Another technique which apparently had been developed was the so-called control-line technique, in which the aircraft was flown round in circles at the end of a wire This type of experiment was very useful for investigating at least stability in the longitudinal sense

Did the author consider that free-flight tunnel testing of helicopter models should be undertaken ? Did he recommend that a similar procedure to that now being followed at Langley Field should be adopted in this country ?

The **Author** replied that if free flight techniques were to be adopted in this country, they would need to be done under such an amount of control—that was to say, avoiding extraneous draughts in big hangars, and so on—that they should be called by another name than free flight In other words, there was not likely to be very much progress with a free flight technique, at least not with small models In some first-hand experience of light airs in sailing yacht tests with a Bermuda rig twenty years ago, there were very nearly free air conditions, to an extent that it was necessary to log the weather on any day that testing was done and when making comparisons to ensure that the same kind of weather was chosen for following tests

The **Chairman** suggested that the tests could be conducted under controlled conditions in a large enclosure There would, in fact, be a free flight tunnel in which the effects of disturbances could be obviated or simulated as required In some respects such a tunnel would appear to be more appropriate for helicopter testing

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* NATO AGARD Report 61, August 1956

especially at near-zero speeds where wall effects in conventional tunnels tend to produce uncontrolled disturbances due to re-circulation of the flow through the rotor as is typical of the vortex ring state

The **Author** agreed Provided that one could take it to the extent of being able to measure and record at the time all the control movements and responses—which, presumably, was impossible, or at least not feasible, with a small scale model—one would not mind what the conditions were One might, in fact, then rather like a thunderstorm It was probably too much to hope for such favourable conditions within any reasonable time One must know the input of the control and also be able to know and measure the response from the control

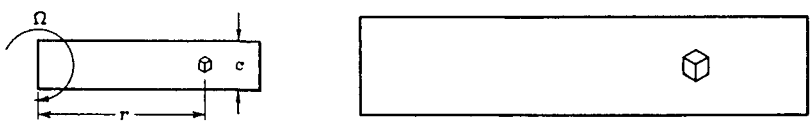
Dr R C Pankhurst *National Physical Laboratory*) said that the paper provided a valuable review of the growth of wind-tunnel tests for autogyros and helicopters He welcomed particularly the extensive bibliography, which afforded a useful starting point for pursuing this fascinating subject in greater detail Of particular interest was the author s account of his own experiments on the Rotodyne and the very encouraging results obtained The author and the whole of the Fairey Aviation Company deserved the greatest congratulation on their fine achievement

The paper was concerned primarily with the growth of wind-tunnel test results, but what was of special interest to Dr Pankhurst was the fundamental aspects of the subject, on which he wished to comment, especially the question of centrifugal effects on the airflow around the rotor blade He was not convinced that these were neglected, provided that one obtained full-scale Reynolds numbers in the tests The fact that the model was of reduced geometric scale did not mean that these centrifugal effects were more severe , he believed the centrifugal effects on the boundary layers were correctly reproduced, though he would not be too discouraged if somebody later maintained that he was wrong

He thought, in the first place, that it was a direct consequence of the principle of dynamic similarity that, provided the tip speed ratio was the same and the Reynolds number also was the same, the centrifugal effects in the boundary layers were correctly reproduced This seemed surprising, however, in view of the much greater radial accelerations in the blade tip region on the model, as pointed out in the paper

It was, therefore, worthwhile to look at the physics of the problem When considering the model of a full-scale blade, one could examine what was happening at radius r to an element of the boundary layer, and see what happened to the corresponding element of boundary layer on the full-scale blade The crux of the matter seemed to depend upon the ratio of the centrifugal forces on the elements of fluid to the inertial forces The centrifugal forces were proportional to $m\Omega^2r$ where m, the mass of the fluid element, was proportional to the elements of length in the chordwise and radial directions and, therefore, proportional to the chordwise and radial distances It was also proportional to boundary layer thickness (δ) at this point Hence the centrifugal force was proportional to $\Omega^2\rho cr^2\delta$

The inertial force corresponded to the aerodynamic loading on the blade and was therefore proportional to (*e g*) $C_L\rho cr(\Omega r)^2$ The ratio of centrifugal force to inertial force was therefore δ/rC_L, or δ/cC_L since by geometric similarity the radius was proportional to the chord



Model Full scale

Dr Pankhurst's argument was that in a model test at full-scale Reynolds number, one had the correct value C_L and the correct value of δ/c This meant that the ratio of the centrifugal to the inertial forces was correct In other words, testing at full-scale Reynolds number did ensure that the centrifugal effects on the boundary layer were faithfully reproduced on the model This was consistent with the general principle of dynamic similarity

In cases where the full-scale Reynolds number could not be reproduced, there was obviously a clear case for gaining more information on scale effect, particularly in circumstances when stalling of the retreating blade occurred It was well known that boundary-layer separation on aerofoils and wings could vary markedly with Reynolds number What was wanted in the helicopter case was a systematic determination of the requirements which had to be met to ensure that the rotor tests were well clear of critical Reynolds number regions

Dr Pankhurst's final point concerned wind-tunnel interference This was obviously closely related to the question of scale effect because it was looking at the problem the other way round the question now being asked was, what was the maximum ratio of rotor size to tunnel cross-section that could be regarded as acceptable from the viewpoint of wind-tunnel constraint ? The several instances mentioned in the paper in which lifting rotors had been reported to behave in much the same way as fixed wings lent support to the use of standard tunnel constraint corrections derived from lifting line or lifting surface theory , but this should at present be regarded as somewhat in the nature of a provisional measure, especially at low forward speeds

One wondered, incidentally, whether the open jet tunnel might have a part to play, particularly in tests near the hovering condition In any event, it should be remembered that relatively large interference corrections could be accepted if they were known sufficiently accurately One did not necessarily need to aim at the conditions in which the interference corrections were small , it might be preferable to accept rather larger corrections if they were known with correspondingly greater accuracy

An essential feature of the helicopter, however, as indeed of other VTOL and STOL devices, was the large vertical velocity components that were associated with the production of high lift at low forward speed These large vertical components might well necessitate a fresh approach to the tunnel constraint problem

The **Author**, in reply, said that on centrifugal effects Dr Pankhurst was right in that the crucial point was that the ratios of the vertical component and the horizontal component forces should be full-scale ratios on the model

The paper had stressed the point of reduced rotor diameter and lack of full-scale Reynolds number in some wind tunnel tests, whereas the other parameters were obtained at full-scale values , it was a case of " odd man out " That was the background to the point made in the paper that centrifugal effects on the airflow at full scale should be assumed to be practically the same as on the model unless full-scale model results were available If full-scale values could be obtained for all the parameters, there was no apparent need to worry about centrifugal effects in the boundary layer They were then looked after on the scale model, with the proviso that there was no cutting across any critical properties of the air In other words, if no question of stability of fluid motion arose as between model and full scale

The second point made by Dr Pankhurst that tests should be made with the rotor known to be clear of Reynolds critical effects on the blades was an important one It was really a reflection of the present state of knowledge of detailed flow around rotors Nobody in the business would claim that he had as yet anything like the information that was ideally necessary

Details of the flow pattern might involve time variant information as distinct from the more usual time average results When sufficient of this information was known, probably the second point made by Dr Pankhurst could be more readily met To be sure of being free of critical effects, however, usually one could only use such tests as the smoothness of the curves and the similarity of form with full scale—smoothness, that is, outside known trouble areas of a different kind For example, the very low tip speed ratio region, in which the curves had better not be called smooth, was a region which admittedly was not yet covered by any acceptable theory Nobody would claim that there was yet a sufficient theory for rotor lift between $\mu = 0$ and somewhere close to 0 1

The Author's main comment on Dr Pankhurst's point regarding constraint corrections was concerned with near-hovering conditions in which relatively high velocities were associated with the lift generated by a rotor The paper referred in one example to rotor disc loading at 2½ lb /sq ft Nowadays, at least 6½ lb /sq ft was of interest and presumably it was possible to go higher This involved the running of rotors under power loading conditions which emphasised constraint corrections Vertical velocities tended to be high in any case and they were getting higher

It was necessary to make sure that a false picture was not built up It was important to measure what was being obtained under given loading conditions and then to do further tests in which wind tunnel constraint was, if necessary, taken away from the rotor, and to compare the answers obtained

Mr R H Whitby (*B E A*) (*Member*) said that a major purpose of model testing by a company which was engaged in building a novel aeroplane and wanted to get it flying within a fairly short space of time was to obtain information that would make the first flights safe and free from catastrophe The type of model testing which the author had described was of great value in terms of basic information and in learning something about the aerodynamics of a new system compared with the fixed-wing aeroplane, about which a great deal had been learnt over the years, but it was difficult to understand what part it would play in actually getting the Rotodyne in the air and through its flight envelope without aerodynamic danger to the pilot

The type of wind tunnel testing which had been described naturally afforded useful performance data, provided it was ensured that Reynolds number and various other factors were correct It also provided derivatives with which a mathematical aerodynamicist could make response calculations and give information on how the aircraft would handle—provided that he did not leave out some important derivatives In calculations done at the R A E some time ago on the longitudinal response of helicopters, the results did not square up at all with the behaviour of the aircraft until a term which allowed for the effect of the body being in the downwash of the rotor was brought in After some adjustment the incorporation of this derivative gave an answer which better fitted what was actually found in flight test of the aircraft

Such considerations led to the view that in the case of a rather novel type of aircraft such as a helicopter of a new conception, there was a great deal to be said for a free flight tunnel Even if draughts and gusts could not be avoided in some free flight testing, the pilot in the aircraft would, after all, have to encounter them

Mention had already been made of three " pilots " being needed in the free flight model testing In fact the help of a fourth man to control the power was found necessary However, this did not invalidate the results of such a technique and this type of testing might be a very useful auxiliary for people who would have to put a novel aircraft in the air and get it flying throughout its flight range

With regard to peculiarities in the tunnel results at values of advance ratio below about 0 1, were these not due to the fact that the body was in the rotor downwash, whereas at higher advance ratios this was not the case

The **Author,** in reply, said that the purpose of model testing was to make desired measurements for quantitative information required Another question was whether sufficient information was obtained Actually the Rotodyne model testing included a much larger proportion than was perhaps suggested in the paper of tests which used the same techniques as fixed-wing testing, because an aim of the paper was to concentrate more on the aspects peculiar to rotary wing work It was fair to say that a large proportion of the design work could not be handled without wind tunnel data It therefore had a very real day-to-day practical purpose

Another point made by Mr Whitby was whether all the terms of the equation were in before calculation It was in large measure up to the tunnel to decide what was happening Presumably, sufficient was known of the theory to say that all the terms were or were not present, and sufficient of the theory was known to be able to say whether enought testing had been done in the tunnel to cover the various terms

Mr Whitby pointed out that in the tunnel, the testing was of static forces, whereas when the aircraft was in the air it was behaving in a dynamic fashion and some of the factors which might not have been checked in the tunnel came into the picture

The **Author** explained that he had misunderstood In a full dynamic case, of course, the tunnel usually could not represent it completely and the rest of the information had to be obtained from free flight That led to the next point concerning free flight Mr Whitby had mentioned a free flight tunnel, but the Chairman, surely, had been talking of free flight not in a tunnel

The **Chairman** reminded Mr Hooper that he had spoken of a free flight tunnel at Langley Field, as described in a paper published by J P Campbell

The **Author** replied that he had misunderstood the question He had been misled by concentrating on the " out of the window and back again " experiment In a free flight tunnel one could make the conditions smooth and controlled

Mr Whitby had emphasised the free flight tunnel If it was made sufficiently controlled, as was normally the case in a wind tunnel, there was something to be said for it, because in wind tunnel conditions, if the model could be kept virtually stationary and the air was passed along relative to it in the usual way, valuable photographic records would be obtainable It would, therefore, have some use, even if not a very deep one

Mr Whitby's point that in testing at tip speed ratios less than about 0 1, the body was in the downwash of the rotor was true, and it was the reason why the configuration of the particular model given as the example had been illustrated That was the clue to it, coupled in the ground effect case with the ground effect on the body as well as that of the rotor Presumably it came in also in the case of the long grass or the corn-field and in flight close to the waves of the sea This interference was much reduced at higher tip speed ratios Such results were clearly helpful at the design stage and could be important because of possible effects on control and stability

The **Chairman** then invited a contribution from Dr Hislop, who had conducted free flight tests with the jet-powered Gyrodyne This small-scale version of the Rotodyne established a considerable amount of data before the Rotodyne was flown

Dr G S Hislop (*Fairey Aviation Co Ltd*) (*Member*) said that it was a special pleasure to hear Mr Hooper outlining merely a fraction of the very extensive work which he had so ably conducted The original intention was that the wind tunnel work should be completed well before the aircraft flew, and it was hoped that the information from the tunnel would be available in plenty of time to provide all the required information so that the design could be tailored accordingly But most people here would appreciate what actually happens The decision to make a start on the aircraft is taken, there is terrific panic from the shops for drawings, everybody is really busy and the drawings begin to pour out and the aircraft begins to grow When this stage is reached the wind tunnel tests are only just beginning !

Mr Hooper had mentioned the two phases The straightforward aerodynamic testing of the fuselage and the tail surfaces had a direct and extremely valuable influence on the design of these components The tests with the rotor required rather more effort because the design of a 6 diameter rotor and drive system to run in a wind tunnel is not just a small item which can be drawn on the back on an envelope

Those who had been closely associated with the work would remember some of the more trying moments with the model in its early days In fact, the very first model had thrown a blade, and this occurrence had proved something of a setback

As the designs proceeded, the value of the model to influence the particular design as it was going through obviously diminished Its great advantage was that it gave warning and advance information of how the aircraft would behave

It was true that a free flight model piloted by a " committee " was a nice idea, but there were not such facilities in this country It would take a long time to develop this type of model and its associated techniques

The Chairman had mentioned that full-scale transition tests had already been done, from which it was known that in principle the transition problem of the Rotodyne was perfectly feasible There was no need to feel that before any flying whatever could be done it was necessary to go through the whole routine of transition, as people were doing, quite rightly, in tilt-wing models The advantage of a full-scale model was that it indicated broadly what the results would be

The more that one did in the model business, the more one learned and realised how much there was to know and how much more work there was to be done In addition to free flight, which would be useful, other kinds of models were needed even more urgently for rotorcraft One of these was a model that would give really reliable information on rotor blade flapping In tests where the flapping had been measured, the feeling was that the flapping angle as such did not match up too well with the full-scale Thought should be given to this , it might even require a research programme on its own

Another model should be used to give detailed aerodynamic data—*i e* pressure distribution around the rotating blades, and so on—to influence the fundamental design of the rotor system

A third model which gave the time variation of the downwash would also be of tremendous value, particularly in dealing with vibration problems Most helicopter manufacturers really appreciated how little they knew about the build-up of the wake from the rotor, especially at low speeds, and how much they would like to know to help the design Hence there was plenty of scope immediately for more model work along these lines and it would be interesting to know whether the Author had reached a similar conclusion

The question of the expected change of trim with forward speed at very low speed had been given very careful thought The author had mentioned that in the ground cushion the change of trim was of the opposite sign and of smaller magnitude Dr Hislop could not give the complete chapter and verse offhand, but he did not think that this apparent change of trim materialised in the full-scale aircraft to the same extent as might be expected from the model tests, because he had never heard the pilots comment about it Whatever change in trim took place was occurring so quickly and the inertia of the aircraft was such that the trim change did not get a chance to show itself and the ordinary amount of control movement was more than enough to handle it

The other value of a model of the type described by Mr Hooper which should be stressed was that its existence meant that the tools were at one's disposal to make an immediate investigation should a really serious problem show itself in flight To have nothing at all available and to be faced with a major aerodynamic problem when part of the way through a flight programme raised difficult questions of whether to press on with the work and attempt to solve it as one went along Having a model in being and knowing that it produced reliable results meant that the tools for investigation were available The flight programme could possibly be readjusted to allow the problem to be sidestepped whilst its actual investigation was proceeding systematically on model scale

One had seen the four pilots flying the free flight models and it was amazing how skilfully they seemed to manage The aircraft, however, performed some extraordinary manoeuvres The behaviour was put down to aerodynamic characteristics, but one wondered whether it could not in part be attributed to the four separate controllers This splitting of responsibility reminded Dr Hislop of an extraordinary set-up which he had run across just prior to the war When flying from Duxford, he came across a newly-installed Territorial searchlight battalion, full of zest to track raiders As an Anson was crossing the sky he asked the young officer in charge how they did the identification, which at the time was quite a problem The officer replied, " I have a wonderful system I have made Joe an expert on tailplanes, Bill is an expert on engines, Harry knows all about wing shapes and studies the ' sit ' of the aircraft They get together and tell me what it is ! " The committee meeting which decided on an aircraft type as it sped across the sky must have been very interesting !

The **Author** replied that sooner or later, somebody, somewhere, had to get more flapping and pressure distribution data on blades and the time variation of pressure on the blades Everybody wanted the information To get any further realistically—in other words, to make a worth-while step forward—all these classes of information had to be supplied He was sure that his own company would wish to do what they could towards it

The **Chairman** pointed out that at one time in the history of rotary-wing development the size of the model appeared to be very important, because at the Airborne Forces Experimental Establishment, Ringway, in experiments with rotachute models which were dropped from aircraft, the rotors would not simulate full-scale autorotative conditions if less than about 10 ft in diameter With a model of 6 ft in diameter, for example, much smaller blade angles had to be used to maintain autorotation It was not until a diameter of about 10 ft was reached that the full-scale values could be used

No doubt a critical factor was stalling over part of the rotor disk With the small models, a large part of the rotor stalled in autorotation, and this appeared to be an important factor in scale effect

Mr J S Shapiro (*Consulting Aeronautical Engineer*) (*Founder Member*), who greatly appreciated the Author's clarity of thought in the presentation of his paper, said that it was this clarity which made it so much easier to criticise the Author, with whom he had one or two points of difference

One thing had been brought out in essence although it was perhaps not called by a proper name The expression ' heuristic " was at one time used to denote a method by which one discovered something whilst the method itself was not of particular value as a method of measurement Model tests could play this part Arthur Young's model that went out of the window and came back again was heuristically superb, although it probably did not teach very much as a method of scientific investigation

Addressing himself particularly to the young people in the audience who might trust wind tunnels and models too much, Mr Shapiro said that there had been some woefully misleading results from model and wind tunnel measurements. This had been very apparent to him, because it was, in fact, his introduction to helicopters.

In the very earliest experimental helicopter—known as the W 9— produced by the Cierva Company, for example, the whole approach was based on wind tunnel results which later proved to be misleading. The measurements were not very precise. It was claimed that the hub mechanism would perform certain functions, and it did, in fact, perform them under the conditions of the wind tunnel, but they were irrelevant in free flight.

A much better documented story from Germany concerned a very worthy and most respectable and respected member of the profession, Herr Hohenemser, who produced a whole lot of evidence on so-called acceleration derivatives, which were later discovered to have been a particular feature of his model mechanism. Mr. Shapiro well remembered that he had lost a lot of the firm's time in chasing these accelearation derivatives.

He particularly emphasised the point made by Mr. Whitby that there were many questions about which the tunnel could not give direct answers simply because they were questions of flight dynamics. Dynamics were a succession of events which were determined by the derivatives, or else the derivatives were of no use. Hundreds of derivatives were theoretically possible and one had to decide which one to look for. No tunnel could show this unless the experiment was designed to do it.

Either the tunnels had not been particularly good in that direction or the tunnel technique and the model technique in tunnels had not yet progressed to the stage when it could be used very effectively as a means of direct and comprehensive measuring. The story was still very much in its beginning.

Designers had been in some ways, and had remained, at the mercy of tunnels. One of the phenomena, for instance, was the vortex ring state and the various other states which were discovered and measured in the tunnel but about which nothing had ever been done since. Certainly, there was a lot more to be done in the tunnel on the vortex ring state.

Another experimental helicopter with which Mr. Shapiro had been concerned was the Tandem. He was surprised that the Author had not mentioned the tests—they were done in the 24 ft. tunnel at Farnborough and had been reported—of the Tandem and three-rotor configurations with 4 ft. diameter rotors. From the designer's point of view, they were eminently successful. They had taught the designer something, they had given a definite Yes or No answer and they had showed both the superiority of one particular configuration over the other and certain specific difficulties of the Tandem configuration, as well as pointing the way to overcoming them. They had also shown up to those who later tested and analysed the theoretical aspects of the problem what to look for. These tests had certainly taught aerodynamicists quite a lot, including certain aspects of the distribution of inflow, particularly outside the rotor, which nobody had ever thought of before, and the variation of inflow velocity with the vertical co-ordinate. These two features, which had not been mentioned before, were clearly and unequivocally shown up in the tunnel tests.

From his personal knowledge and tunnel experience, Mr. Shapiro denied emphatically that tunnels gave any worth-while results concerning performance of pure helicopters. It could be calculated much more accurately than it could be obtained in the tunnel. But this did not apply to the Rotodyne, in which there was a completely new configuration and performance was greatly dependent upon the co-operation of the rotor flow and fixed-wing elements.

With the "pure" helicopter, to attempt to predict performance results from tunnels was a waste of time. Whether it must be so and must remain so was difficult to answer, but nobody had obtained, say, hovering performance with even a small fraction of the accuracy—or, one should say, a large multiple of the accuracy—that could be obtained by pure calculation. It was possible to work out the hovering performance of the helicopter within an accuracy of 1 per cent., but this could never be achieved in a tunnel, until it was a full-scale tunnel with a full-scale model.

There was one aspect which might be an important subject for tunnel investigation but in which Reynolds number was of decisive influence. It had not as yet been mentioned here, although it had been very well known that Reynolds number decisively affected the lift coefficient at stall, even if it did not affect sufficiently the drag coefficient. In that respect, there simply was no similarity between any model and the full-scale rotor. What was more, it was now increasingly being realised that com-

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pressibility stall on the retreating blade was becoming of major importance Here again, one could see no similarity between the model and the full-scale

Nevertheless, to return to the " heuristic " method of investigation, there was no doubt that the earliest tunnel experiments by Wheatley in America proved that it was possible to go to very much higher advance ratio values than anybody had thought before Under the influence of these tunnel results, which, in terms of quantitative measurements, yielded very little usable information but did show that it was possible to go further than was hitherto imagined, something was eventually done Thanks to the work of the Fairey Company, very much further progress had been made than was thought possible even three or four years ago

The **Author** replied that he was encouraged to note the number of points on which Mr Shapiro agreed that more wind tunnel information was needed, if only it was possible to use the right wind tunnels or make the right tests in them

The reply to the point about the work on tandem and multi-rotor tests which had been reported from the Farnborough 24 ft tunnel was that only a limited space was available in the paper and the tests had come after the technique of making such measurements, exemplified in the paper, had been established The paper was intended more to show the scope of wind tunnel work and how it had grown up than to present particular results

One was familiar with Reynolds number effects on lift and drag in fixed-wing technique , and, of course, the retreating blade added difficulties in the rotating wing Some of the tunnels and models in the past were small, but it would be fairer to say that some other tunnels could give a quite good estimate of performance

Mr Shapiro had said that he had never known of performance data for the hovering condition from a tunnel being realistic When the hovering measurements were taken on the Rotodyne model s 6 ft diameter rotor, however, lift was compared with theory and approximated closely The results did not come quite as close elsewhere in the tip speed ratio range, but at hovering the comparison was really good

The comments concerning the Reynolds number effects elsewhere were yet another emphasis on the need for more detailed information from elaborate tunnel tests , and if any substantial advance was to be made, the problem must be faced There were laboratory artifices by which Reynolds numbers could to some extent be faked, but apparently this had not yet been done extensively on rotor blades It remained another interesting example of the need to do more work

Mr P V Hoare (*Westland Aircraft Ltd*) (*Member*) said that he wished to speak on one or two practical points concerning wind tunnel work He was operating a 4 ft wind tunnel in the very early days and found there were many difficulties in obtaining the required information owing mainly to the fact that all the variables were not known, and plans had consequently not been made for their measurement He could, therefore, support the remarks which had been made tonight about the need for feeding all the variables into the tunnel if a comprehensive answer is to be obtained

As an example, he remembered a case where it was required to design a bomb that would explode before hitting the ground, and to do this it was decided to place a small bomb inside the nose of the large one and connect the two by wire The drag of both bombs and of the combination was carefully measured in the wind tunnel and adjusted so that the difference in drag would tighten the connecting wire

The bombs were put on an aeroplane and the combination was dropped The small bomb preceded the large bomb for a short distance and was then seen to fall back, and in the finish the large bomb reached the ground first In the wind tunnel work the drag of the wire had been forgotten

A practical point of interest was to know the material of which the models were made and the order of accuracy of construction , if wood or timber was used, was there a difficulty in preserving the surface finish and the accuracy of measurement after a model had been in use for say six months ?

In the case of the Rotodyne, what was the effect of the downwash from the main rotor on the behaviour of the stub wings, and had any measurements of the down loads on these wings been made in hovering flight ? It would also be of interest to know the effect of the downwash of the main rotor on the performance of the propellers

The **Author** replied that the material for what might be called the aircraft model —the fuselage, wings, tailplane, control surfaces, and so on—was good-quality mahogany, well seasoned and laminated Concerning accuracy, the usual limits for models of that size were used—*i e*, metal templates for any important profile parts, such as aerofoil sections The manufacturing limits were that daylight could not be seen under the template, giving the order of 1/100 in The effective limit was about 1/64 in in timber

The rotor blades had a steel leading edge portion and the trailing portion of the blade section was in Tufnol material, cemented together The accuracy was to usual tool-room accuracy, where necessary On the steel portion of the blade a chief requirement was avoiding scratches There was plenty of material left for strength as to average stresses

There were no special difficulties in making the model The parts had, of course, to be designed and well thought out and any metal parts of the rotor of special interest were made in the tool-room

The effect of rotor downwash on the wings was always measurable in the tests Most of the figures that would be published in the paper, corresponding with the slides which had been shown, gave the effect of the rotor on the model What in fact had been shown was the effect of interference, which hitherto had not been readily available from experiments Corresponding measurements were made with the rotor removed in a complete set of tests

Nothing was shown in the paper concerning the effect on the propellers, and it was difficult to give summarised information off-hand, but, as would be appreciated, there was a readily measurable effect at low forward speeds

Mr Hoare asked whether, if a model was tested and then put away for several months and a repeat test subsequently made, there was any certainty that the shape of the model was exactly the same as it was before

Secondly, did the interference between the main rotor and the body occur at zero forward speed or did it apply to any forward speed ?

The **Author** replied that laminating techniques had been developed over a number of years and the size of the lamination varied according to the thickness of the part, the laminations becoming narrower towards the trailing edge of an aerofoil section , and in using good quality mahogany for the timber parts it was found that they did not significantly change in shape provided they were laminated The method of laying laminations was part of the art of the model shop, and no serious trouble had been experienced

When is was wished to check the shape after an interval of time, the model went on the surface table and a check was made, but only rarely was any patch-work necessary

Interference from the rotor on the rest of the model Rotodyne was shown in the results up to a tip speed ratio of 0 53, the highest value used in these tests It was most prominent at values of the ratio between 0 and about 0 1 and then rapidly decreased

A vote of thanks to the Author for his excellent paper, proposed by the Chairman, was accorded by acclamation and the meeting then ended

Written Contribution

From **Mr J R Marsh** (*Department of Supply, Melbourne, Australia*) (*Member*)

I have read Mr Hooper's paper with considerable interest and whilst congratulating him on his excellent summary of wind tunnel testing of helicopters and its uses I would like to submit the following comments

Mr Hooper has mentioned several times the difficulty of constructing model blades synamically similar to full scale At the Aeronautical Research Lzboratories dynamically similar blades were made for the main model as described in Ref 9 above This caused no difficulty as the design for the blades had already been done for the full size rotor The model blades were made from wood materials cut from the same batches as used for the prototype and, using the full scale forward and tip speeds ensures the satisfaction of Eq 2 except for the Reynolds number and gravity force parameters The results are, of course, only strictly valid for a density altitude equivalent to that of the 9 × 7 atmospheric tunnel With gravity effects being negligible it has been found, curiously enough, that Reynolds number effects on the torque coefficients of the one seventh scale model are also insignificant Model blade stresses and strains are equal to the design full scale values with the added safety factor of the tunnel tests being limited to a rotor loading range of zero to a little over one "g' whereas the prototype blades were designed for a range of —1 to —3½ "g"

It has been found absolutely necessary to keep the model blades free of collected dirt and oil Runs over a period of one hour showed an increase in torque coefficient of twelve per cent due to collected matter

One aspect of the usefulness of the tunnel not mentioned by Mr Hooper is with reference to rotor head drag A radical change in rotor head design was brought about for the model being tested at A R L by drag tests very early in the design

Author's reply to Mr Marsh

The author thanks Mr Marsh for his written contribution His information based on practical experience is specially interesting and valuable In the paper, rotating rotor head drag was not separately mentioned as it had been fairly fully referred to in Mr Marsh's recent paper (*cf* reference 9)

"Investigations of Ground Resonance in Helicopters with an Analogue Computer"

by H Fuchs, B Sc, Ph D

With reference to the above paper which was published in Vol 11, No 5, the October, 1957, issue of the Journal, the Author has requested us to quote the following additional reference —

Saunders-Roe Report No SR (H) A/22 Helicopter Ground Vibrations (Interim Report), by A L Buchan