The relationship of research to decisionmaking in intercontinental traffic

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INTRODUCTION

The topic which I have been asked to deal with - "The relationship of Research to Decisionmaking in Intercontinental Transport" is not one that I chose myself, and I must say that I had considerable difficulty in trying to respond to it.

The easiest part of the title is clearly "Intercontinental Transport" which seems to limit the scope to long-distance aviation and ocean shipping, putting aside the aberrations of the odd intercontinental railway, highway, inland waterway and even pipeline. I gladly accept this limitation. More specifically, intercontinental transportation naturally breaks down into three modal components - aviation, general cargo shipping and bulk shipping; I distinguish the two shipping modes because of their very different structures and technological development paths.

The more difficult parts of the title are the terms "Research" and "Decisionmaking", and I must say I struggled with these for some considerable time. The definition of research I am most sympathetic to is taken from a well-known definition of economics - "Research is what researchers do". This seemed to describe perfectly my own experience with research and, even more so, researchers. I was recently somewhat disconcerted to come across a definition of research attributed to Wernher Von Braun, most recently of NASA fame, that seemed similar yet was very defferent. His definition was "Research is what I am doing when I do not know what I am doing". The Von Braun definition suggests a useful classification of research into three categories, in increasing order of specification:

a. Non-objective - or abstract non-directed inquiry; the research referred to by Von Braun.

b. Objective non-specific - such as trying to find a means of improving some particular transport vehicle or facility.

c. Objective specific - such as tackling some very specific well defined problem.

This does help in defining my topic further. Transportation research has not been characterized by "mad scientist" types pursuing abstract non-directed inquiry. Furthermore, the objective specific research task - the nuts and bolts of improving a product - which accounts for by far the largest research effort, is too microscopic and detailed for much to be said in this type of presentation. By elimination, I am left with the second category - objective non-specific research - as the focal point of my comments, at least to the extent that they deal with technical research.

This is made all the more complex by the fact that research itself runs the gamut from technical (or engineering) research to market research, to systems research and to socio-economic research, recognizing, of course, the increasing role of interdisciplinary research cutting across traditional lines.

Decisionmaking implies decisionmakers and there are a variety of decisionmakers who are impacted by research. They include:

- Owners/Operators of mobile and fixed systems
- Manufacturers of transport equipment and facilities
- Government officials involved in transport policy
- International agencies and other international groupings involved in intercontinental transport
- Transport users.

I am afraid I will have to deal with all these decisionmakers.

The more I tried to precisely define my topic, the more complex it seemed to become and I did not think it useful to prepare a paper dealing with definitions or taxonomy. Therefore, I have interpreted my topic somewhat broadly and taken as my theme the broad relationships between research and change in long-distance aviation, general cargo shipping and bulk shipping. I was encouraged to do so by the fact that the presentations which follow mine are all mode-specific.

I should like first to deal with the broad macro relationship between research and change over the past two decades in each of the three modes I have identified and to explore the interactions and driving forces. I have concentrated on mobile transport equipment, rather than on fixed facilities, on the theory that fixed facilities tend to respond to perceived needs arising from changed mobile equipment. I would finally like to probe the deficiencies in research performance and explore what the future may hold in store.

THE LAST TWO DECADES

The only way to judge the effectiveness of research is pragmatically - by what is has achieved. It is well and good to publish learned papers, to build impressive research facilities and to appear at prestigious international conferences. But the usefulness of all this is nothing if it does not lead to improvements in the transport system. The proof of the pudding is in the eating, not in the recipe or in the cooking.

By this standard, one must conclude that research has served intercontinental transport very well indeed over the past two decades. Those of us who spend our lives working in transport are perhaps too close

to the changes to fully notice this. But the progress in the past two decades has been nothing short of remarkable. Any balanced look at the effectiveness of transport research must begin with this in mind. The intercontinental transport system today is a remarkably superior one to that which existed 20 years ago, both with regard to cost and to service. In this most fundamental sense, research has served the industry well.

It is useful to recount briefly the major developments which have occurred.

In long-distance aviation, we have evolved from the DC-7, Constellation and Britannia to the Jumbo and perhaps even the Supersonic. The change in technology, in cost levels and in service standards has been nothing less than remarkable, and the fascination of the story is lost in any short summary. Suffice it to say that the successful development of long-distance aviation will be one of those few basic developments which future historians will attribute to our generation.

With regard to ocean shipping, general cargo handling has evolved from the small, slow multi-purpose general cargo vessel, spending perhaps half its time loading and unloading by methods best characterized as medieval, to the large, fast automated container, Ro-Ro or LASH vessel, capable of being loaded and unloaded in perhaps 24 hours at highly automated ports. Bulk shipping has evolved from the 10-20,000 dwt. tanker or bulker to the flotilla of 250,000 dwt. VLCC's, with some even larger, and 120,000 dwt. dry bulkers, all capable of rapid loading and unloading and literally scraping the bottom in most of the world's traditional ports and channels. These changes are perhaps less dramatic than the changes in international aviation, not only because they are less visible but also because they have not changed the lifestyle of the world as much. But, nonetheless, they are changes which have fundamentally transformed ocean shipping (and hence world commerce) and are certainly the most dramatic since the demise of the sail. Indeed, in comparing the evolution of *inter*continental and *intra*continental transport over the last two decades, it is clear that the changes which have occured in intercontinental transport have been by far the more dramatic and significant. This is by no means intended to disparage developments in automobile, bus, truck, rail, barge, pipeline and short-haul aviation, which have not been insignificant. But in sum total they are certainly overshadowd by developments in intercontinental transport.

HOW HAS THIS HAPPENED

In trying to understand the relationship between research and decisionmaking, it is interesting to explore the mechanism and driving forces whereby dramatic and far-reaching developments occurred in each of the three intercontinental modes. One would be hopeful that some general conclusions could be derived from such a review which might shed some light on what we might expect in the future. I am sorry to report that the only conclusions which emerged were the most general ones and not the least bit surprising - namely that each faced its own set of external and internal pressures and driving forces, and that the developments of each were uniquely related to the specific circumstances of each mode. Expressed more positively, the general theory of the development of intercontinental transport is that there appears to be no general theory. Research responds to pressures, but to a variety of pressures, and progress is the result of the complex interaction of internal pressures, exogenous pressures, technical developments and motivation.

I should like to develop this thought further by considering briefly the developments in each of the three modes, paying particular attention to:

1. The nature of the changed technology
2. The roles of each of the three major players - the manufacturers, the owner/operators, the ultimate users.
3. The driving force for change.
4. The location of the entrepreneurial thrust.
5. The propagation mechanism for the spread of new technology.

Consider first international aviation, the youngest of the modes with an effective birth date after the Second War. Of all the modes, the technological changes in international aviation were the most dramatic, fundamental and far-reaching. But they did not result from dissatisfied users, be they passengers or shippers of cargo, clamouring for better service and/or lower prices, nor from the owner-operators urging the manufacturers to improve their products. Both the users and the owner-operators were too fragmented, too weak financially and too ill-informed technically to have much of an impact.

Rather the driving force for change came from the manufacturing industry - to be more specific, a handful of U.S. aerospace firms - which aggressively packaged the spillover of military R & D into commercial aviation products and then aggressively marketed these products to the owners/operators.

This was clearly the location of the entrepreneurial thrust and drive and remained so even when the spillover effedt from the military side became less important. One is tempted to generalize and say that the pattern whereby the entrepreneurial drive is located at the manufacturing level seems to be true, in general, of the high-technology industries.

The propagation mechanism for spreading the new technology was also interesting. The new products were first purchased by one, or a handful, of airlines and as the advantages in terms of both better service and lower unit costs became obvious, they were followed helter-skelter by the other airlines wishing to maintain their competitive position. It is interesting to note that all *successful* new products featured reduced unit costs,, and the competitive nature of the airline industry took it from there. As an aside, this propagation mechanism explains the waves of re-equipping that have periodically swept the industry and also the persistent oversupply of equipment.

The user of international aviation, both passenger and freight, was an enthusiastic responder to lower real cost coupled with improved service. Indeed, his response was so enthusistic that it added fuel to the flames, furher encouraging the manufacturer to push ahead and the airlines to acquire new products. Only very recently has this process seemed to come to a marked pause. The dismal economics of the Concorde places it out of the mainstream of the story, since all succesful new designs have offered improved economics.

Consider next the developments in general cargo shipping over the past two decades - the so-called container revolution. In total contrast to international aviation, the basic technological changes which were required to make the revolution happen were fairly simplistic, and I have no doubt that a good naval architect working with an equally good production engineer could have put together the basics of today's container handling system in the 1930's, if not the 1920's. The changes were technically simple and in no way pushed against the technological frontier.

When they did finally occur, once again it was not because of pressure from users demanding better service at lower cost; the users were too fragmented and too small individually to initiate change. Nor was change promoted by the manufacturers, the ship-builders, who seemed quite content to carry on producing traditional type vessels forever. Rather the driving thrust for change originated from the owners/operators, faced with dramatically escalating labour costs, particularly at ports, with no scope to improve productivity within the confines of traditional technology. If cargo had to be loaded and unloaded by the traditional methods, port labour costs and port delay could not be reduced.

The driving force for change in this instance was a handful of entrepreneurial ship owner/operators who carried out the research, put together the skeleton of a marine container system and aggressively forced the traditional ship-builders to respond to their needs. And in the first instance, it was not the traditional, well-established owner-operators who developed and promoted the new technology. The lesson in this appears to be that longevity and tradition are poor bedfellows for research and development, and shipping is one of the world's oldest industries.

The propagating mechanism, once the new technology was seen to be remarkably cost-effective and workable, was emulation by the other ship owners-operators, so that within a span of very few years, the entire industry was transformed. The ripple effects of mechanized handling at ports is also interesting: mechanized handling of containers made possible reduced port times which improved the economics of larger and faster ships - all of which could have been built, at least technically - many years before the Container Revolution. Exactly the same is true for the dramatic feedback effects of marine containerization on the inland transport system. Inland transporters responded with a technology that could have been designed in the 1930's or earlier if there had been a need. Fortunately, when marine containerization was being developed, there existed the skeleton of an appropriate inland system in the use of semi-trailers for highway movement and rail piggyback handling.

As in the aviation case, the user of general cargo shipping was an enthusiastic responder to lower real cost coupled with improved service to the point that for many years the only dependable forecast regarding the market penetration of container handling into the general cargo trade was that all existing forecasts would be exceeded.

Finally, consider the case of bulk shipping, the least visible of the three modes under consideration. The technological developments which made possible the 250,000 dwt. tanker and the 120,000 dwt. dry-bulker lie somewhere between the "high" technology of aviation and the "simple" technology of container shipping. Fundamental developments in ship-design and power plants as well as in the strength and fabrication of materials were necessary for the new technology to emerge, but the changes in technology were still an order of magnitude smaller than the changes in aviation technology.

Of all the modes, bulk shipping is the most complex and difficult to understand with regard to identifying the driving force behind the technical change, the entrepreneurial group and the propagating mechanism. This results from the rather unique structure of the industry, with the participants playing multiple roles. Not only are users of bulk-shipping often also owner-operators (the large fleets of the major oil companies and of the iron ore subsidiaries of some major steel companies, for example) but users are often also manufacturers (the ship-building subsidiaries of several Japanese and U.S. steel companies) or closely linked to manufacturers (the Japanese Zaibatsu). All this reflects the fact that in bulk shipping, in direct contrast to aviation and general cargo shipping, the users are few, large, and economically powerful compared with the owner-operators and the manufacturers and there is hence considerable vertical integration.

The closest one can come to a simple and coherent explanation of the driving force and motivation for the change in technology, is to say that it originated in Japan, almost as a national mission. This was a mission with several purposes - to meet and minimize the costs of the country's projected needs for huge imports of raw materials, to maximize the country's manufacturing capabilities, to maximize the country's export capabilities. All these factors came together in a uniquely Japanese way reflecting its role as a user and as a manufacturer. The research and entrepreneurial roles were almost entirely Japanese.

The propagating mechanism, once the Japanese had put together all the pieces and shown that bulk shipping costs could be significantly reduced was a combination of very entrepreneurial owner-operators, responding to pressure from major bulk commodity users, all accelerated by the dramatic increase in international trade in petroleum and dry bulk products, particularly coal and iron ore, resulting from the basic facts of economic geography, and further reinforced by the closure of the Suez Canal. The surge in petroleum trade was exogenous to shipping technology - world shipping would have had to find some way to accommodate to the economic geography of petroleum - while the surge in coal and iron ore traffic was to a major extent a response to the availability of low-cost, long-distance bulk transport.

In summary, I have no difficulty in concluding that, in a macro sense, research has served intercontinental transport well over the past two decades in the sense that the transport product has become very much better and cheaper. But the relationships between research and decisionmaking have been different in all the modes, depending on the complexity of the technology, the economic strength of the various interests and the location of the entrepreneurial thrust. It is an interesting story.

I said before that there was no general theory of the relationship between research and development in intercontinental transport. This was perhaps an exaggeration, because there are a few common threads running through the story. One is that all the successful developments were cost-reducing and service-improving; this is not a very remarkable conclusion. A second is that the very competitive nature of all intercontinental transport modes made available a very effective and rapid propagation mechanism for the developments once they had been perfected. There is hope for those of us who still believe in the effectiveness of the competitive marketplace. A third is that effective research and development requires a dedicated sponsor, prepared to pursue new concepts in a single-minded manner. The sponsor can be the manufacturer - the case of aviation, the owner-operator - the case in general cargo shipping - or some combination of interests - the Japanese coalition of manufacturers and users in the case of bulk shipping.

The final common thread in all of this is that the role of government in the developments which have taken place has been minimal and what role there has been was almost wholly responsive. Government has

taken the major role in providing the fixed facilities for the intercontinental modes, but in a responsive manner, reacting to the developing technology of the mobile systems. Government enterprises have been participants in the intercontinental transport scene, in various roles, but generally they have acted as have the private participants; they have not been the driving force, with the possible exception of the Japanese and the well-known difficulty of clearly separating private and public motivations. Government through its regulatory function has had to permit certain commercial developments to take place, but has generally not been a major delaying influence. The key decisions have been made without government involvement and research has impacted private decisionmakers rather than governmental ones. Perhaps this explains some of the dynamism that has taken place.

THE NEXT DECADE

I have discussed developments over the past two decades and, to be consistent, I should use the same time horizon in looking ahead. I am sorry to say that I have not the courage to do this. Two decades is a very long time in terms of technology and the history of technological forecasting over such a timeframe is abysmal. I thus will concentrate my remarks on the time-frame of the next decade.

In looking ahead, there is a natural bias to give excessive weight to the present and to forecast a continuation of present trends. We all know that this is wrong and highly misleading, and that if there is one future certainty, it is that the future will differ from the present. But even acknowledging this bias, my own view is that we seem to have reached a plateau in the development of intercontinental transport, at least in technological developments of the magnitude and importance of those that have occured over the past two decades. Of course, there will be further progress and improvements, but I find it very difficult to believe that they will be as fundamental and farreaching as have occurred over the past two decades.

It is tempting to attribute such a conclusion to overconservatism, to say that any timid person could reach this conclusion at any point of time. But I do not think that this is so. Had I been standing on this platform in the late 1950's, I like to think that I would have been able to predict that dramatic research-based changes were on the horizon for all the intercontinental transport modes, even over a single decade. There was certainly enough work in progress and portents of change. This is simply not the case today. The developments I foresee are more of a filling in, a consolidation, rather than basic new thrusts. I would not mind, and would even welcome, being proved wrong in this prediction.

In intercontinental aviation, aiplanes may become larger, they will certainly become more fuel-efficient and quieter and the present sub-sonic speed standard is most unlikely to change. Costs are unlikely to decrease significantly, if at all, to say nothing of decreasing to the extent they have over the past two decades, given the very large increase in fuel prices that must be met.

General cargo handling seems unlikely to change dramatically. Containerization will continue its spread to lower density routes, new ship configurations may emerge for certain routes, handling equipment will continue to evolve and the actual boxes may become more sophisticated. Ships are unlikely to become much larger, faster or more economic.

Bulk cargo handling also faces no dramatic changes. Certainly the practical and economic size of vessels seems to have been reached. Further, I see nothing to suggest state-of-the-act changes in ship automation over the next decade. This applies also to general cargo shipping. I do foresee considerable development in the specialized types of shipping, such as transporting LNG or operating in far northern climates, and I will return to this theme later in this paper.

In summary, our expectations for the performance of research over the next decade should be scaled down considerably from the performance over the past two decades. This is not to say that there will be no scope for technical research; rather that the research is likely to be more specific and applied to clearly defined problems rather than to a quantum jump in the overall level of performance.

SOME DEFICIENCIES

I should now like to turn to my second theme - the deficiencies in our research performance to date and how we might deal with them. My comments in this connection will deal more with "soft" research rather than "hard" research; not only is this were I perceive the major deficiencies to be, but it is the area closest to my own interests. Once again, I will deal with each of the three intercontinental modes separately.

With regard to aviation, the success of our technical achievements is only matched by our failure on the commercial side. Structurally, the industry is locked into that very strange and unique organization, IATA. It grew out of the restrictionism in the 1930 and 1940's, and was fashioned in what appears to be increasingly like concrete, but at a time when the airline industry was vastly different from what it is today. It has been deseribed as everything from "a cartel" to "a disaster", but whatever one chooses to call it, there is little doubt that it has very serious shortcomings and, as it presently functions, is not particularly well-suited to the tremendous change and dynamism of the intercontinental aviation industry.

One could write a separate paper explaining the reasons for this. Suffice it to list some of the more serious deficiencies:

- the strange mixture of governmental and commercial interests
- the cumbersome method of decisionmaking
- the veto power
- incomplete coverage of the marketplace.

All of these deficiencies manifest themselves in an inability to deal with change in an aggressive forward-looking manner. I shudder to think how our economy would look if all industries were organized in this matter.

But the basic test again must be pragmatic. International aviation has been one of the classic postwar growth industries, yet the IATA carriers are *not* financially healthy and, have been steadly losing market share. Nor does this seem about to change. The industry has been fortunate in that it was swept along by improved technology, by growth in markets and by large scale government assistance. Commercial success, as we normally understand it, has not been the driving force. One must wonder whether this can continue indefinitely.

This is surely a fertile field for research. Not abstract technical research, nor research done in the ivory tower, but rather policy-orientated practical research of the type that can convince governments and airlines of the world that there must be a better way and that commercial flexibility and viability are in the interests of both users and suppliers.

Another fertile field for useful research is the entire

area of market analysis and research. The market forces governing the demand for aviation services are very far from being understood, to say nothing of being rigorously modelled. The proof of this is that it took the major carriers, their governments and various international organizations at least a decade to understand the growth potential of cheap charter-type transportation. The fact that they had tremendous difficulty in responding to challenge, once they had understood it, is explained by my previous comments on IATA.

It is nothing less than shameful that an industry which has been an acknowledged technological leader is such a laggard in its commercial organization and market understanding. I commend this entire area to those with a serious interest in policy research. I know that many of the relevant decisionmakers would welcome such an effort.

My last comment on aviation has to do with the huge and highly visible commercial disasters which have recently been visited on the industry - and I refer specifically to the Concorde and certain international airports. I suggest these are related to the very large non-transportation impacts that are expected from intercontinental aviation, or aviation generally, and to the very large role played by government in all aspects of the industry. I have seen economic research studies of these projects, done well before the fact, which conclude that they were viable and self-sustaining. Given the well-known outcomes, one can ask whether one should fault the researchers or the decisionmakers. The real answer is probably both. When decisionmakers know in advance what type of answer they want, for reasons only vaguely related to the economics of the project itself, when researchers themselves know what answer the decisionmakers expect, and when major proposals are put forth on the basis of a confused and unquantifiable list of justifications, one has present all the makings of a disaster. This is perhaps the worst type of relationship that can exist between the researcher and the decisionmaker and we should not be the least bit surprised when it produces disasters which discredit everyone in ugly post-mortems.

Given the visibility of aviation projects, their rapidly increasing threshold costs, the high level of non-transportation expectations from such projects, (coupled with the low-level understanding of the propagation mechanism for such effects) and the overly incestuous relationship that can develop between the researcher and the decisionmaker, we must be wary that further disasters are avoided. They do nothing for the integrity of transportation research and decisionmaking, to say nothing about the huge waste of public funds which results.

Turning next to general cargo shipping, the major deficiencies, in my view, once again lie on the software rather than hardware side, and it is there that research effort should be concentrated. For example, the entire documentation, customs clearance and financing aspects of general cargo shipping have yet to adopt themselves to the rationalization which has occured on the physical handling side, and there is still an administrative jungle involved in clearing cargo in nations of the world which should know better. The tools of systems analysis and modern communications/datahandling are at hand and there is need to apply them, for the benefits will be considerable. I am aware that a start has been made at this in many countries and also internationally, but there is still a long way to go.

Another example. Container transport by its very nature is multi-modal transport. It should be more than that, that is to say, integrated transport, with cargo moving on a single waybill, expeditiously transferred between modes where appropriate, with the shipper/consignee fully assured as to responsibility and liability. We are still some distance from that goal and there exists another field for productive policy-orientated research.

Intercontinental transport, particularly for general cargo, is not simply "shore-to-shore" but "door-to-door". The overall test of efficiency must be a total systems test, not simply a modal test. As an example of what can go wrong, I would mention the disasterous port congestion situation in parts of the Middle East and Africa. The problem here has to do both with hardware - the capacity of the port and inland transport systems - and software - the administrative difficulty in clearing cargo.

The difficulty with resolving software problems such as those mentioned, and this applies equally to the software/organizational deficiencies in *all* intercontinental transport, is the complexity of the decisionmaking process and the plethora of decisionmakers, all with differing interests. Not only are the owners/operators and their customers involved, but a major role is played by various agencies of national governments as well as financial institutions. It will not be an easy task, but there is much scope for interesting and productive research.

Turning finally to bulk shipping, the growing number of maritime mishaps, as more and more ships ply the world's trade routes with environmentally dangerous products, might suggest the need for improved navigation and communication systems, as well as safety standards. I do not think this is the case. Our navigation and communications systems are not deficient. Rather our ability to effectively enforce their use is terribly deficient. The same is true for safety standards. The need is to devise an effective enforcement system, compatible with national sovereignty and freedom of the seas. We are still some way from this - and I commend this area as a most worthwhile one. There is no lack of interested decisionmakers.

The commercial problems of bulk shipping unfortunately do not easily lend themselves to research solutions although those in the industry would welcome solutions from any source. The problems I have reference to can best be seen in the Norwegian fiords where a fleet of VLCC's rides at anchor, or in the fact that almost new ships are being sold at a substantial discount under what it cost to build them 3 or 4 years ago, to say nothing of their present value.

I hardly need elaborate that bulk shipping is the classical boom or bust industry, resulting from the total mobility of its capacity, the exogenous market forces which regularly make themselves felt (e.g. closing of Suez; OPEC) all coupled with a long lead time between ordering new vessels and their delivery. All this is exacerbated by the structure of the industry which results in herd-like swings of over-optimism and over-pessimism, force-fed by competing national subsidy programs which seem to have as their goal the maximization of ship construction in as short a time period as possible. I would be straining the limits of credulity if I were to suggest that research has an answer to these problems or that decisionmakers will change their reaction patterns because of research advice. But we should certainly do the research; perhaps someone will listen and we shall not be condemned to repeat the past.

There are, however, several research areas relating to bulk shipping which are terribly important, and will

become increasingly so. One has to do with the entire environmental problem of handling bulk cargo in ports. The problem is a very real one for it does no good to perfect the bulk handling of products by ship if overlapping and confusing port environmental standards, coupled with nervous public reaction resulting from lack of information or misinformation, preclude the construction of appropriate ports where needed. I would not suggest that there are not environmental factors which need full consideration in port planning, but I would maintain that we need as input hard knowledge based on sound and valid environmental research so that the issues can be dealt with rationally rather than emotionally. The research need is physical- what are proper standards and how do they apply - administrative - how do we organize our jurisdictions to apply these standards - and social - how do we communicate the facts to the public. The relationship between the researcher and the decisionmaker, in this instance, is a terribly complex one, but this does not make the problem less important.

A second interesting research area relating to bulk shipping has to do with specialized transport, and I would suggest that more and more maritime research will be devoted to this general area. For example, it is clear that the world energy problem will require dramatically increasing intercontinental shipments of LNG. Indeed, this is now beginning to happen. Ship and shore equipment has responded to the need and presently-produced equipment might be described as second generation. But the complexity and extremely high cost of the equipment remains a major problem. A quantum jump in technology would be a major step forward in meeting energy deficiencies and eliminating wasteful flaring of natural gas. Consumers, producers and owner-operators are all interested clients for such a development.

Another area of specialized transport which must receive increasing attention is marine transport in far-northern areas. This is not simply a Canadian problem, although Canada has a vital interest, but one that impacts most of the world's major countries. Economic pressures are forcing us to develop our far-northern resources and, by necessity, the maritime industry is involved. The research needs are extremely wide-ranging from the design of vessels and power plants, to detailed mapping, to understanding and forecasting ice formations and their movements, and even to attempting to physically control the environment in key areas. As the pressures are not only economic, but also political, the research users will be both private interests and governments.

CONCLUDING COMMENTS

It is the nature of my topic that a simple and clear conclusion is not to be expected. In summary, I have argued that:

1. Over the past two decades, there have been dramatic research-based development of intercontinental transport. Based on results, research has served the industry remarkably well.
2. The interaction between research and decision-making has been different in each of the intercontinental modes. Research responds to pressures and each mode has been subject to different endogenous and exogenous pressures.
3. The development of intercontinental transport seems to have reached a plateau, and the next decade will witness more of a filling in rather than a continuation of the dramatic changes of the past two decades.
4. There remain many interesting and fascinating research problems to be dealt with, with regard to both software and hardware.

If there is any single conclusion, it is that the future will be different from the past, but no less exciting and challenging.