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THE DANIEL AND FLORENCE GUGGENHEIM MEMORIAL LECTURE

CHOICE AND BALANCE
A RESEARCH PROGRAMME IN AERODYNAMICS IN PERSPECTIVE

by

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Abstract

NLR's research programme in the aerodynamics of steady compressible flows is surveyed over the period 1960-1970, from the point of view of research planning. The factors determining the choice of research subjects, and their balance, are discussed. Three major projects in aerodynamics, and their motivation, are briefly surveyed. In an epilogue, some general observations on research in aerospace are made.

1. The problem of choice in aerospace research

Around 1924, Daniel Guggenheim, then at the age of 68, began to take an active interest in aviation, at that time in the United States mainly practised for its entertainment value. He initiated and supported on a grand scale various major activities, notably in education and research, among them the work of Goddard, the American rocket pioneer. The impetus he provided, continued by the Florence & Daniel Guggenheim Foundation after his death in 1930, has generally been recognised as an important factor in a development changing the aerospace scene of his days beyond all expectation, and giving his country world leadership in one of the major areas of advanced technology.

Today, nearly half a century later, when in the tradition of the ICAS we pay tribute in this first lecture of the 8th Congress to a man acutely sensitive to the changing opportunity of his time and acting in a spirit of public responsability, we cannot but be aware doing so in a period in which the future relation of science and technology in society is again in critical discussion. Many critics of contemporary society (fig.1) agree that if not the choice of objectives, then at least the balance of priorities has gone astray; unfortunately there is less agreement on the political options as to quality and quantity of controlling action. Now, most of us are not concerned with policy decisions on a global scale, and I hasten to reassure - or perhaps disappoint - you in having this morning neither analysis nor solution to offer of the predicament of mankind. However, I think we all can agree that in whatever terms

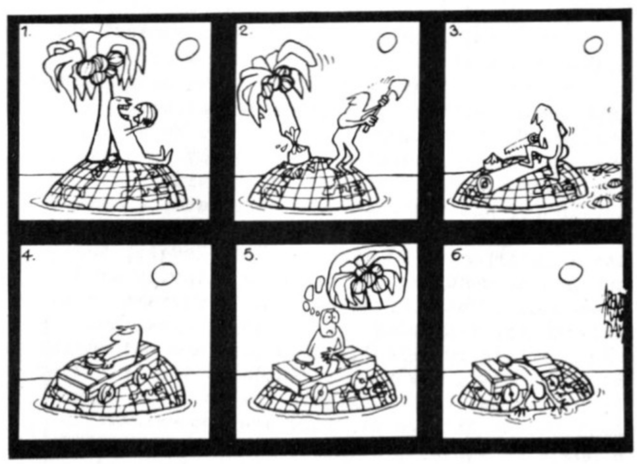

Fig. 1. (Courtesy VU-magazine).

one appraises the interaction of technology and society, its problems already vitally affect the aerospace community today, and increasingly will do so in future. Most of the present company will probably also be inclined to continue to share Guggenheim's expectation that in this development, scientific research will remain at least one of the keys to progress, however redefined.

These two commonplace observations point out the topical interest of a problem that over the last decade has been recurring with increasing frequency in policy discussions at all levels in research institutes, universities and industry: the problem of choice in research. What are the factors that effectively motivate the choice of research subjects, and how do we balance them in our policy decisions ?

The problem how to control the development of science, and the technologies based thereon, as distinguished from the question how to advance or conduct it, is of comparatively recent origin. The development of science has been variously considered as progressing autonomously under the force of its internal evidence (ref.1), as moving in jumps under the impetus of scientific innovations intermittently induced by brilliant individuals (ref.2); or, in a different tradition, in its relation with social and economic conditions (ref.3). None of these descriptions gives much information on the problem of research policy and planning; and indeed, this problem has only emerged with the institutionalization of science completed. Research as conducted nowadays is part of some institute's programme, what is next to be done is embodied in its planning, the institute more often than not being supported by public funds. An exception to this is perhaps the field of mathematics, which (as I recently heard observed at a faculty meeting) apparently is still hand-made in the attic, by candle-light. As a rule, though, both basic and applied research are now produced by institutes, and these are presumably under control. Seen on the national level, public funding is involved for various institutes conducting basic and applied research in many directions; then this control is up to political scrutiny and ought to be optimised in the national interest. This is the problem of choice as met on the governmental and the institutional level; analogous problems exist of course in purely industrial research, or in the many forms of mixed private and public enterprise. It has now become also a recognised academic problem, having several research groups and a newly published journal directed towards its study (ref.4).

Evidently, in its most general context the optimization of research is a problem of research policy, which finds expression in research planning, implemented within multiple organizations through their management structures. Moreover, one has in the general case obviously to consider the international, national and institutional levels (or alternatively the corporate, company and laboratory level), with proper regard to their interfaces. On each of these levels, then, one has to consider the research activity as a feed-back process directed towards certain broad scientific, technological or social aims, and consider its dynamical, economical and methodical aspects. Next, one has to decide on the method of optimization, beginning with how to weigh what factors. On this point there are two extreme schools of thought, both claiming essentially that here is no arguable problem. One holds that, dependent as we are in research on the hidden depths of the genius of the individual researcher, we can only try and provide a favourable research atmosphere including adequate resources, and see what happens. The other school maintains that given sufficiently large computers, and even larger stores of statistical information, the whole problem can be entirely rationalized in terms of cost-benefit estimates and systems analysis. If we accept this, the problem is computable, not arguable.

All this beingthe case, let us begin to cut down the problem to those aspects this lecturer could be reasonably expected to talk on, in this company, for one hour.

First of all, I will leave out all organizational or managerial aspects of research. Secondly, I will confine the discussion to the institutional level of applied science, in particular the aerospace laboratory; and reduce to the perspective of the working floor. This means that in the interface with the national level, certain broad research objectives are assumed to be defined, and certain resources have been allocated. What remains to be considered, then, are the internal components of the dynamical, economical and methodical aspects of the conduct of research, as mentioned above. These can be described in terms of a number of balances affecting the choice of research subjects (fig.2).

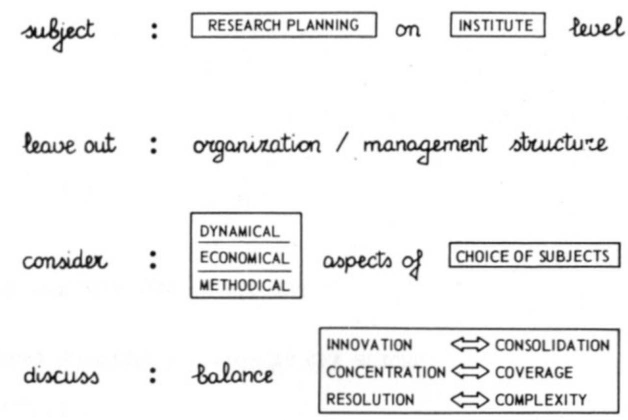

Fig. 2. Balances involving choice of research subjects.

Assuming for a moment complete freedom of choice, one could choose to direct a laboratory's total capacity either towards complete innovation, or to improve consolidation of existing knowledge; one could concentrate all resources in one subject, or try to cover all possibly relevant areas; finally, it would be possible to develop exclusively highly refined methods uncovering loads of detail, or only try and find rough and ready methods applicable in situations of great complexity. In theory, one could choose any combination of these extremes; in practice, of course, we have to strike a careful balance, and this is our research policy in whatever degree of explicitness we seem expedient.

Now how do we balance, if possible near the optimal ? - always assuming the usual case in which the laboratory has a non-trivial say in its future programme. This is the central problem, for which in my opinion, no mechanical method of solution can be found. What I propose to do, therefore, is to undertake a case study of the problem of choice in an actual research situation. For obvious reasons, I have chosen the research in aerodynamics of steady flows conducted at NLR, over a ten-year period. Not all the work done in this period has been taken into account; only those parts that can be considered programmatic in the sense of having continuity; the complemental incidental activities having taken some 25 % of the total. On one count, however, I will just beg the question : a description will be presented how the balance worked out, but having been for most of the period a member of the group concerned, I will not presume to give an evaluation of its results, either on internal or external criteria.

2. Aerodynamics at NLR : broad research objectives

In the Netherlands, we like to draw the dividing lines in such a way, that we come out as the smallest and poorest of the large and rich countries. This means that we feel we cannot afford to entirely opt out of advanced technology, although our scale of operations is obviously restricted. The formula we have arrived at is to be warm advocates of international cooperation, on the basis of a limited national effort in certain carefully selected areas. As one such field, what has now become aerospace technology was chosen in the post-war period, and industrial, educational and research capacity in this direction was built up. The research capacity has been mainly concentrated in the National Aerospace Laboratory NLR, which stands at the service of the industry, the government agencies concerned with aviation, and the civilian and military operators. The laboratory itself is operated as an independent organization, controlled by the various interested parties; and lives partly on a budget supported by the government, partly on contract research.

Now let us survey the aerodynamics research scene (fig. 3). Much if this is laid out at the

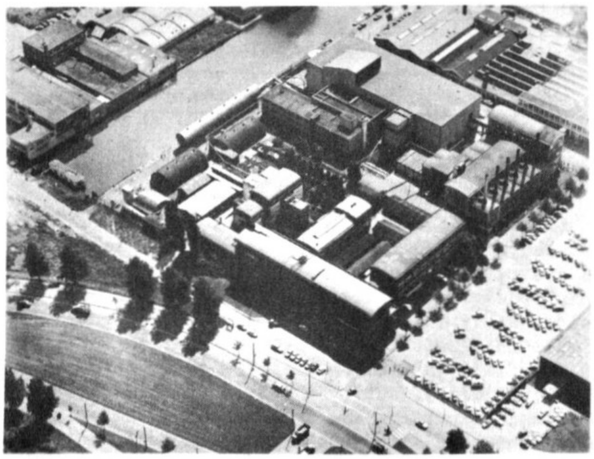

Fig. 3. NLR location Amsterdam.

Amsterdam location of NLR, most conspicuous being an integrated system of low speed, transonic and supersonic windtunnels of large dimensions. It will be clear that such facilities are not continuously required by the national industry, and in fact a sizable share of total capacity is run on behalf of foreign aircraft projects. Partly motivated by the support that was felt required for the successful operation of these experimental facilities, but chiefly by the importance of building up know-how in aerodynamics in its own right, there is a group concerned with theoretical aerodynamics. This group was newly formed practically from scratch from 1959 onwards, after several prominent researchers had succumbed to the continual brain-drain exercised by the Dutch universities on the laboratory staff. We will be following the considerations of this group, which at no time exceeded 15 people including programming staff, in the years 1960-70.

The broad research aims of this group can be stated as follows : to develop and make available a body of knowledge in theoretical aerodynamics, of current or potential interest to the national industry and users; and where necessary supporting the development of experimental techniques.

Another function of research is surely to help open lines of communication with research institutes abroad working in the same field. When the subject is how a research programme is shaped, it is necessary to mention the exchange of ideas in many international contacts; especially the discussions, sometimes developping into officially channelled cooperation, with our British colleagues in several institutes.

3. Methodical developments : resolution versus complexity

The basic physical principles of fluid mechanics have been laid down a long time ago, and on the fundamental front progress has been steady but very slow ever since. However, we are approaching this subject from the engineering angle, which means that one is not primarily concerned with building up theory from first principles, but with constructing mathematical models for the flow, having an experimentally validated applicability in certain circumscribed areas. One then must measure progress on a different scale.

The years 1960-70 can from one point of view be characterised as the period in which the computer became really operative in aerodynamics. That this is certainly true is testified by fig.4, a plot of the development of the daily equivalent computer time consumption by our group in this period. However, this aspect is not particularly illuminating, as the interest is surely not in the fact that, but in the manner in which computers were employed. Let us try to bring this into focus.

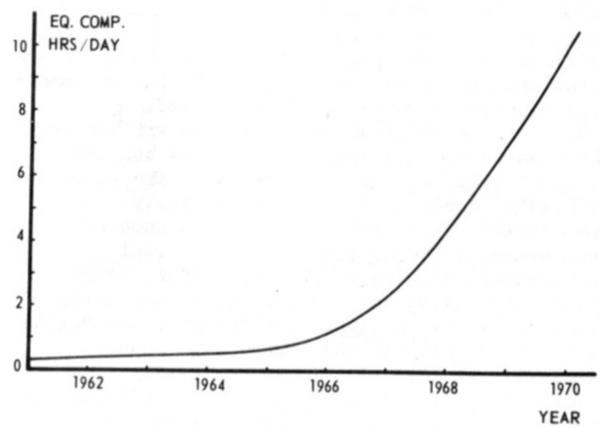

Fig. 4. Computer time used for aerodynamics at NLR.

Some years ago, the great physicist Eugene Wigner gave a talk on : "The unreasonable effectiveness of mathematics in the natural sciences " (ref.5). In aerodynamics one has not the feeling at all that mathematics is unreasonably effective; on the contrary, mathematical methods are effective only when the flow has first been taught to behave, by careful design. This is why model experiment will continue to be part of the aerodynamical design process in a foreseeable future : the mathematical models for inviscid flows represent in a sense the design ideal that is reached only after unwanted viscous flow phenomena have been successfully suppressed.

However, the obvious drawback in aerodynamical model experiments is that the diagnostics are rather unwieldy. It is often easy to see that something unwanted is taking place in a windtunnel, e.g. by analysing force measurements, but it is usually very difficult to make out what is going on exactly, and what to do about it. One has to localize the trouble area, and then by bringing in further instrumentation, more precisely analyse the flow phenomena, before being able to take remedial action. Now the possibilities to do all this on the scale of a highly complicated aircraft model are clearly restricted : internal instrumentation is limited for reasons of space, external instrumentation disturbs the flow; and even if one can see enough there is still the problem what to do. One can - and does of course - magnify the scale of the phenomenon under study in a separate model experiment, but then the relation with the original determining environment is cut; one can this way perhaps improve the understanding, but only very indirectly the control of the original situation.

We summarize all this as follows : in the experimental approach, the flow field originally is given as an unstructured whole. By increasing instrumental resolution, one expects to find the substructures, i.e. uncover the flow field's complexity. Seen from this angle, the art of design in aeronautical engineering has the same aim as in scientific experiment, namely to reduce complexity in their objects, i.e. suppress non-relevant sub-structure.

Now let us see how one stands in this respect in mathematical theory. When one is given a solution in classical mathematical physics, say as an analytical expression solving a boundary value problem for a partial differential equation, the resolution is in the first instance infinite, or rather, inversely proportional to the numerical precision : at every point in the domain of definition a numerical value is defined. However, if a solution can really be written down, the chances are the complexity of the boundary involved will be very low, otherwise the analytical expression would have been prohibitively complicated. This explains the prevalence of boundaries of low complexity in classical mathematical physics : spheres, planes, half-planes and cones; and the success of conformal transformations in plane flow theory : one begins with transforming to the figure with lowest complexity of them all. (I am not really attempting a precise definition of complexity for geometrical figures; cf. a recent paper by Van Emden, ref.6).

One of the important recent developments in applied mathematics, the method of matched asymptotic expansions (ref.7) is precisely concerned with this situation : the original structure is cut down to sub-structures of lower complexity, and local solutions are then constructed; these are later pieced together in an analytical process getting soon too involved for comfort.

Now a basic problem of applied mathematics is : can we handle situations of greater geometrical or physical complexity, by lowering resolution in our model, putting up a comparable computational effort ? One is, of course, usually readily prepared to sacrifice resolution in mathematical models : unlimited accuracy is of limited applicability in nature. It is clear that here is the mirror image of the experimental position, and this is why experimental and mathematical models are so nicely complementary.

All this introduction has been necessary to be able to state in a few words what has been happening in aerodynamics in the 1960s : several novel ways have been developped to increase the geometrical or physical complexity of mathematical models, at the expense of the resolution. Let us list what these ideas have been (fig.5). First of all, consider the classical way of lowering resolution : the method of small perturbations.

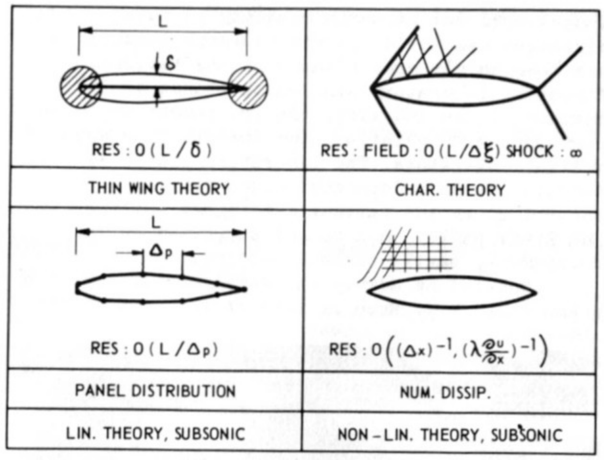

Fig. 5. Resolution and complexity in aerodynamic theories.

In thin wing theory, one does not distinguish between points on the given aerofoil contour, and on a straight line connecting the end points. On this reduced structure of low complexity, one quite easily solves the flow problem in terms of a simple integral. Now for every specified point in the field this integral gives a value for the flow quantities, but on the strength of the original assumption one cannot locate the position where this value applies more accurately then an aerofoil thickness : we have blurred our mathematical optics to this extent, and gained considerably in computational simplicity. This turns out allright as long as nothing much happens over distances comparable to an aerofoil thickness, but this is obviously untrue near the ends of the aerofoil; and in thin wing theory, things go violently wrong there. Now this can be remedied, and also resolution increased if one likes, with the aid of the method of matched asymptotic expansions; but at every step, the theory gets increasingly complicated. Analytically, the situation gets hopelessly out of hand if one is forced to represent the basic skeleton as an object of higher complexity : in the sequel we will meet an aircraft, "seen" as an infinite cylinder cut by an infinite line vortex : the analytical solution is incredibly complicated.

Compare all this with the "panel-method", represented also in fig.5. We begin by looking at the aerofoil as an object of higher complexity, being built up from a number of straight line pieces, "panels". From the point of view of analytical computation required, the increased complexity does not matter, as all the panels, carrying elementary singularities, are separately treated, and the results summed afterwards. The shape of the object comes in as the solution of the flow problem is found as a matrix equation, which one could not solve by hand, but quite easily in a computer. Obviously the resolution is now inversely proportional to the panel size *), but - this is the critical point - to increase resolution one just increases the number of panels.

*) There are some problems here locally.

raising the amount of computation required, but not the complication of the method. There are also no difficult areas, and this means that when increasing complexity of the basic structure, even going over to three dimensions, one meets practical, but no fundamental problems.

Next consider another modern development, the non-linear method of characteristics for supersonic flow (fig.5). This is a numerical field theory, and here the resolution is inversely proportional to mesh width in the field. The basic situation is, however, already rather complex : there are two connected structures, the aerofoil and the shock system. The resulting difficulties are here overcome by virtue of the localizing properties of the governing differential equations. However, in three dimensions, the complexity of the situation threatens also to become prohibitive. Here then is another method to reduce the complexity of the basic flow model. One can rig a numerical computation scheme in such a way, that it smears out gradients (ref.8). The resolution then depends on the local flow properties, in particular shock discontinuities are smoothed out as steep gradients, giving locally a blurring to the extent of several mesh widths. The important thing is, however, that shock waves are not recognised within the mathematical model as a separate structure. The method has been recently very successfully applied in transonic flow theory (ref.9).

In summary, then, what has been happening over the last decade is that mathematical methods were developped, in which the resolution could be lowered in such a way, that theoretical flow models of much greater complexity now could be handled. In fact, due to the increase in computer power, mathematical models could be constructed, that were comparable in geometrical detail with the ordinary model experiment such as used in aircraft development testing, instead of with special low complexity experiments only. On the other hand, the greater computing capacity could alternatively be used to increase resolution and obtain more physical detail in the flow model. This is the methodical balance, that had to be struck in any aerodynamic programme developping in this period. We will now see how this worked out in NLR's case.

4. Programme development : innovation versus consolidation; concentration versus coverage

4.1 Development of programme. - A survey of the development of the programme under discussion, with the family relation between diverse projects drawn in, is presented in fig.6. The projects have been classified as to their belonging to subsonic, transonic or supersonic flow theory; and with respect to motivation as being of an exploratory nature, or as being directed towards practical application in aircraft development.

In applied science, all projects are of course "development directed", but some are more so than others. In the category "exploratory work" the motivation was to explore the possibilities of methodical and/or technological innovation, that on the longer view could be of potential interest in aircraft development. Here a somewhat more distant view is permitted, but also a higher claim to originality required. There are two

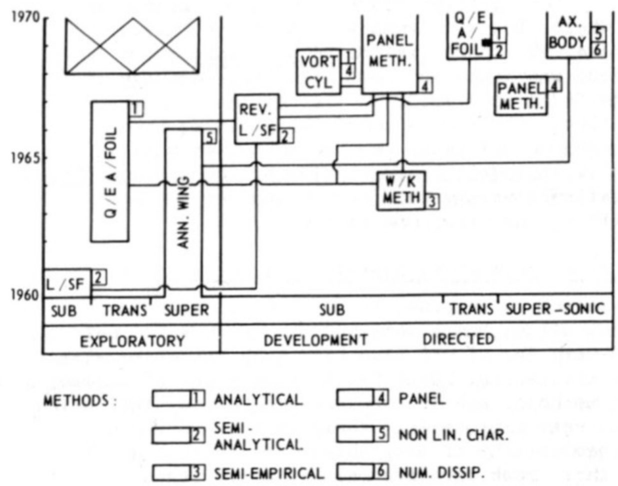

Fig. 6. NLR research programme development, 1960-70.

Legenda

L/SF : lifting surface theory, cf. ref.10.

REV L/SF : revised lifting surface theory, cf. ref.10.

Q/E A/FOIL : theory of quasi-elliptical aerofoils, ref. 9.

Q/E A/FOIL* : extension of quasi-elliptical aerofoil theory (to be published)

ANN WING : annular wing project, ref.11.

AX BODY : theory of three dimensional supersonic flow around axi-symmetric bodies (to be published).

VORT/CYL : vortex-cylinder model, cf. ref.10.

W/K METH : NLR-version of RAE-method, R.A.S. TDN 6312.

PANEL METH (sub) : NLR panel method, cf. ref.10.

PANEL METH (super) : NLR-version of BOEING-method, ref.13.

reasons why in an applied science laboratory, this category of work is essential. First of all, this is where the new ideas for future work are supposed to come from; secondly it often sets a standard for the laboratory's methodical capabilities, e.g. in the development of programming techniques. All this is not necessarily less so in the category of work undertaken with a view to immediate practicability; but here the emphasis is different, and the consideration of keeping in front, or at least up with the state of the art as practised elsewhere is of greater importance than claiming mathematical firsts. It is arguable, whether the development of quasi-elliptical aerofoils or of a panel method requires the greater intellectual effort; the difference in classification only means that the results of the first were not originally planned to be applicable in aircraft (they were, eventually !), and exactly this is the panel method's only motivation.

Let us first comment on the programme as a whole. You will remark that in the initial phase, the concentration in the exploratory group was high, and that later the coverage of the field increased. On the one hand, this reflects the expansion of the group as the programme got under way; on the other hand it reflects a deliberate biasing of the concentration/coverage balance for the purpose of getting the programme started.

4.2 Consolidation; supporting activities. - Research is supposed to be directed towards innovation; but a research programme would not go very far if not also very much concerned with consolidation. There is the problem of efficiency of methods, and of computer programmes, that often can bear improvement; there is the problem of accessibility of programmes for others than the author. Both problems require attention and effort, but could be called secondary. However, a primary problem is the experimental, and the intrinsic validation of a new method. In theory, the latter is simple, involving just the construction of a mathematical error estimate. If ever this happens in this field, I for one would be very glad to hear about it; so one has to find another method.

As will be apparent from the published literature, validation both experimental and otherwise, has been a major concern of the group. Possibly the most spectacular ever example of such supporting activities is the application of the panel method to Lennertz' analytic solution of the incompressible flow around a vortex-cylinder combination, which involved actual computation of the original solution. This way, certain pressing problems of the optimal distribution of panels could be answered (fig.7), and it did appear (fig.8) that the most elegant way of arranging panels just spoiled accuracy (ref.10).

4.3 The annular wing project. - Now let us go into some detail of the three major projects within the programme.

In the years around 1950, the general theory of linearized supersonic flow was developped by several American and British groups. In the later years, the problem of minimizing supersonic wave drag received much attention, in particular two methods : minimization by mathematical optimization of solutions within linearized theory; and secondly, minimization by means of "favourable interference". The latter idea obtained attention from various workers around 1955 in the particular form of the "annular wing" concept. The theme was also taken up at NLR in 1956, and here is what they did about it (ref.11).

The basic idea is as follows (fig.9). Consider a symmetrical pointed body of revolution in supersonic flow. As is well known, within linearized theory momentum is transported towards infinity between the shock waves, resulting in pressure rise over the front and pressure fall over the rear of the body, and interpreted as the "wave drag" of the configuration. However, by adding a specially designed shroud (fig.10), one can screen off and reflect the outgoing wave pattern onto the rear of the configuration, giving complete pressure recovery and zero wave drag. Fig.11 shows a variation on this theme : a configuration with base area, cylindrical wing but specially designed afterbody to do the same trick. This is as far as the problem got shortly before 1960.

At that point, however, the critical observation was made that this whole way of going about the problem was intrinsically inconsistent. After all, the basic idea depended essentially on a very accurate placing of compensating effects, but in linearized theory as used here, one cannot really distinguish between points on the body axis and on the contour, so where is the accuracy ? In other words, for what we intend to do, the resolution of our theory is really insufficient. The only solution then is to sharpen focus, and use real non-linear theory. That the basic idea works there as well, is show in fig.12. The original (dash-line) body is one with base area, having minimal wave drag under certain geometrical constraints; a theory of such bodies in a non-linear context being one of the early successes of the new theory. However, by adding a thin shroud, 40 % extra body volume could be added, without increasing this minimum. However, there was one artificiality. As a result of the particular non-linear theory chosen, the bodies arrived at in this way always began with cusps, forming a shock away from the body, outside of the region that came in for theoretical consideration. What, if we again sharpen focus, and also want to bring shock waves into the picture ? One result is shown in fig.13, the body now starting of with a conical nose, producing a shock wave being partially reflected into the region between body and annular wing, the whole situation being optimized for minimum wave drag. With another refinement to bring vorticity in within sight, and a basis for the optimization of a lifting configuration laid, the theoretical part of the project was stopped around 1963, with only an almost perfect experimental validation to follow (fig.14). Why this, after all the theoretical successes ?

In practice, one has of course not only to worry about wave drag, but also about friction drag. Now what we do are doing is to add an amount of extra surface to diminish wave drag, but when a realistic lift/drag balance is worked out it appears that the resulting additional friction drag produces lift-over-drag values that are interesting in a very small speed range only, as compared with the convential delta-wing arrangement. Perhaps even more fatal is the fact that drag is optimized sharply at one flight Mach number, and what one wants is low drag over a wide range of conditions. So the world's airlanes were not going to be filled after all with supersonic annular wing aircraft designed by NLR; this much was of course clear already after the preliminary studies. Still the project was continued for some time, because at that stage of programme build-up, the theoretical spin-off in this advanced project was what we were really after.

The know-how gained was used some years later in work on three dimensional supersonic flows around bodies, in support of Dutch commitments in the ELDO project. Here a hybrid method was developped, giving high resolution near the front shock, and a lower one in the field; this method giving healthy looking results as shown in fig.15 (unpublished).

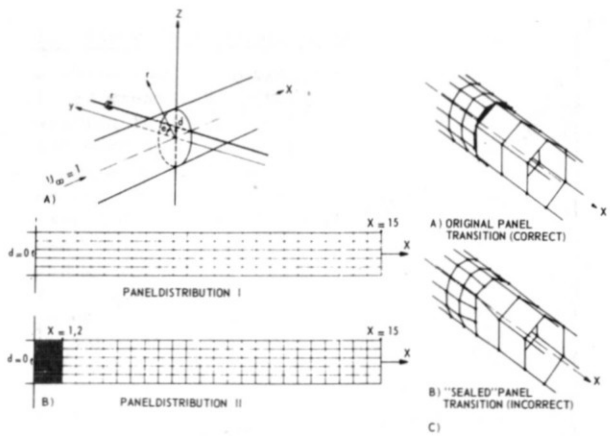

Fig. 7. Panelling of cylinder-vortex configuration.

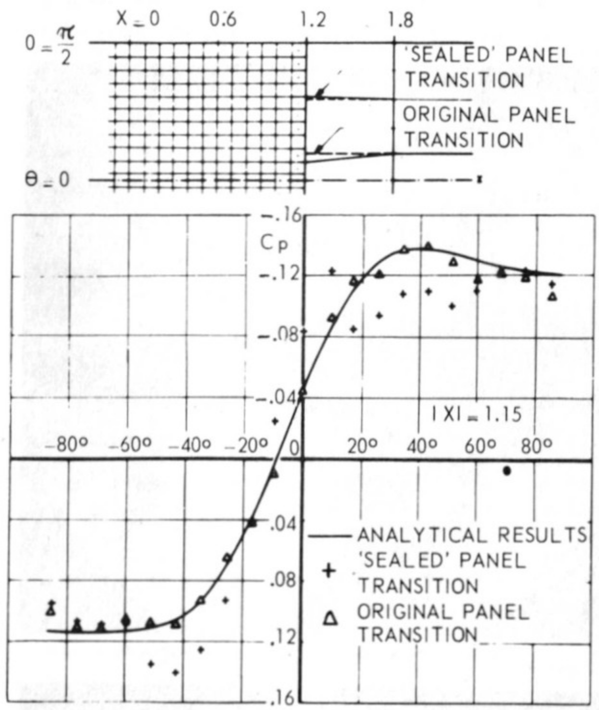

Fig. 8. Panelling of cylinder-vortex model, pressure plot.

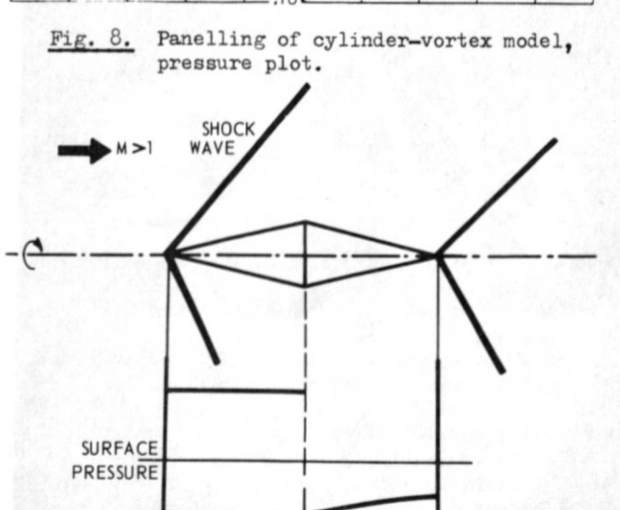

Fig. 9. Wave drag of body in supersonic flow.

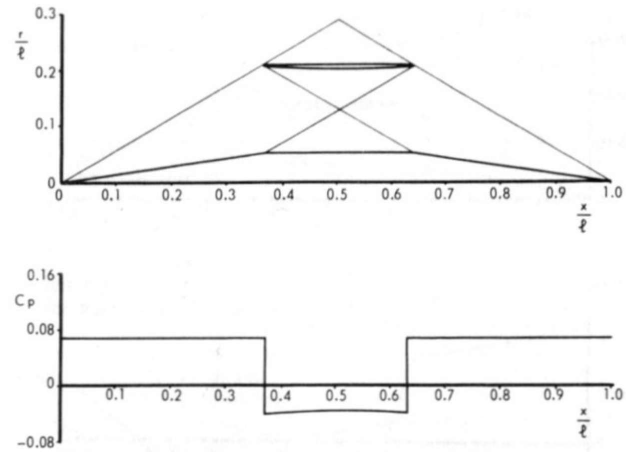

Fig.10. Axially symmetric body with zero wave drag; linear theory, $M_\infty = 2$.

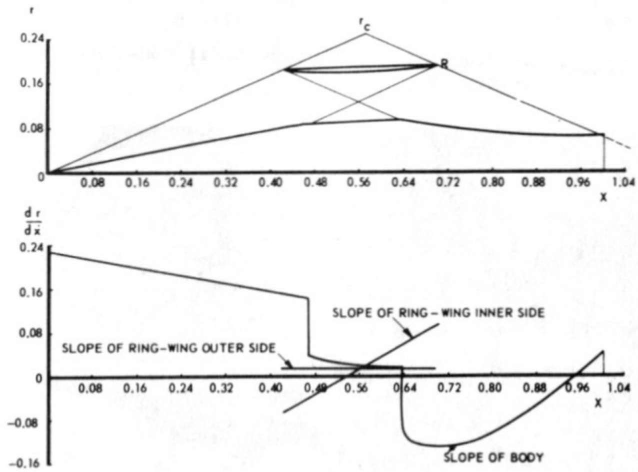

Fig.11. Configuration with base area; linear theory, $M_\infty = 2.5$.

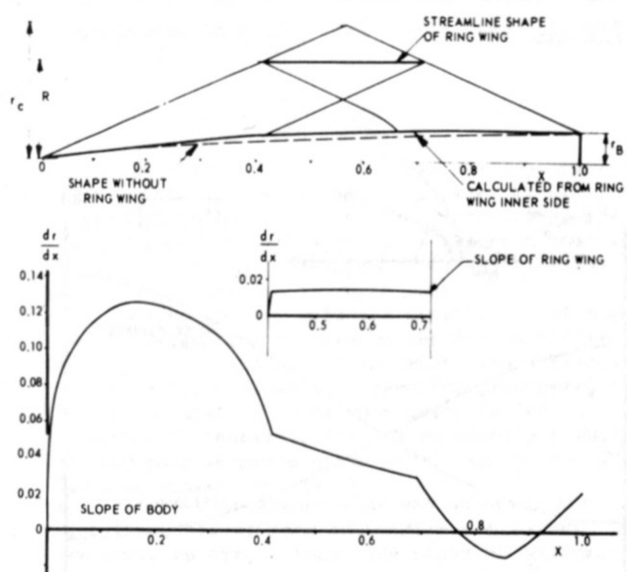

Fig.12. Decreasing wave drag by addition of annular wing; non-linear theory, $M_\infty = 2.5$.

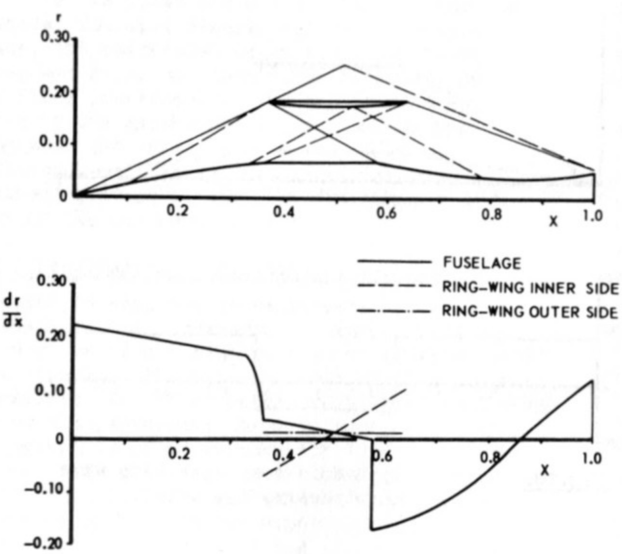

Fig.13. Configuration with base area; non-linear theory, $M_\infty = 2.5$.

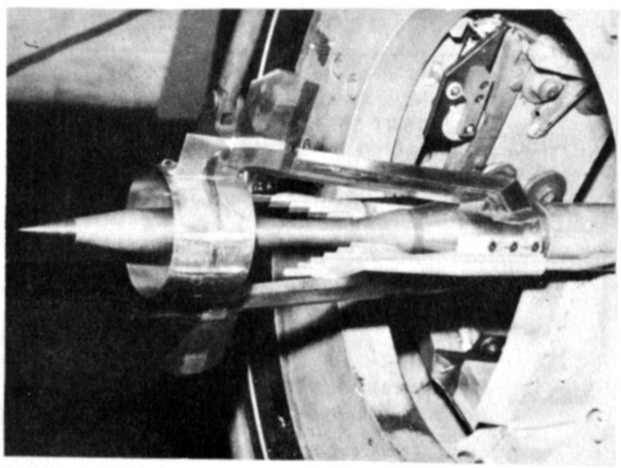

Fig.14. Test of low-wave-drag configuration.

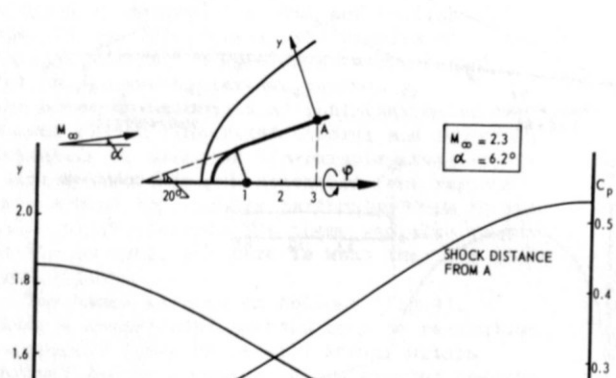

Fig.15. Flow field about inclined body of revolution.

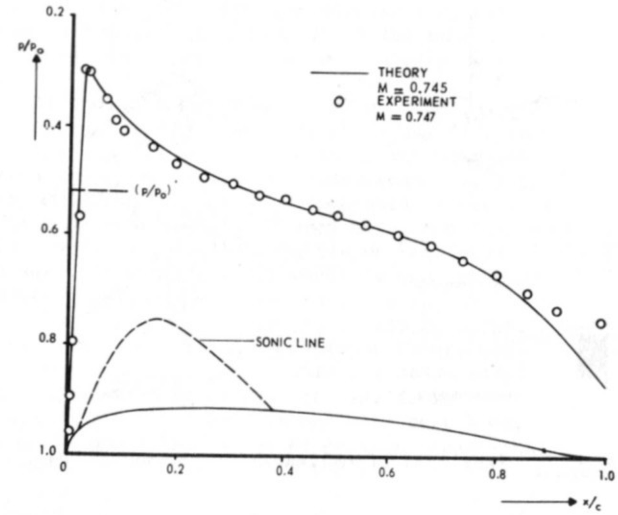

Fig.16. Symmetrical quasi-elliptical aerofoil.

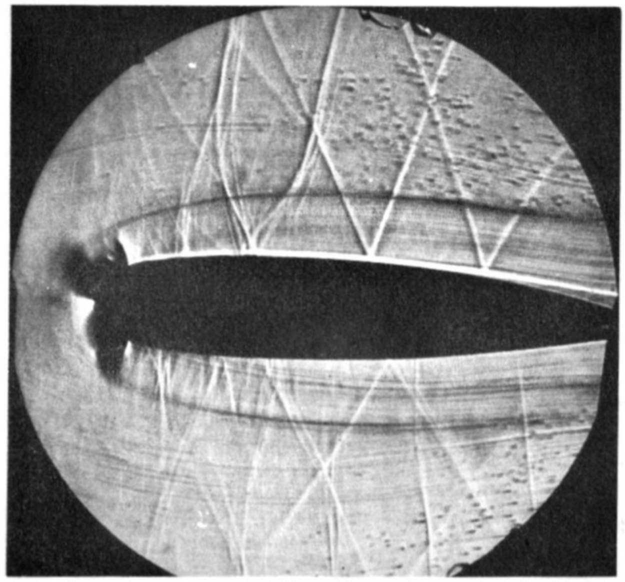

Fig.17a. Wave propagation through transonic flow.

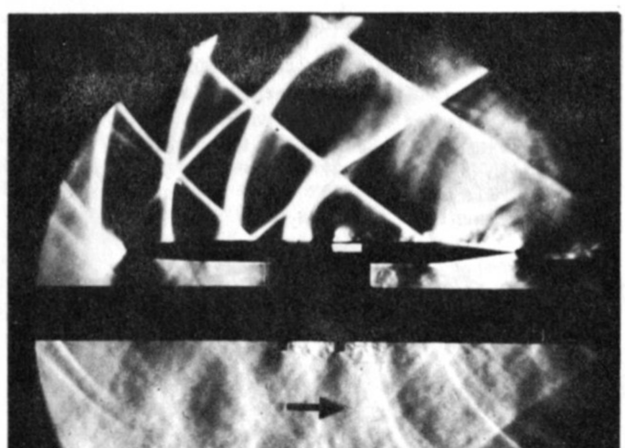

Fig.17b. Wave generation by cavity flow.

4.4 Shock-less transonic aerofoils. - Around 1955, a whole body of theoretical literature, some of it of great mathematical sophistication, was written to explain why certain theories nobody had ever applied in real situations, but presumably yielding transonic flows without shocks around aerofoils, could not ever be expected to work. One of the reasons advanced was that in such flows, weak time-dependent disturbances would come to a standstill, coalesce, and let the basic flow collapse into one with a shock. In 1961 at NLR, we began being interested in the application of difference methods on subsonic, eventually on transonic flows. It was thought that to this end, comparisons with exact subsonic solutions would come in handy, but these were not then available. It was decided to construct such solutions on the basis of the challenged theories in the subsonic case; and perhaps have one transonic example computed to watch the collapse in a wind tunnel, expecting to observe an interesting experiment. Soon it became abundently clear why nobody had cared to carry out computations : the theories involved were either not applicable in realistic situations, or not correct there, and gave rise to tremendous computational problems. These were duly sorted out, first results were given in 1964, a completely revised theory announced at the 1966 ICAS Congress, and definitive results published in 1967 (ref.9). From one point of view the transonic experiments were disappointing. Nothing happened, i.e. experimental agreement with theory was near perfect (fig.16). In 1966, when this particular experiment was run, this was no longer a surprise. Herbert Pearcey, then at the National Physical Laboratory, had around 1964 experimentally developped shock-free transonic profile flows. The theory gave rise to an interesting experimental study (fig.17) of the stability of these flows, in which even not-so-weak travelling waves were shown to fail in blowing up the flow pattern (ref.12).

Experimental verification for lifting flows was obtained early in 1969 (fig.18), here of course boundary-layer effects are more prominent. From a practical point of view this is not a particularly good aerofoil, there being too little loading over the rear. Such aerofoils were theoretically produced by a competing theory of the same scope developped around 1969 at New York University, the rear-loaded aerofoil being found in a somewhat involved iteration process. As the significance of shock-free designs for aerofoil development was by then amply shown, the transonic aerofoil project was restarted in 1969 as a purely development directed effort. The first aim was to develop a semi-analytical modification procedure, which would add rear-loading to the original analytical aerofoils. This idea proved completely successful (fig.19). The second aim is to be able to prescribe general aerofoil characteristics within the theory, without having recourse to involved search operations. Here very good progress is being made.

To my knowledge, this very particular physical situation is one of the few cases *), where high theoretical resolution has really paid off in engineering aerodynamics. At the same time, it is probably the last application in this field of the classical analytical methods. Concentrating on this, we did miss exciting new developments in the application of finite difference methods in transonic aerodynamics (ref.9), our original motivation.

*) there are always nozzles, of course.

4.5 Panel methods. - The NLR work on the subsonic aerodynamics of three-dimensional aircraft configurations is not primarily intended as an initial design tool, although among other uses, it can certainly be used as such. The idea is rather to be able to offer an analytical instrument to wind tunnel users, complementary to the experimental techniques. In modern design practice, aerodynamic loadings have been continually increased, pushing flow conditions further against their limits, making viscous break-down of increasing concern to the designer. As explained before, refined wind tunnel instrumentation is essential but limited in scope. On the other hand, the theoretical models are yet also of limited applicability, being of no use in either the low speed, high lift condition, or in the transonic cruise condition. However, there is a large range of conditions, in which theoretical methods can very much increase the efficiency of wind tunnel use.

Work in this direction started in 1964, when a semi-empirical method based on work of Weber and Küchemann at RAE was made available to NLR in the context of an Anglo-Netherlands cooperative programme. This is still available as a very simple and cheap quick-look method, but what we learnt from it in particular was the art of empirical compressibility correction, which is still part of the more recent methods. During 1967, a hybrid method was developped involving A.M.O. Smith's pioneering work in the panel method for the thickness part, and lifting surface theory for the lift part of the problem. In 1969, the full panel method was arrived at, based on the principle of fig.20.

An application, to me still spectacular, is shown in figs.21 and 22. Let me end this survey with the remark that in 1967, also the supersonic panel method as developped by Woodward & Carmichael at the Boeing Company, was implemented at NLR computers (ref.13).

5. Epilogue

In this lecture an attempt has been made to survey part of the aerodynamical work of NLR from a particular point of view : objectives, choice of methods, and the balances to be observed therein. Clearly, I am now committed to extract a moral to the story, never an enviable task.

First of all, with the benefit of retrospect, it is always easy to arrange results in pattern, then give an interpretation. Much more difficult it is, being engaged in a research programme, to decide on what is to be done next; in the dynamics of research, inertia is usually high, but straight-on segments often short. Again, how do we balance, how do we plan ?

In the programme that was selected for scrutiny, the effects of inertia are indeed evident, as are, I hope, the signs of control being applied. This is not to say we had a PERT-scheme telling us in 1962 what we would do in 1967. But within the bounds of NLR's usual planning mechanism, certain themes were clearly pursued for some considerable time, others

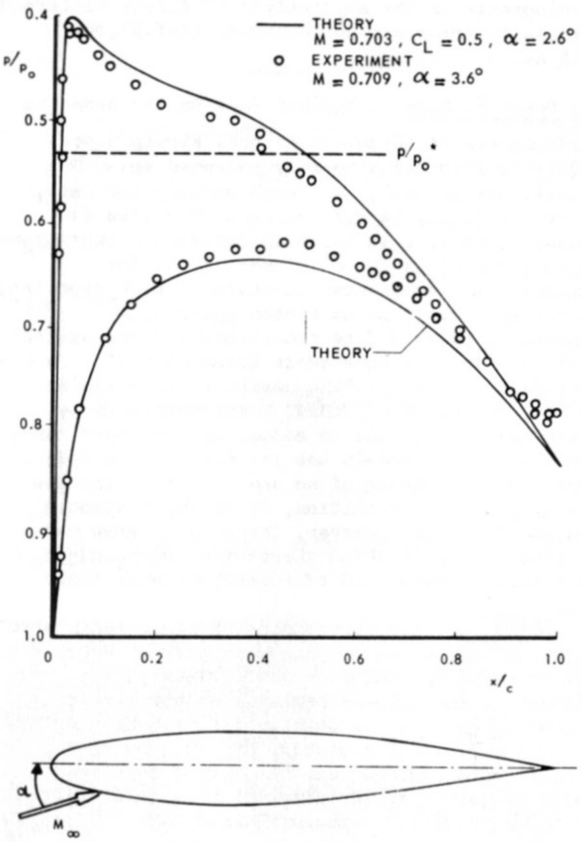

Fig.18. Lifting quasi-elliptical aerofoil.

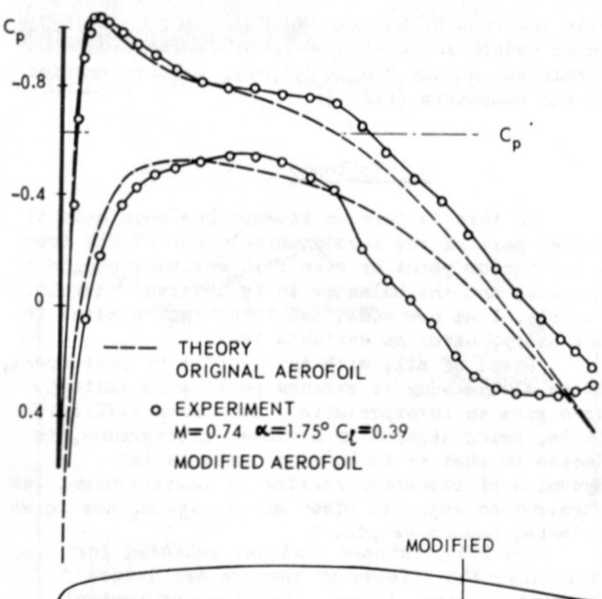

Fig.19. Modification to increase lift.

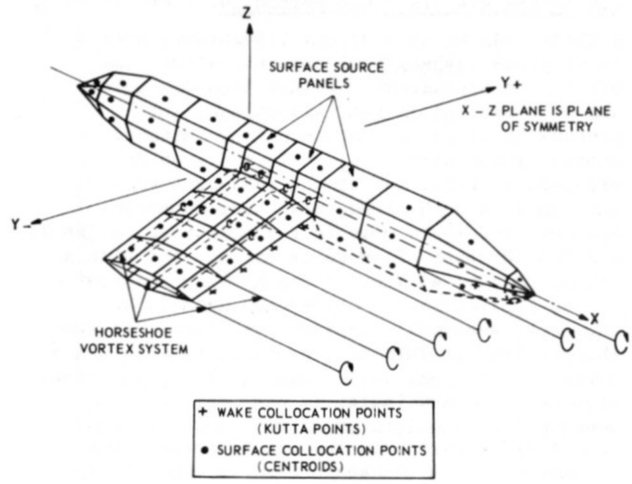

Fig.20. Application of panel method to non-planar configurations.

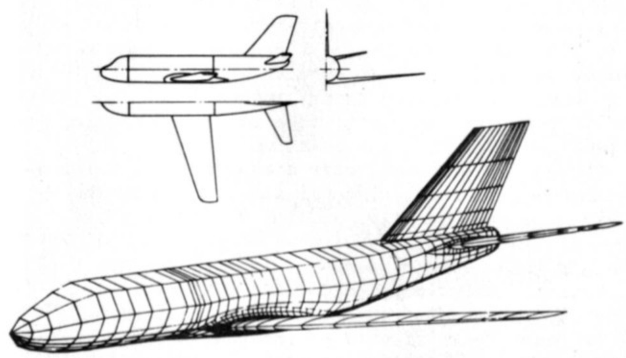

Fig.21. General arrangement and panel plot of wing-body-tail configuration.

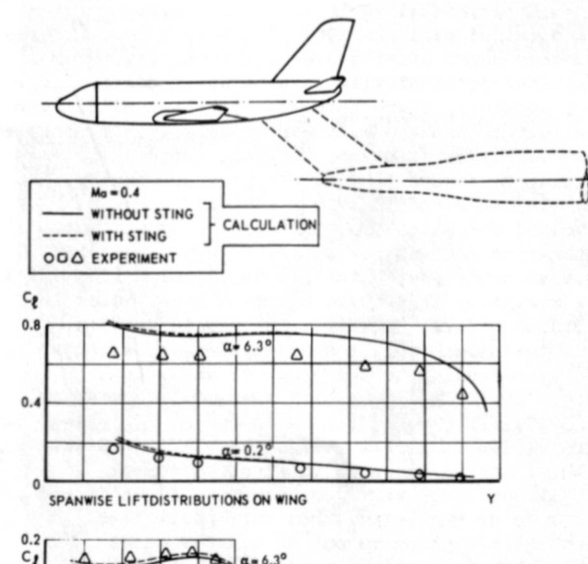

Fig.22. Results for wing-body-tail configuration.

discarded. How were these balances struck ?

In the introduction we met two extreme planning schools of thought, the believers in the inspiration of genius, and in the divining powers of computers. Now the number of geniuses on the NLR staff has traditionally been low, and our computers were usually filled beyond their limits such as they were. Moreover, to the proponents of the latter school one could offer for serious consideration the general history of mathematical wave drag optimization in aerodynamics, as an example what happens when optimizing in too restricted a universe.

Our method of striking a balance, if so it can be designated, has been to systematically invite discussions about what everybody was doing, both on the home front and with the outside parties concerned. Everybody professing a flash of genius could go along, within budgetary limits, and as long as he could explain to the others of the group what all this was supposed to be in aid of. The merits of the results of this simple approach are for you to decide, but it certainly improved working conditions. There may be a moral here for other planning situations, where of course computers may well be part of the system - held in careful check, as in science.

However, what was discussed were problems in the institutional context, but the planning issues of real consequence are debated on the higher levels. How is aerospace research to react on this, if at all ?

As one who has left the field for several years now, and who carries no further responsability there, I can offer only one general observation, for what it is worth.

Over the last decade there has been a rise in the problems associated with the increase in volume and intensity of air transport : problems of noise and pollution, air traffic control, airport location; also structural problems have become evident in the economics of aircraft production. Expectedly, in future many of these problems will yet intensify; on the longer view also the world energy situation has to be added to this list: after all, many competent observers estimate that conventional fuels may become less readily available than at present upon a term, differing less than an order of magnitude from the development time of today's conventional aircraft. However this may be, in the immediate future problems will surely abound, especially in very densely populated areas. My contention is that these problems should be considered structurally, as the integration problem of the air transport system with society's other subsystems. Then there is certainly an optimization problem here, which ought to be scientifically approached : not to maximize air transport, but to optimize the transport system as a whole. This evidently requires a multi-disciplinary approach, including technological, economical, sociological and regional planning expertise. The problem is who is going to do this exercise.

Most aerospace laboratories are organised like the other big-science establishments : along the lines of technical disciplines, and around the big hardware. This is reasonable, but makes perhaps these institutes less well equipped and affiliated, then would be required to diversify into the business of producing research tools for policy-making on the national levels, and beyond. However, there are two reasons why inclusion of this problem in the aerospace laboratory's research programme should be seriously considered. First of all, among all research institutes concerned with transport, only these have the scientific tradition and the scale of operations required in this problem. Secondly, there is a historical motivation. After the initial phase, when the art of flying at all was the problem, flight research has increased its scope, and became subsequently and cumulatively concerned with safety, economy, operations and environmental problems - just because these areas became of major concern to the business of flight. In my opinion, the integration problem as stated has by now become of just such major concern.

Finally, let us complete the full circle, and finish on the note we started on. Perhaps more than anything else, aviation has made the world we are living in, a small one. This perspective has recently been dramatically confirmed by space flight. Both technologies, paradoxically, have helped us realise that our real problems are in fact quite down-to-earth, and both can help us solving them. Now let us get on, and act on this discovery.

Acknowledgements

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