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SOME REMARKS ON DYNAMIC AEROELASTIC MODEL TESTS IN CRYOGENIC WIND TUNNELS

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I should like first to give you the background of this talk. Bob Kilgore came to see me at Bedford last June and we discussed some problems of cryogenic tunnels. I said, "I'm very interested in the feature of the cryogenic tunnel that enables you potentially to sort out the effects of static aeroelastic distortion and Reynolds Number." (The cryogenic concept is unique in this respect.) But my question was, "Really, can you take advantage of it?" and Bob said, "Ah, what a splendid topic for discussion." and so here I am!

Well, the first thing to say is that the principle of the cryogenic operation, this variation of Reynolds number and the variation of static aeroelastic distortion, is unique and that is something which, from the structural point of view, we are very interested in and we would like to capitalize on. And wearing a structures department hat, I think it's legitimate for me to certainly draw attention to this potential advantage in a way that perhaps I couldn't do if I were just wearing an aerodynamics department hat.

This is particularly so since Moss of RAE Farnborough gave a paper at Modane in June which showed just how important the effects of static aeroelastic distortion are. He made two wind-tunnel models. One which was what we would call an ordinary wind-tunnel model which is the sort of thing most of us are familiar with, just solid steel. And then he tested the thing with the same geometry, but with a cunningly constructed wing so that he was able to represent the right static bending and the right static torsion. He tested these wings at the same q, the same kinetic pressure, and the same Reynolds number and was able to get a measure of the effects of static aeroelastic distortion which was very large and very serious even on things like C_{L_α}, the variation of C_m with C_L, and actually had profound effects on the buffeting measurements. But that's a side of the story that I think we must leave for a smaller group.

So these effects are real and they're serious! I would like to say in passing, that although on one of my slides I use information from one of Bob's graphs, which assumed a total pressure of 8.8 bars in the NTF, I wonder whether it's really possible to make models to withstand that kind of pressure. Certainly the LaWs group, people like Phillip Pugh and Cyril Taylor, did look into this question very carefully and I think they came up with the answer that with the best steel that you had and a solid steel model you really couldn't manage with pressure much above 6 atmospheres.

If you write down the equations of cryogenics and how you get your Reynolds number variation, you see that the cryogenic principle really does allow you to vary Reynolds number independent of aeroelastic distortion.*

Now, we are very concerned with some of the problems of instrumentation that might affect dynamic measurements and in particular buffet measurements. Also questions which would affect the design of models. It's well known that lowering the temperature of steel or aluminum alloys generally produces a small increase in Young's modulus. Let's neglect this increase. There's a significant increase in the proof stress and the ultimate tensile stress, but unfortunately these favorable changes are generally accompanied by severe reduction in the ductility, the Izod impact value and the fatigue life. Bob has got some very interesting and typical curves there if we want to pursue this. But, looking at a

*See, for example, NASA TN D-7762, "The Cryogenic Wind-Tunnel Concept for High Reynolds Number Testing," by Robert A. Kilgore, et. al., Nov. 1974.

relatively old survey that I found in RAE, I've concluded that it should be possible to find materials that will maintain adequate ductility and Izod values down at these temperatures. It has been suggested that the balances that will be required will produce temperature drifts and thermal strain in the model, the balance, and the sting. Well, I'm fairly hopeful that these problems, although they exist, will be solved. And more so since talking to Bob this morning about progress in the pilot tunnel.

Mean static pressure measurements on the model are needed even in the structures department for Richardson's and Garner's method for predicting the pressures associated with oscillating wings. We see no problems, really, measuring mean pressures remotely because these can be measured at room conditions. But I think the fluctuating static pressure measurements will be much more difficult. I don't know of anything equivalent to a Kulite* pressure transducer that you can take and operate at a 100 kelvin. May be there is such a small transducer around, Bob, but we don't know and we would be very interested. It's an important point because it has been suggested that you can use fluctuating pressure measurements to actually predict the excitation provided by separated flows, actually to find the forcing function.

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*The use of trade names does not constitute an endorsement of any manufacturer's products.

Now I suspect that at this moment you can't do that and you couldn't use this mode of predicting the buffet at cryogenic temperatures. That being so, I think that's a justification for my looking at a rather old-fashioned way of doing it in a few moments. So we would be very interested, Bob, to know about miniature pressure transducers and miniature accelerometers that can really operate at cryogenic temperatures to measure the model dynamic response over the full temperature range of the tunnel.

For the measurement of wing buffeting at cryogenic temperatures I thought that I would have to start with something reasonably simple. The problem that I considered is rather an over-simplified one. In this particular case, we are going to restrict the problem (unwisely perhaps in the light of one or two things we know of) to fundamental wing bending. So this is a nice simple mode. And we're going to look at it either with a wing-tip accelerometer or by measuring unsteady wing-root strain which I suspect on a half model would be much easier. (You could probably do this now, in your pilot facility.) This technique is so useful in conventional transonic wind tunnels that it's interesting to ask if it could be applied in a cryogenic tunnel. We'll admit the importance of higher bending modes to slender-wing aircraft and the importance of torsional modes for some swept-wing aircraft.

For simplicity, let's just imagine that we're considering an unswept cantilever wing of constant chord and constant thickness. And then, of course, the frequency of that wing is the square root of Young's modulus times the moment of inertia of the section and divided by the mass per unit span. Well, let's suppose that we've been lucky and that at ambient temperatures, 300 kelvin, with low Reynolds number, we've actually got the right frequency parameter on the wing. (The frequency parameter is $f\bar{c}/U$.)

Let's assume that it happens to be the same as on the aircraft at the altitude and Mach number of interest. This is an assumption, of course, it's just to make life easier for us. In real life, one is normally really quite close on an ordinary wind-tunnel model at room temperatures. You're generally in the right ball park, but plus and minus 30 percent would cover it. Let's assume it's right. And we can then derive the buffet excitation parameter,... I think perhaps we'd better have the first Vu-graph now, please (Fig. 1).

There's rather a lot on this. From the equation we can derive what we call the buffet excitation parameter from the measurements of the model response. You'll see on the left-hand side of the equation a term $\sqrt{nG(n)}$. That is the spectral function for the generalized force in the first wing-bending mode. Then there is a factor of 2 over $\sqrt{\pi}$ which comes in from the arithmetic and then we have a term in brackets which is nondimensional. m_1 is the generalized mass in the first wing-bending mode with respect to the tip, $\ddot{y}$ is the tip acceleration and that's divided by q times S. q is the kinetic pressure. S is the wing area. And that square bracket is all multiplied by $\zeta^{1/2}$ and ζ is the total damping (% critical).

Now generalized mass we can either measure very easily or, since we've been so lazy and chosen a flat plate of constant thickness, we can estimate it and get it very accurately. $\ddot{y}$ we can measure from our wing-tip accelerometer or we can infer it from our wing-root strain gages. q we ought to know. S, the wing area, we ought to know. ζ, the total damping, we can derive from our spectra. We can derive this from the spectra either by autocorrelation techniques, but that's rather old hat now, people don't like that method, so let's use something more sophisticated, like the random DEC process, which one of my colleagues, Geof Butler, has got working at Bedford. And so you can get the total damping on this wind-tunnel model.

We've got what we had set out to achieve, the buffet excitation parameter, $\sqrt{nG(n)}$. (This is precisely analogous to $\sqrt{nF(n)}$ introduced yesterday,* the definitions are exactly comparable.) So we're at the design condition at the right frequency parameter with a low Reynolds number. If we reduce the total temperature to increase the Reynolds number, our frequency parameter goes up because the velocity of sound goes down. (That's ignoring the variation of Young's modulus.) We can now repeat the experiment at high Reynolds number. m_1 is, of course, unchanged. $\ddot{y}$ will be different. q is unchanged: I'm assuming we're electing to operate at constant q. The total damping will be different, but it will be different in an interesting way, as you will see in a moment. But the important thing is that the frequency parameter has now changed.

This is sad, because we're deriving $\sqrt{nG(n)}$, and what we want is a variation of that buffet excitation parameter with Reynolds number. Unfortunately, we're not going to see that because we are combining it with a variation in frequency parameter. It's rather difficult to quantify what that sort of change in frequency parameter will produce in typical excitation spectra, but certainly you could be off by as much as plus or minus 30 percent and that is really rather serious. Hence if we are aiming for precision in our measurements, it is unacceptable. The obvious way of getting over this difficulty is to say, well, let's regard this basic model as a light model. Let us pretend its made out of aluminum alloy and let us make it with some holes in. Having got these two measurements we'll stop

*See NASA CR-2722, "Some Remarks on the Design of Transonic Tunnels With Low Levels of Flow Unsteadiness." Lecture given at the NASA Langley Research Center on September 15, 1976.

the tunnel and we'll put some lead weights in. That will reduce the frequency and bring the frequency parameter down. We will now have two lower frequency parameter damping conditions when we test the model again. We will now have the right frequency parameter when we test at low temperature.

If you do that, you now find that your tests at the two temperatures on each model give you this kind of picture. You've got buffet excitation parameter here, at the correct frequency parameter, and you have the two Reynolds numbers, 133×10^6 which is a very respectable Reynolds number and also 27×10^6. So reading the buffet excitation parameter that way you can truly see the effect of Reynolds number on your buffeting measurements. And then looking at the curve in this light you can see the effect of the frequency parameter. You can get some feel for the sensitivity of the buffet excitation parameter to the change in frequency parameter. It turns out that if you have an aluminum wing and if you were exactly right there, you only need to fill in something like 35 percent of the chord in lead weights to be able to reduce the frequency parameter sufficiently to correct it again, to get the right value at the lower temperature. And this is a very interesting result.

Now, that takes care of frequency parameter. What happens to aerodynamic damping? A problem of using this technique on ordinary wind-tunnel models is that the structural damping is generally much too high in proportion to the total damping measurements. Structural damping is typically 1 or 2 percent of the critical damping where the total damping you measure(aerodynamic plus structural) is, say 3 percent of the critical damping. So that the aerodynamic damping on your ordinary wind-tunnel model is very small indeed, at ambient conditions. And you don't really know what the aerodynamic damping is on the aeroplane, even for this fundamental mode, because it's difficult to predict. There's a lot of evidence that funny things happen to the aerodynamic damping when the flow has separated. So you would really like to be able to scale the aerodynamic damping from the model to the aircraft. But you can't do that because it's an ill-conditioned sum. Your total damping ζ is quite small or is of the same order as the total structural damping that you have. Therefore, you've got to take an ill-conditioned number, multiply that by a large number, which is typically 10 or 20 for an ordinary wind-tunnel model, to get the corresponding aerodynamic damping in flight. This is rather a serious limitation of this technique.

But now suppose you say, "Well, yes, but we're not interested in what happens at the low Reynolds number case, we're interested in what happens at high Reynolds number." That's the lower temperature. And then you ask yourself, "What happens to aerodynamic damping?" To quote Bob Kilgore, Old dame nature works in favor of cryogenic temperatures. Unquote. Aerodynamic damping must go up, it's proportional to $\frac{1}{\sqrt{T}}$. Therefore, the aerodynamic damping which is typically one per cent at ambient temperature on a conventional wind-tunnel model, by the time you're down to 100 kelvin it will be up to 2 percent and it will be a much larger percentage of the total damping, now say 4 percent, as long as nothing funny happens to structural damping. This is an important proviso because I don't think anyone really knows what the structural damping will do. We do know the internal structural damping is very small on materials and on these typical wind-tunnel models most of the damping is at the joints. But it's certainly a very remarkable and interesting bonus that the aerodynamic damping, the thing that you really want to measure, is going to go up. That's what you need to extrapolate to flight and it's going to be bigger at cryogenic temperatures.

This has a very useful side effect. Let's assume that there are no Reynolds numbers or frequency effects on this buffet excitation and look at the equation at the bottom again now. $\sqrt{nG(n)}$ is the same, we haven't changed the generalized mass or q or the wing area. Your total damping has gone up. ζ has gone up, because your aerodynamic damping has gone up. Therefore, inevitably, $\ddot{y}$, your wing-tip acceleration, must go down. This means that you might have a factor in your dynamic testing that will tend to offset the loss in fatigue life that you're going to have because of the low temperature. Your fluctuating stress level must inevitably be down because your aerodynamic damping is going to go up. That I found a pleasant and surprising result.

Just coming back to the simple things of life, if you're trying to measure buffet onset, and there are a lot of people around who still say, "Well, I'm only interested in buffet onset," of course, this change of frequency parameter doesn't matter. And I don't think you need to worry about the differences between light models and heavy models. One build of the model will do and you can get buffet onset. But if you really plan to measure the severity of buffeting, I don't see how you can really avoid using this light-heavy model technique. In practice, I don't think you'd play it the way I've suggested. I think you would say, "I'm not spending millions of dollars on a high Reynolds number transonic tunnel and then getting most of my data at 27 million. Where I want my data is at high

Reynolds number." And so, in that case, you would probably design your model to have the right frequency parameter at the lower temperature and the high Reynolds number. So you'd be at the little triangle up on the left for the heavy model. You would design for that, and you wouldn't worry too much that at the lower Reynolds number your frequency parameter was wrong — you would laugh at that. You would take measurements down there, but you wouldn't worry too much about them. You certainly should be able to do that once you've got a feeling for how important this parameter is, and we don't really know. What I've sketched on my figure at the bottom is only tentative, although the excitation parameter is of the right order of magnitude for severe buffeting. You would design your model so the frequency parameter was right at the lower temperature.

And incidentally, while you were doing this, if you wanted to be really clever, taking note of the fact that you may have a little hidden bonus in the reduced strains and stresses that are going to come about as a result of the increased aerodynamic damping, while you're at it, you might even be able to do something about building in the correct static aeroelastic distortion as well, as on Moss's wind-tunnel model. But that's probably asking a lot. However, if you are going to consider what the frequency of this ordinary wind-tunnel model is, it wouldn't be too difficult to get someone to think about putting in the right static bending and torsional stiffness.

Now, where does this take us with regard to the flutter models? I think exactly the same principle applies. It needs more qualification, of course, but basically if you're operating in the cryogenic mode, you are going to find that mass has got to be added to the model without altering the stiffness. Effectively, this means that your replica model needs a lower structural efficiency. In fact, Bob and I were talking about this at lunch with Norman Lambourne at Bedford and he made this point. I'm not sure that you can make replica models for these low temperatures. I don't think the RAE workshops are competent to do it at the moment but I would like to think of someone trying.

I'd just like to mention some quite general things that really are asides, and then I'd like to suggest a particular test that we might talk about using this technique and this principle. The main interest in buffeting is predicting scale effects and evaluating the separate effects of static aeroelastic distortion and Reynolds number. Surface flow visualization is very important for this and I would certainly hope that a surface oil-flow technique can be developed to go with the cryogenic temperatures, because, in my experience, this is a vital adjunct of buffet prediction, to know exactly what separations on the model are exciting the structure. And boundary layer transition is also very important. As I said, it would be unwise to assume that achieving full-scale Reynolds number will give full-scale transition positions, because on the model transition will be determined by the combined effects of the tunnel unsteadiness and surface roughness. In the construction of replica models we may have to accept a lower level of surface finish than we have on conventional wind-tunnel models. I think perhaps we could just have the second Vu-graph now and I'll show the little Aunt Sally that I drew last Friday morning.

I'm taking the liberty of suggesting if anyone would like to study the techniques of buffeting measurements in your pilot cryogenic tunnel, that it isn't too early to do this. I think this is the time you should be doing it and I suggest that you might like to consider testing two half models that really are quite simple (Fig. 2).

The one that I'm keen on I have a vested interest in, wing A. This is a 65° delta wing of constant thickness so it would be a very easy model to make. There is a slight chamfer on the leading edge, which is as sketched. I have a nice set of buffeting measurements on this configuration. I also have a nice set of aerodynamic damping measurements on it which ties up much better than I've any reason to expect with the estimated aerodynamic damping. But the interesting thing about this is that, of course, a delta wing of that type is insensitive to variation in Reynolds number. And so I would argue that if you could prove your techniques on that planform and see how they compare with my measurements you'd have all the technique questions sorted out.

Now, if you say, "But we want to see the effect of Reynolds number," then we go to Wing B which has a semi-span of 150 millimeters and a chord of 50 millimeters, and that would have a wing section which Bob had already got and which is of interest both to RAE and to NASA, and Bob is going to test that in the 2-D test section. Hopefully, that would be an easy wing to make and test. But, of course, on that wing you would expect large effects of Reynolds number. And so I wouldn't recommend testing Wing B in the exploratory investigation of the technique.

I realize that I've gone beyond Bob's original request which was to just look at what one could do, but I'm a firm believer in looking far ahead. I think to get your feet wet in - sorry, cold - in some of these problem areas will be very good. Thank you, Bob.

Kilgore: Thank you Dennis. We do expect and hope to have some discussion and feedback. Raising questions and hopefully answers to questions.

Mabey: I don't know about the answers.

Polhamus: Dennis, on this last one, the delta wing that you suggested, for the instrumentation aspects of it, don't you feel that we probably can do the best job by comparing in the same facility the ambient and the cryogenic results at the same Reynolds number, if we can assume that the flow quality is independent of those two parameters?

Mabey: Yes.

Polhamus: But this checking one facility against another always concerns me because we know on these AGARD round robins, we never get identical answers and we've taken the approach here for our static work that we always make checks in the same facility with the same instrumentation.

Mabey: Yes. I think this is right. What Bob would be measuring would be unsteady wing-root strain at the fundamental bending frequency — What would it be? 500 hertz, say, — as a function of angle of incidence. Now the quality of the flow in the tunnel will only determine what happens at very low angles of incidence, say from 0 to 5°. Then we have the formation of the vortex, the plateau and then the vortex breakdown up here. Now these levels of excitation are very much higher than anything which pertains down here. That's a tare that we don't want. But, it doesn't spoil this experiment. But I agree if you could do this in the facility with air or nitrogen, it would be advantageous.

Incidentally, writing down the frequency has reminded me of something that I should have said. We have to be very careful when we do this experiment because when you change the frequency parameters, if you've got your heavy model, you're now going to get fewer cycles of buffeting.

And can I clearly state we need something of the order of one thousand five hundred — one thousand five hundred cycles of fundamental wing bending to be able to derive the buffet excitation parameter accurately. So, it's a little bit unfortunate. It means that with the heavy model technique at the higher Reynolds number and lower temperature, because your frequency is down to keep the frequency parameter constant, you will have to take your data for that much longer. But, although I haven't worked it out, I think the frequency would be of the order of five hundred hertz. So your four seconds will give you 2000 cycles. (The four seconds you were talking of yesterday, Bob.) So that's more than adequate. But to get the same accuracy in the buffeting measurements, you will need to run that much longer at the reduced frequency.

Hanson: Would you care to elaborate on that criteria of fifteen hundred cycles to get meaningful data? Do you have some experimental evidence that you need that long..?

Mabey: Well, we have in this test on the delta wing referred to, Geof Butler has tried, using the random DEC process, looking at short lengths of the tape. And actually I think we could get by with 1000 cycles, but I reckon that's the absolute minimum. We would prefer 1500 cycles. We have some measurements of buffeting in a blow-down tunnel, not our own, and these were made quite independently of ourselves, and they tried the same trick, breaking up the record, and they came up with about 1000 cycles. The difficulty is, it's a question of what accuracy you want. The fewer cycles, the smaller the accuracy. It's just simply a question of whether you will get the precision that people are claiming they need if you accept the smaller number of cycles.

Hanson: Was this agency taking different sample lengths and seeing if by varying them you would change the answer?

Mabey: That's right. I'm sorry, I should qualify this too. This was for separated flow conditions, genuine buffeting conditions. Down at zero lift on that curve I don't think you need 1500 cycles of buffeting, I'm sure you don't. But when you're up in the vortex breakdown region, then you certainly do. Even when you're up on the plateau you need something of that order.

Hanson: Is this because for the vortex flow type of buffet response you have a kind of beating or mixing of the ...?

Mabey: That's right! The flow itself that you're trying to measure is inherently unsteady. It really sets its own time scale. This applies even measuring the steady static forces on the model. That is why I made the remark in parenthesis yesterday about Coe's paper, that he presented to the STA last year, (which is now available as AIAA paper 75-142). It did bring out this point rather nicely, that even to get the d.c. level, in separated flow conditions, you do need the longer measurement times.

Reed: How about elaborating on the increase in damping with the reduction in temperature.

Mabey: Well, that's just a consequence of writing down the aerodynamic damping on this model as proportional to ρ times U. I hadn't realized it, but I wrote the equation down and that's the way it works. I sound rather like a magician pulling a rabbit out of a hat. But it's there in the equation. It's just that I don't think we appreciate these things.

Hall: At the very beginning of your talk, you mentioned that you doubted if a static pressure device could be operated in the cryogenic environmert and I assume the need for getting it in the environment itself is the fast response times required of the gage. I know, this is the beginning of your reference...

Mabey: Sorry, I meant you could measure that remotely. You could pipe the pressures out to your observation.

Hall: For all pressures...?

Mabey: For the mean pressures you could probably do that.

Hall: Right, but did you mention an alternate method that could possibly measure the fluctuating pressures?

Mabey: Oh no! What I'm saying is that I think if you want fluctuating pressures you certainly can't use it in that way — with piping, because obviously the density changes may eliminate this technique right at the source. So, I'm saying that you unfortunately have got to make something like a Kulite that will operate down at the very low temperatures. I'm just waiting for Bob to spring up and tell me that such a device is being developed.

Kilgore: I'm not sure. I think we do have pressure devices, that could be called microphones, that have inaccuracies at cryogenic temperatures, order of five percent. We really need Joe Guarino or someone here from our Instrument Research Division to fill us in on what is available.

Mabey: Well, if you could get five percent now and it's a relatively small transducer, then I think most of our worries would be over. Because I think if anyone with a buffet investigation could integrate his pressure and get that sort of accuracy he would be more than satisfied, wouldn't he?

Hanson: That's right.

McKinney: Dennis, do you think you have to have this transducer located closer to the source, say, than in the fuselage?

Mauey: On this delta-wing experiment? The accelerometer on this delta-wing experiment?

McKinney: Yes.

Mabey: No, I don't see why you shouldn't use a wing-root strain gage, in principle, you've only got to calibrate that.

McKinney: No, I guess I was referring to the pressure measurements. Where you're trying to make dynamic pressure measurements. Obviously, there is a tube-length limitation.

Mabey: Yes. Well, O.K., you might get away with a wing root. But I don't think on a general wing that you'd be able to do this.

Hanson: You talk about frequencies of interest in the order of two to five hundred cycles a second.

Mabey: That's right, yes.

Hanson: It would take a pretty short tube length to do that.

Mabey: Yes.

Well, we've never liked this technique and I just don't see how you could possibly make it work in this environment. I think that measuring fluctuating pressures to predict buffeting is a very hard way of doing it and it does raise all sorts of questions. You've still got the problem of what you're going to do about damping. I think intrinsically, allowing your model to integrate these pressures for you is a better and easier way. But as I say, I'm prejudiced and I can't take an objective view. I mentioned the thing about the fluctuating pressures because a lot of the work that Norman Lamborne is doing is concerned with oscillating controls. And for that work you have got to know the fluctuating pressures. That is the end product, and I don't see, at the moment, how you could actually do it at cryogenic temperatures.

Kilgore: Dennis, there's quite a bit of work being done in this country, because of high pressures involved in high Reynolds number tunnels, to define on-line the precise shape of the models, especially the wings. Conceivably this readout could be used to give you the information you need.

Mabey: It could certainly give you the acceleration. Yes, very nicely, and presumably give you mode shape as well.

Kilgore: Right, depending upon the refinement of the technique.

Mabey: Yes. Which would be important if the model really were a flutter model, but I've restricted our interest to a model which is an ordinary wind-tunnel model which, hopefully, isn't going to do anything silly with mode shape.

Baals: Dennis, what would be your assessment of the importance of Reynolds number in this buffet-flutter area, considering, just from your inspections of the kinds of changes in wing pressure distributions we've been able to identify between low and high Reynolds numbers?

Mabey: That's a good question. On this delta-wing model, I don't think the Reynolds number matters at all, where in fact, the dimensions I've selected for that model of yours would go in your tunnel and the Reynolds number range would overlap my own measurements. So you could take those measurements, overlap the Reynolds number range, and then you could go on up in a way that I couldn't. But, I would be very surprised if you saw much of an effect of Reynolds number on that plate. There will be some. We know on Concorde that, contrary to what we had expected, there were scale-effects on that slender wing with the sharp leading edges, but they were comparatively small.

Baals: If you had something like C-141 where you would expect....

Mabey: Ah! Right. Or on Wing B. On Wing B, if we take the conventional situation, and let's say that we are essentially interested in a flutter test, then that has a really important simplification because this almost invariably means that you are testing at zero lift. If you're testing at zero lift, the flow is attached, the adverse pressure gradients are really quite small, there are no separations, and therefore, a priori, you wouldn't expect large effects on the flutter boundaries. Unfortunately, if you want to do a buffet investigation and you want to do something like a 5G pullup on an F-15, then this involves a huge area of separated flow. You would be very worried about scale effects. Even before buffet onset you'd have severe adverse pressure gradients, you would certainly expect scale effects in this situation. So that's a nice clean distinction. I used the word clean very carefully. But, the catch is the situation where you can't draw the line between flutter and buffeting so neatly. And I suspect there may be one or two situations creeping up on us where this nice dichotomy can't be maintained. In that case, I would have to be agnostic about it. But I would suspect that the scale effects would be very important.

Hanson: Do you feel that their importance apply primarily to the buffet onset? As distinguished from the maximum where you have well separated flow and...

Mabey: I wish we knew the answer to that one.

Hanson: You don't have any experience?

Mabey: I don't think anyone does. I think such evidence as we have, or I've seen, is that on conventional wings, the Type A flow separations, I think the major effect of Reynolds number is to raise buffet onset, and the slope of the excitation versus incidence curve is not generally affected. But, having said that, I do know of one aeroplane which is in service where the addition of vortex generators, which was essentially a boundary-layer control device, raised buffet onset. But it didn't alter the buffet penetration limit because the pilot, in fact, had a curve whose onset was deferred and when it finally went it went much more severely. But, I think it's difficult to generalize. It's impossible to generalize. This is certainly a question we would like to know. In flight, of course, it gets awfully confused. It's actually very interesting because even on quite a stiff aeroplane you can not sort out these effects — static aeroelastic distortion and Reynolds number. Your pilot produces a set of buffet points at different altitudes. You are changing Reynolds number and you're changing static aeroelastic distortion. And, in the light of the scatter of flight data, I don't think its surprising at all that we should be having difficulty in establishing tunnel-flight correlations.

Rao: How reliable do you think are balance measurements for predicting onset and intensity of buffeting?

Mabey: I don't think they're accurate at all. I would put very little faith in anything derived from buffet measurements on a balance —, sorry, apart from buffet onset. If you're lucky you may be able to get buffet onset.

But in general, anything that comes out of your model balance is nonsense because it's an unrepresentative mode and it's an unrepresentative frequency parameter.

Having said that, you can use your balance outputs, sometimes, to learn a great deal, for example, about the flow in the tunnel. I didn't mention this yesterday, but, just as a point of history, in the movie we saw a slender-wing model being pushed into the tunnel and then we saw sound waves going past it at transonic speeds. My research into tunnel unsteadiness started in the days before we had pressure transducers and all we could do was to monitor the fluctuating response on one mode of the model on its balance, which in fact, was the axial-force fluctuation. And that gave a measure of the excitation in the air-stream at that frequency. It was an unrepresentative mode relative to what an aeroplane has, but it was useful in helping to sort out the tunnel unsteadiness, at least getting into the problem.

Now it may be, Bob, that this would be something useful to monitor in your cryogenic tests because you can get it as a bonus whenever you're running. It doesn't cost you anything. You can measure the damping in that mode from your trace. You can get a buffet excitation parameter in that mode and maybe, hopefully, on successive tests of the model, you could learn something about changes in the tunnel if you were having difficulties with instrumentation, getting pressure transducers to work and that sort of thing. It's just a thought. But generally speaking as for buffet prediction, no, I have no confidence in it.

Polhamus: Dennis, you mentioned some Reynolds number effects observed on the Concorde. Do you feel this is primarily in the secondary vortex?

Mabey: I don't know. I don't know where this comes from. I don't think anyone does. There are differences. They are really comparatively small, but, in assessing the performance of this aircraft, people have been measuring more parameters than on any other aeroplane, I think, than have ever been measured before. People have attempted to get drag and lift-drag ratio to better accuracy than they have ever done before. And so fine detail has been measured which on a conventional aeroplane perhaps would have been lost. But, yes, there are differences in the mean pressure distributions, they are small, but I don't think anyone knows their origin. Probably they are in the secondary separation area. Certainly on the slender wings that I know of, if you look at the primary vortex you can't see anything at all if you test two Reynolds numbers. They are identical.

Polhamus: Have you seen Stahl's static data, in Germany? It's a more slender wing than the Concorde and he does show some Reynolds number effects at transonic speeds. But, again, I'm not sure whether or not some of it isn't aeroelastic effects. It was at fairly high pressures.

Mabey: Yes. This could be.

Polhamus: But there could be effects on the secondary vortex.

Mabey: Well, of course, this could be where its coming in on the Concorde because the wing is an extremely flexible structure. At the time the wind-tunnel testing was done I don't think people in the RAE were quite as aware of the importance of static aeroelastic distortion as perhaps we are now.

Perhaps I should correct that. Tests were made of slender-wing models that had got the right sort of deflections, they were deliberately made flexible but I think it's true to say these were never actually done on the final Concorde planform. The tests were made in the early days and it was said, "These effects are all relatively small and mixed up with any small Reynolds number effects." But, I think it's true to say that we didn't actually have a proper distortion model of the final Concorde planform tested. So that's one possible discrepancy.

Nicks: Well, hasn't the situation improved with the flight Reynolds numbers as opposed to what the model tests would have shown. In other words, you wouldn't get in trouble by virtue of your differences here on Concorde. It gets better at flight Reynolds number.

You might expect control or tail-size problems or something but... I think you would improve your flutter type problem at the higher Reynolds numbers of flight, most likely.

Mabey: Yes, I think this is probably right.

Nicks: You might add some weight to the airplane to help take care of it.

Mabey: Yes. But when I'm talking of differences I do mean specifically differences when the aircraft is flying at 10 or 12° incidence with steady vortex flow. And then the pressure distributions are a little different from what we were expecting. And this affects things like spanwise load distributions. But they are small.

I'd hate to have given the impression, and I must correct it if I have, that there are large differences on the Concorde, they really are quite small. But on the other hand we are talking about fine detail.

Polhamus: But, I presume there is some radius on that leading edge and we have observed, of course, some Reynolds number effects if you have some radius. As you go to full scale you tend to lose lift, for example.

Mabey: That's right. Well, at a fairly late stage a change was made on the leading edge, which isn't as sharp as it used to be, say 5 years ago.

Unidentified Speaker: Would you expect more Reynolds number effect on a wing with less sweep, like the American proposed SST, for example?

Mabey: Oh, I wouldn't want to generalize on that one. What I would say is that I think when you come onto Wing B, as we were suggesting, this cantilever wing of constant chord, then I think you will have some splendid large Reynolds number effects on that.

Polhamus: With regard to using main balance data, you indicated that you didn't feel that you could use that very accurately even for buffet onset...

Mabey: Well, for buffet onset it's probably alright.

Polhamus: Particularly if you have a leading edge type separation, your loss of thrust...

Mabey: You might be alright with buffet onset, but, in one of my papers which is published, but unfortunately classified, there is a case where we did get a wrong answer on buffet onset.

Polhamus: What were you using as an indicator?

Mabey: We were using fluctuating normal force. And this gave...This is quite interesting actually, because its something we never explained. It was a wing that had a planform something like that [sketching at blackboard], and we plot C_L for buffet onset versus Mach number. And we got an answer at $M = 0.6$ which was very sensible and in very good agreement with the flight data. But, when we got to 0.8 Mach number we got buffet onset, [sketching at blackboard] which was up here at 0.5. The real aeroplane was down here at 0.15. So we were seriously in error on this curve. In fact, what happened was a result of the model dynamics on the sting, which, of course hadn't been tailored with respect to frequency or stiffness, but was just as it came for static-force measurement. What we were seeing, I think, in this situation, was a gross fluctuating normal force, was showing a very large increase in the condition where the model really didn't know whether to be buffeting with shock-induced separation, like that, or with a leading-edge separation, like that. In other words, this is transonic attachment. And we saw the transonic attachment buffeting there. We didn't see this early phase of buffet onset which was actually observed in flight. This is rather amusing because this tied up with... I think, ties up with a problem of current interest on something else. But, be very careful about using fluctuating balance measurements. You may be lucky. You may be lucky. But you can be seriously in error even for buffet onset.

Igoe: Well, Dennis, how would the use of a wing-root bending gage have altered that result?

Mabey: Because you've got the right mode shape and the right frequency, and subsequently, we put wing-root strain gages on the model and we got very sensible answers in agreement with the flight data.

Igoe: Well, then you attribute this difference in what amounts to the buffeting excitation to the difference in mode shape that the wing was responding in.

Mabey: Yes. The model is going to do an integration for you. And it integrates the fluctuating pressures with the mode shape that's being excited and at the frequency. The implication of that result is that the integral of the low frequency in the balance — in that balance mode when you do something like that, is of a very different order from what you get when integrating the fundamental bending. That's the sort of physical explanation, isn't it Perry? It must work like that.

Hanson: That raises another point, I'd like to ask you about though, Dennis. Everybody's familiar with your technique of taking buffet coefficients, intensity levels, with measurements of the wing-root bending moments. And I was wondering how these recent indications from Gerald Moss...He's observed some very strong torsional responses..., I don't know whether you call it buffet response now or not. I was wondering, how does that affect your technique, do you see coming up with a refinement to that technique whereby you put on torsion gauges and get pretty much the same thing?

Mabey: I think you'd be very wise to put on torsional gauges. Yes. Perry made a very fair comment that while restricting the measurements to fundamental wing bending we have left out what happens to fundamental wing torsion. Yes, we have. And, in principle, I'm quite sure that we should, now, put gauges on to measure the wing torsion as well. The difficulty is, I suspect that you won't get such a good match of frequency parameter. I suspect the mode shape isn't far wrong, but I doubt if the frequency parameter would be much further out, I think, than the first bending mode. But that at least would give you some insurance.

Hanson: From what I know of the results of that experiment, he measured predominately torsional response at some combinations of Mach number and angle of attack.

Mabey: That's right. And no response in fundamental bending mode.

Hanson: Yes. That's a little unusual, though, isn't it?

Mabey: It's very unusual.

Hanson: There wasn't anything unusual though about the stiffness distribution or mass distribution of that wing, was it?

Mabey: Only that it was right.

Hanson: I mean it was characteristic of an actual airplane wing. I guess the point I'm getting to is, do you have any feel for why that would happen on this wing and has not been observed on other wings? Although, I'll modify that. I do know that Kennedy has indicated that he has measured some small response in torsion on the F-111, but certainly not anything predominately important.

Mabey: I think the physical explanation of this is a fairly simple one, that people are striving for much more performance than they used to. So you're making your wing sections stall at precisely the same C_L. That's the optimum. If you do that and, if the wing is swept at low angle, then I think you must expect that this type of response may appear. Its much more likely to appear than when the wings are swept back 65° and the flow is highly three-dimensional.

Hanson: I gather that Moss now thinks that his response isn't buffeting, per se', but is rather more associated with flutter. Is that a correct assumption?

Mabey: "Stall flutter," or perhaps we would be wiser to use the word "buzz."

Igoe: Would that be because it's just a single mode response rather than a coupled response?

Mabey: Well, I don't think we know sufficient about the phenomenon yet. I think it is uncoupled. It's certainly amplitude limited and it's much more analogous with the work that, for example, Norman Lambourn did on aileron buzz. I think that's the best analogy that we know of. But I don't think we know enough about this particular problem. Moss had a nice simple experiment to look at static aeroelastic distortion and put on some fluctuating gages just to see what happened. This is what happened.

Reed: Do you see a role for the cryo-tunnel in flutter clearance? Actual aircraft using fine dynamic aeroelastic models?

Mabey: Well, I'll be agnostic here. You've got a very fine Freon tunnel and as long as you've got that I suspect you'll use it. I mean that's capital that's paid for years ago by the American taxpayer, and I suspect you're going to prefer to do your flutter tests in it. On the other hand, I think if you follow this sort of argument about reduced structural efficiency, there might well be situations where perhaps you would sometime in the future want to use the NTF for flutter testing. But, I really don't know about that.

Reed: I was just wondering about special problems in constructing models.

Mabey: Well, I'm sure there will be. I think that the sort of model that I'm suggesting here, buffet wing, I'm pretty confident that given a couple of years and maybe losing one or two models down the tunnel, you'll be able to do buffeting as a matter of routine. I don't know about replica structures, of course. This is what the people like Jack Baldock would really like to know, if you people really think it can be done.

Hanson: By a replica model you don't mean in the sense that the model is just reduced geometrically and scaled to flutter, but is scaled stiffness and...

Mabey: Scaled stiffness and same kind of aircraft... I'm using replica in the broadest sense. What's your view, Perry, surely you have your Freon tunnel...?

Hanson: Well, I think that certainly the cryo-tunnel can be well put to use measuring flutter in the sense of trend studies using fairly simple models, but when you go to a dynamic scaled aeroelastic model you're getting into a pretty expensive model just for use in our facility. The other complications of the extreme temperature would make it prohibitively expensive for one thing. My big doubt, though, would be in the change of structural damping with those large temperature changes. I realize you do get changes in aerodynamic damping also, but I suspect that if the model is built of different materials...fiberglass, aluminum, balsa, all of the things that go into one of these models, that your chances of having large changes in damping — structural damping, are pretty good.

Reed: Or even the shape of the model. Two different coefficients of expansion.

Mabey: I think if we're careful we could probably make an aluminum wing loaded with lead shot without getting into that, don't you think, Perry? I think we could do that.

Baals: Aren't we sort of talking about a similar situation though...We've been perfectly content to do all our transonic work at three million Reynolds number and now we're beginning to find out we're not getting the right answers. So we've had to go to some very expensive facility now to close out the Reynolds number problem. Maybe in the flutter and the buffet area we're in this same problem. We do have a Freon facility but you're still in the transonic region sitting there with a few million Reynolds number. And as we get into this area more deeply and you find out you do have a Reynolds number problem associated with buffet, then you're probably going to use the cryo facility like we're going to use it. It would be a topping plant in many respects in which you'd do as much of your homework as you could in — let me call it sub-scale facilities, and then you'll try to nail down the Reynolds number effect in some key area that you're concerned about. And I think with this single cryo facility and the multitude of low-Reynolds number facilities we have, we can look at it as being used in that kind of a vein.

Mabey: Yes. While we're on that one, I'll just throw in that it is astonishing to me the remarkably good agreement that Perry got on his F-111 flutter model when he was measuring buffeting, the Reynolds number really is quite low. It's only just a million!

Hanson: It was an order of magnitude off.

Mabey: And, it would be, I think, most unwise to assume that this would happen on a wing with an advanced section. This wing almost certainly had type A flow separations that are insensitive to Reynolds number. But even having said that, with a different γ and everything else, it is really very remarkable. It would be unwise to assume that this is a general result. If that is so, then it strengthens what you've just said about using the cryogenic tunnel for these techniques.

Baals: I do hope that they don't come into a Reynolds number problem because you've got problems enough with aeroelasticity effects. But, for awhile, you know, it looked like the C-141 was a peculiar phenomenon and somebody said maybe it would be easier just to buy up the C-141's and take them out of business. But, we find out that the whole development of the supercritical airfoil at 3 million Reynolds number is almost impossible except by using very subtle and ingenious techniques which can be applied in rather limited areas. When you go to 3-D it's sort of every man for himself. So, this provides the capability anyway now, to do the high Reynolds number development of some of the advanced airfoils in the 3-D configurations. With this kind of capability, that would exist in the facility, as long as you're getting good correlation between wind tunnel and flight and you're off by a factor of ten [in Reynolds number], more power to you. But, as you get into some of the supercritical airfoils and you find out that the center of pressure may be off by 20 percent in the transonic range, then I think sort of historically you may find that you're going to go through the same traumatic experience that we did about three or four years ago, or longer, when we ran into these Reynolds number problems. It sort of required that we had to do some of our high Reynolds number development in flight, which gave us the typical three-year cycle time which is completely unacceptable.

Mabey: It just occurred to me that I might give a plug for flow quality here. Things like C-141, transport aeroplanes, are very much more concerned about buffet onset than buffet penetration. Buffet penetration is virtually academic for them, but buffet onset is very important if they're cruising just below the buffet boundary. Well, what my work has shown, quite clearly, is that buffet onset is very sensitive to flow quality and you can't detect buffet onset [in a tunnel with inadequate flow quality]. And, therefore, in talking to people who control money bags and the expense of tunnels, one should always be careful to point out that what they save on a cooler could easily be lost on getting the wrong buffet onset at 0.85 Mach number on some advanced transport and that might pay for your cooler in one week of operation.

Hanson: There's a lot of talk about testing buffet models in this facility now, flutter models and things like that. Is the facility being designed so that when somebody wants to put a model in the tunnel that might go down the tunnel...?

Baals: Wayne? We've only vaguely thought about the possibility of trying to put a screen in the fan section, which I think is probably what you're talking about.

Hanson: That's right.

Baals: Is the fan screen, by the way, in the transonic dynamics tunnel?

Hanson: Yes.

Baals: Is that what you really mean?

Hanson: Yes. In conventional tunnels you meet a lot of objections sometimes when you ask to put a flutter model in the tunnel where there's not only a chance but maybe a likelihood that the thing might break loose and go down the tunnel. That's a no-no.

If we're seriously considering testing flutter models and buffet models in the cryogenic tunnel to establish these Reynolds number effects, then in the design stages that ought to be taken into account. Some means of safeguarding the tunnel.

McKinney: We should at least provide provisions for installing the screen at some point in time.

Baals: Yes. I've made some simple power calculations. And they get pretty horrendous if you have to live with it all the time for those few cases. But, I think that we do have enough pressure ratio in the system to go to 1.2 Mach number or higher. And for specialized flutter tests, I would think it would be very appropriate to start to design a screen that could be installed for those cases where you want to dedicate a period of time for flutter tests.

Here again, I guess, the interest and the interpretation of the necessity for doing that kind of work is the kind of thing we would like to scout with you because the driving force for the necessity for higher Reynolds numbers in flutter is foreign to us, shall we say, but certainly we can start making the first move to try to design into the tunnel the possibility of installing an interchangeable screen. An adequate hatch that could take this thing in and out.

Mabey: If you put those things in now it'll save you a lot of money later.

Hanson: I have a question on the flow quality, maybe directed to Bob. We know flow quality varies quite a bit in transonic tunnels. Is the NTF being designed to have comparable flow qualities with an existing facility or ...?

Kilgore: I can defer to Wayne. I think the philosophy is that it should be superior to existing facilities. Wayne will elaborate on that.

McKinney: The intent is to try to make it superior to existing facilities and I guess that's part of what we're in the process of trying to iron out right now.

Hanson: You mean the best of existing facilities?

McKinney: Better than a half of a percent. I guess our design goal, of course, has been a tenth of a percent and many people feel that we can't get there, including Dennis, and this is probably right. But I guess one of the things that we're trying to work out at this point in time is what steps to take to insure that we do get the best that we can get within real-life constraints, after careful consideration.

Kilgore: That prompted Dennis' remark about leaving out the cooler. Actually, we left out some correlation here in that the presence of the cooler as such, whether it cools or not, does not affect the flow quality. But, its presence is going to affect the performance of the rapid diffuser and what comes after it. And so, Dennis' point is that to save a buck by removing the cooler may save that buck, but may cost you an awful lot in the [flow] quality of the tunnel.

Mabey: And on the prediction of buffet onset on a transport aircraft, we know that onset can be sensitive to the quality of the flow.

Unidentified Speaker: They're not going to take the cooler out and not put anything in its place, are they? I think they're going to put some honeycomb in its place.

McKinney: If the cooler comes out, its losses, or its benefits on flow quality, will be compensated for by the addition of a honeycomb and screens, in combination, at that location.

Unidentified Speaker: Does that sound alright, Dennis?

McKinney: Our hope is that from a flow-quality standpoint we will wind up just as good or conceivably better.

Baals: But, by virtue of having to provide something in place of it, the cost differential now is coming down to a point where I would tend to think it's almost within our noise band of cost estimating.

McKinney: You're getting down to the one percent category.

Baals: That's about right. One percent.

Reed: The cost onset boundary.

Hall: How vulnerable would that internal insulation of the tunnel be to a model flying down the tunnel?

Baals: How's that again?

Hall: Well, you know that....

Baals: Oh, I know what you mean.

McKinney: Probably would be the least vulnerable part in that area because remember in the high speed diffuser and so forth, there is an internal liner. Its almost a two-shell type of construction, with insulation in between. So there is an internal liner which provides the aerodynamic surface that's pretty good gage material.

Hall: O.K.

McKinney: However, there's not much to stop it from going into the fan, as presently conceived.

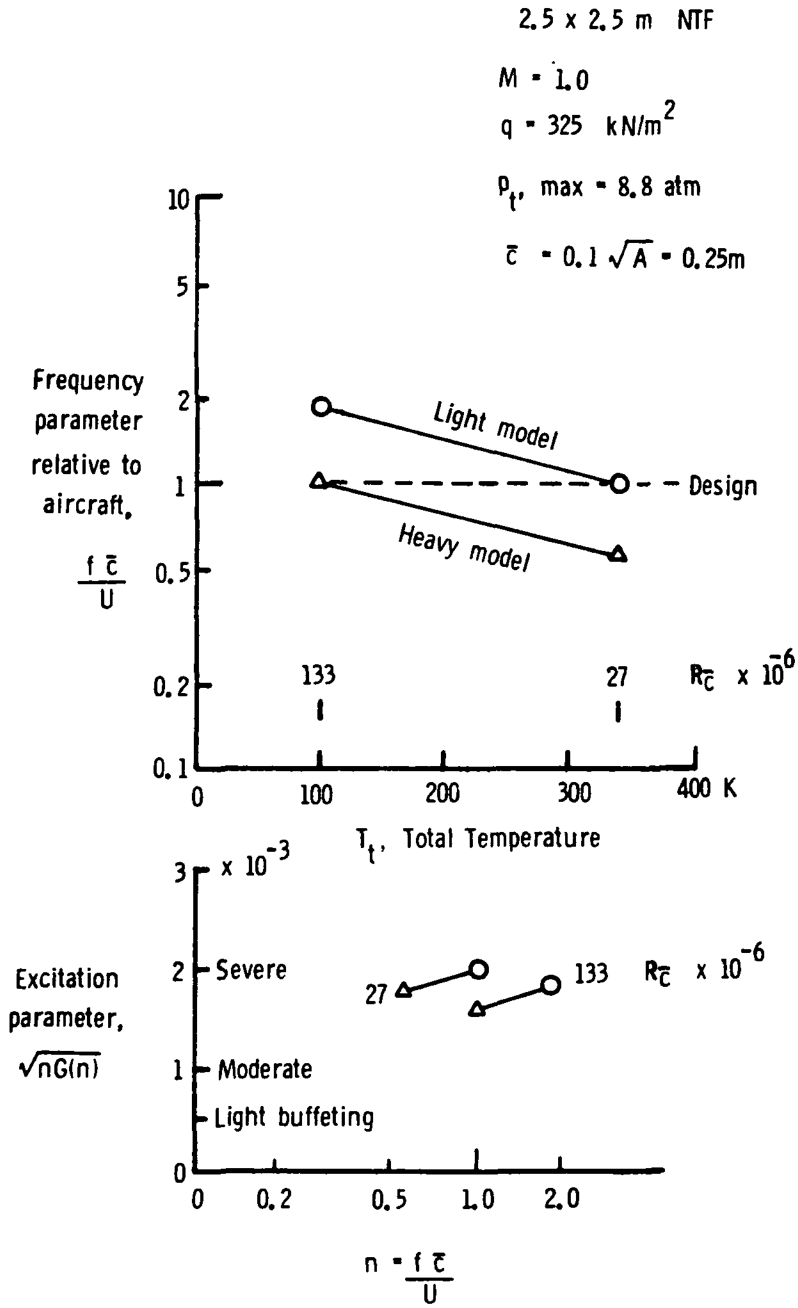
Kilgore: Gentlemen, our approximately hour and a half is up.

What we'll do now is thank you for your presence, your attention, and your participation. We thank Dennis for his involvement and interest in this and for his willingness to subject himself to two trans-atlantic crossings at subsonic speeds. Hopefully, it will be rectified in the near future. And with that the formal, or informal, as you will — part of the program will end. Thank you.

Mabey: Thank you.

REFERENCE

Mabey, D. G.; and Butler, G. F.: Measurements of buffeting in two 65° delta wings of different materials. RAE TR 76-009, January 1976.



$$\sqrt{nG(n)} = 2/\sqrt{\pi}\left[m_1\ddot{y}/.qS\right]\zeta^{1/2}$$

Figure 1.- Prediction of Buffet Excitation Parameter.

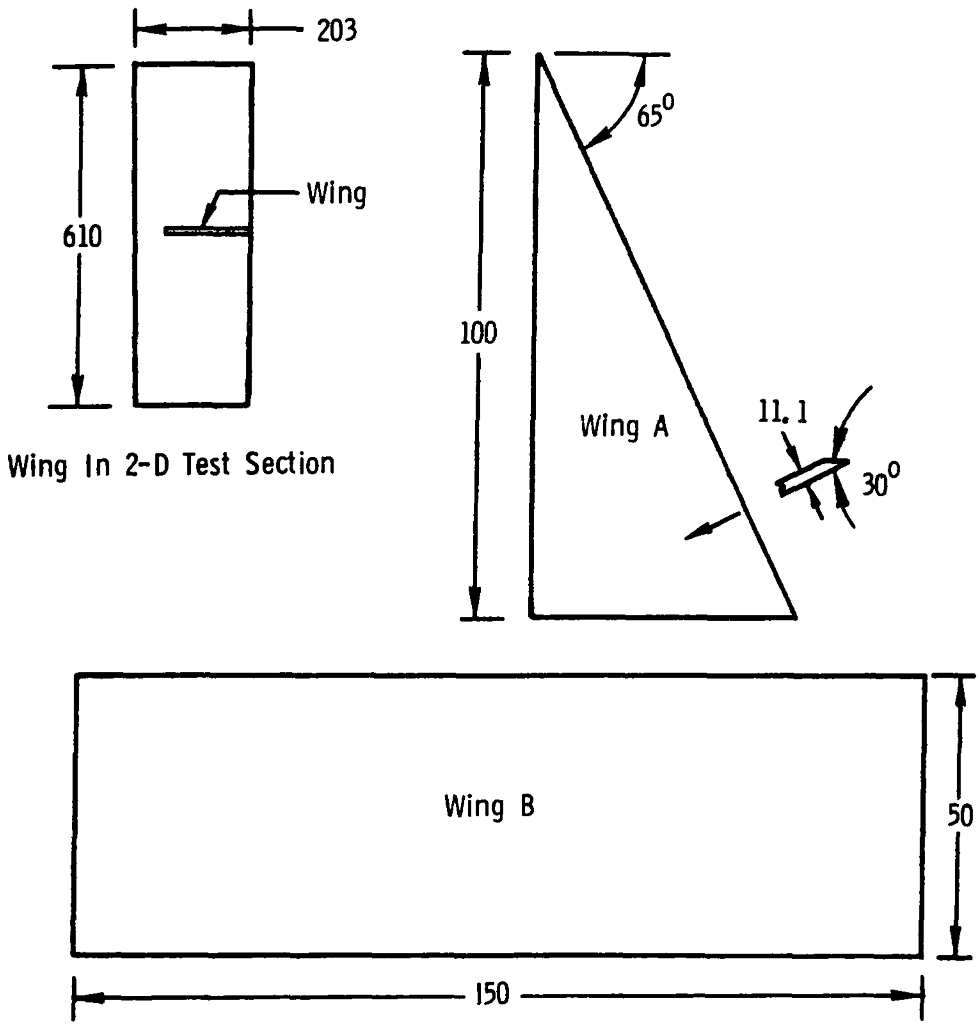


Figure 2.- Wings for buffeting tests. All dimensions in mm.