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ENVIRONMENTAL IMPACT OF HYDROELECTRIC STRUCTURES
IN NORTHERN QUÉBEC

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1. HYDROELECTRIC DEVELOPMENT OF NORTHERN QUÉBEC

Traditionally, most electrical energy in Québec has been produced by hydroelectric power stations installed in the St. Lawrence basin. With other forms of energy replacing oil, the share of electricity in Québec's energy budget should rise from its present 26% to 45% by 1996.

As part of this approach, Hydro-Québec's construction program calls for building, in addition to the La Grande Complex (13 500 MW) now under construction, two other large scale hydroelectric projects in Northern Québec:

- The Grande-Baleine Complex, north of the La Grande Rivière, with installed capacity of 2900 MW.
- The NBR Complex to the south, with installed capacity in the range of 8200 MW.

These three projects involve 9 rivers, whose watersheds total 360 000 km². Some twenty reservoirs, totalling nearly 22 000 km², will be created and six rivers will be diverted to feed the 21 powerhouses on the developed rivers and generate more than 140 TWh annually.

The nature of the impact of these major projects on a nordic environment which had been spared industrial development until very recently has naturally aroused much concern. That is why environmental concerns have been accorded major importance throughout the planning, design and construction of these projects.

While large-scale impact studies are being carried out for the Grande-Baleine and NBR projects, the La Grande Complex is the subject of close monitoring and very extensive evaluation. Protective and remedial measures are implemented.
during the construction of this complex, and original solutions are applied to the principal problems caused by development.

2. TERRITORY UNDER DEVELOPMENT AND INHABITANTS

Located some 1000 km northwest of Montréal, the La Grande and NBR Complexes are part of the region commonly known as the James Bay Territory. It covers an area of 350 000 km², nearly one-fifth of the province of Québec.

This taiga region has a continental climate marked by a long and severe winter. The annual average temperature is approximately \(-4{°}C\), about 11° lower than at Montréal.

Scattered across the James Bay watershed are innumerable lakes and 6 of Québec's 19 largest rivers.

The sparse forest consists mainly of black spruce, jack pine and larch. Also found are many peat bogs. Among animal species, the beaver is undoubtedly the most important in the territory because it is the main resource of the winter trapping activity by the native people. In addition, the Cree traditionally hunt the migrating geese along the coast of James Bay during the spring and fall.

The population of the James Bay Territory is about 30 000, about 6500 of whom are Cree. Most of the native villages are on the east coast of James Bay, at the mouths of the major rivers. Although their villages are located outside the immediate vicinity of the structures, the Crees are affected by the construction.

In fact, the modified water system and the installation of major infrastructures have resulted in new forms of land use which may well disrupt their traditional way of life. Thus, from the very beginning of the project, the native peoples undertook steps to make sure that their territorial rights were respected.

Negotiations involved the 13 000 native people of Northern Québec, i.e. the 6500 Cree of James Bay and 5500 Inuit of Nouveau-Québec, the federal and provincial governments and the Société d'Énergie de la Baie James, the Société de développement de la Baie James and Hydro-Québec. After three years of negotiations, the James Bay and Northern Québec Agreement was signed on November 11, 1975.

This agreement is a major social contract, defining the rights and obligations of the parties and the future modalities of the management and development of the resources of all of Northern Québec.

In addition to approving the construction of the La Grande Complex, this agreement defined the lands to be used exclusively by the native communities, guaranteed their special hunting, fishing and trapping rights and established a
system of environmental protection for future projects. The native enjoy extensive powers of self-government and over a period of 20 years, they will be paid compensation amounting to some 225 million dollars.

It is under the terms of this agreement and in a spirit of environmental protection that the La Grande Complex, which I will now describe for you, is being built.

3. THE LA GRANDE COMPLEX AND THE ENVIRONMENT

In its first phase, the La Grande Complex calls for the construction of three powerhouses on the La Grande Rivièrè (LG 2, LG 3 and LG 4), as well as the partial diversion of the Eastmain River to the south and the Caniapiscau to the east. These diversions will nearly double the flow of the La Grande Rivièrè, the main tributary of James Bay, which will attain 4300 m³/s during maximum production. Begun in 1972, construction will continue until 1985. This Complex will then be producing some 62 billion kWh per year, 35.8 billion kWh of which will be coming from the LG 2 powerhouse, the most powerful underground generating station in the world.

Six other powerhouses may be built during phase II, which would add another 3300 MW to the installed capacity of the Complex.

Throughout this project, environmental concerns have been considered at every stage of construction as well as during the operation of the works.

First, during the impact assessment and project approval process, environmental specialists have participated in the analysis and selection of the development alternatives. Then, when the project has been approved, the engineering documents related to the various components of the project (dams, dikes, spillways, etc.), from the definitive preliminary project to the construction plans and specifications, are again analyzed by the Environment Dept. That is when the environmental recommendations are discussed with the engineers so that they can be taken into account and incorporated in the final design of the structures.

Moreover, special environmental protection directives adopted by the Corporation are an integral part of the contractual clauses of all tender documents. These directives are supplemental to government laws and regulations and make it possible for those responsible for the jobsites to minimize the effects of construction activities while the structures are being built.

During construction, a monitoring team is responsible for seeing that the environmental directives are applied and that the structures comply with the
the environmental protection clauses. All jobsites are subject to daily checks by the environmental officers who live in the territory.

In addition, following the extensive ecological inventory in progress since 1973, a major monitoring network has been progressively established to observe the environmental consequences during and after the impounding of the reservoirs. The observations collected are used to evaluate the measures applied to alleviate the impact of the construction, propose new measures if necessary and predict the impact of future projects.

Parallel to these programs, a series of mitigating activities is proposed by the Environment Dept. to alleviate unavoidable undesirable effects of the project. This department is also responsible for carrying out ecological improvement measures and checking on remedial construction when it has been included in a project.

4. MAIN ECOLOGICAL PROBLEMS ASSOCIATED WITH THE LA GRANDE COMPLEX

Defining remedial works for a large-scale project like the La Grande Complex was a major undertaking. The objectives were:
- assure the biological quality and productivity of the environment;
- allow the reservoirs to be used for traditional and recreational activities;
- obtain a visually acceptable environment near the sites most frequently visited.

Very diverse types of construction, involving a total investment of the order of 200 000 000$ have been or will be executed for these purposes. I would like to discuss with you some of the problems which attracted our attention during the past three years, as we watched the successive impounding of the LG 2 and Opinaca reservoirs.

a. Effects of Flow Cut-Off in the La Grande Rivière Downstream from LG 2

Since impounding LG 2 required the flow to be completely cut off directly below the dam, the intrusion of saline water from James Bay to the first rapids at LG 1 could have had an impact on the fish populations of the estuary, which provide the residents of Chisasibi with some 50 000 kg of fish each year.

SEBJ therefore sought solutions which would make it possible not only to protect the spawning grounds and sites for hatching and hibernation of the fish in this part of the river but also to maintain an acceptable fishing level in the longer term.
A scale-model study established the following facts:

- during the ice-free period, the saline limit rises rapidly to km 37, the site of LG 1;
- while there is an ice cover, however, this advance can be slowed down and limited to km 20 if a river flow of 8,5 m³/s (300 cu.ft./s) is maintained throughout the winter;
- after the thaw, saline intrusion can be contained within acceptable limits by the addition of extra inflow from the spillway.

This is how SEBJ came to select the scenario of closing off the river when there was an ice cover and to restore a flow of 285 m³/s (10 000 cu.ft./s) to the river through the spillway during the spring. Moreover, in order to improve the passage of fish to LG 1, the temporary diversion channel was modified to decrease speeds and thus permit the passage of fish upstream, where a section of more than 30 km of river could receive them.

This is what actually happened:

On November 27, 1978, at the time of the cut-off, the lower section of the La Grande Rivière downstream of km 35 was entirely covered with a thin layer of ice.

Three days later, the presence of saline water was measured at km 3,8.

The general speed of the phenomenon was quite in keeping with what the scale model had predicted, except that the location of the site of equilibrium at kilometer 19,6 did not occur in nature because the river flow measured was less than that initially expected.

Due to the network of monitoring stations that had been installed, it was possible to measure the maximum salinity values reached beneath the ice cover.

After having reached its upper limit, the salt water front retreated with the inflow of fresh water from the spring floods in the smaller tributaries. The ice left the river between May 13 and 21. The saline limit again advanced up the river, reaching kilometer 22,5 on July 10, despite natural discharges of 80 and 45 m³/s in June and July and a flow of 56 m³/s discharged at LG 2 from June 27 on. The saline water was then subject to to-and-fro motion caused by the variations of discharge from LG 2 and inflow from precipitation.

Most of the fish found in the La Grande Rivière downstream of LG 1 moved upriver with the progress of the saline front. For example, the crisco and longnose sucker, which are semianadromous fish (i.e. they spend the summer in salt water but prefer fresh water during the winter) went upriver as the salinity increased.
Mortality rate amongst the fish did not increase significantly while the La Grande Rivière was cut off downstream from LG 2.

The commissioning of the LG 2 powerhouse on October 27, 1979 not only ended the saline intrusion but also restored the river to its previous condition, and fishing yields returned to what they had been before the cut-off.

b. Effect of Impounding a Reservoir During the Winter

As I mentioned, the closing of the second diversion tunnel on November 27, 1978 initiated the filling of the LG 2 reservoir.

Because the La Grande Rivière valley has a narrow morphology, the water level had risen to about 75 m within only one month. Later, the flooding expanded horizontally and the water level rose more slowly.

At the start of September 1979, the reservoir had reached its minimum operating level (167.7 m) and the LG 2 powerhouse was officially inaugurated two months later, on October 27, 1979.

At its maximum level (175.3 m), the reservoir is 2836 km² in area. Its creation resulted in flooding a portion of the La Grande Rivière and Kannaupscow and more than 500 lakes and the submersion of about 975 peat bogs and nearly 2510 km² of terrestrial ecosystems.

Morphometrically, this reservoir has a maximum depth of 145 m, while its average depth is 22 m. The maximum length (SW to NE axis) is 112 km, while in its N-S axis, it is 42 km long. The total volume is about 62 billion cubic metres.

Filling a reservoir creates temporary conditions which are different from those which will exist once the dam is operating. Thus, in order to evaluate the physical, chemical and biological changes, various measuring and survey programs were carried out in the LG 2 Reservoir while it was being impounded.

Based on the observations made during that winter, we can describe how the ice cover progressed.

For example, in the case of the submersion of a lake, the water invaded the existing ice cover by overflowing the perimeter. Due to the severe climatic conditions prevailing from January to March, the water froze rapidly, thus adding a border of ice to the cover already formed. Under the pressure of the gradually increasing underground water level, the ice cover rose, expanding in area and thus causing the land to gradually disappear.
Similar phenomena were observed in the larger rivers. Moreover, in the La Grande Rivière and Kanaaupscow, ice agglomerations were observed at what had been the sites of rapids and when impounding began, ice piers were noted on the edge of the La Grande Rivière, as the result of the rapid rise in the water and other local phenomena.

In the tributaries, the pressure exerted below the existing ice cover caused breaks in the centre of the ice, showing cracks as it rose.

During April, when the reservoir was at elevation 140 m, several cracks were observed. These were generally narrow and shallow but quite long.

The thickness of the ice, as measured at various periods at the monitoring network stations, varied from 60 to 100 cm, and at several locations it was noted that there were several layers of ice separated by layers of slush occasionally more than 20 cm thick.

In addition, the intense cold which prevailed at that period promoted the rapid formation of an ice cover as the waters spread.

By rising with the advancing waters, the ice cover girdled the trees and uprooted them. Whole forests, imprisoned in the ice in this way, were pulled up over large areas yet maintained their natural contours, despite the many metres of water covering the original forest floor.

Trees not subject to deforestation in this way were nevertheless victims of the action of the ice cover. Not having sufficient leverage to uproot or break them, as it rose, the ice acted as a pruner, leaving little piles of broken branches on the ice cover.

When the ice melted, we saw enormous islands of ice and uprooted trees which were breaking up under the impact of the spring thaw. Most of the uprooted trees did not maintain their floatability sinking in the waters of the reservoir as soon as the ice cover, which had served as their "buoy", could no longer hold them on the surface.

During the ice-free period, most of the floating debris, pushed by the prevailing westerly winds, accumulated on the banks and in the bays. In June, the greatest concentrations were observed at the edges of the Sakami River, southwest of the LG 2 dam and in the centre of the reservoir. At the end of October, it was estimated that some 270 000 m$^3$ of woody material was floating on the reservoir.

As far as terrestrial fauna is concerned, animal activity on the edges of the reservoir was relatively intense. Many willow ptarmigan and hare were noted throughout the winter season and several predators, such as the snowy owl, wolf and
fox were observed. However, some of these animals undoubtedly succumbed as the waters rose. Still, the impact on the fauna was less significant because impounding the reservoir began in winter, when the animals could move on the ice. The main problem resulting from their movements was that a frequent change in habitat made them more vulnerable.

As for the beaver, which is the main resource for traditional activities of the native people, the impact on this species was minor, because there was intensive trapping before impounding. The purpose of this program was to recover the resource while reducing trapping pressure on the peripheral zones.

The physical and chemical properties of the water were generally very satisfactory. Exchanges with the newly inundated terrestrial environments were slowed by winter conditions. All in all, the impounding of LG 2 in the winter was essentially positive, and this initial experiment will be repeated with the Caniapiscau Reservoir.

c. Effects of Increased Flow in the Diversion Zones

From the time the configuration of the La Grande Complex was finally determined, SEBJ has been especially concerned with the environmental impact of the diversion of rivers.

Thus, in the increased flow zones, the ecological development plan calls not only for the whole range of remedial measures provided for reservoirs but also some significant construction to:
- concentrate the flow of the waters;
- avoid raising the bodies of water and thus reduce the flooded area, and;
- counteract erosion phenomena.

Such work has been done in the Boyd-Sakami diversion zone.

In the area around Lake Boyd, installing protective dikes has made it possible to avoid flooding secondary valleys and thus preserve an area with a high animal potential.

At Lake Sakami, which is one of the most productive inland aquatic systems as well as an area intensively used by the native people for traditional activities, we excavated a channel at the lake outlet and installed rock masses in order to promote the formation of a stable ice cover and eliminate the risks of ice jams, which would have led to a major rise in water levels.
At this time, after the first year of diversion, we are in the process of evaluating the adjustment in the narrow sections of the route and the significance of the erosion which has occurred there. Except for the kilometre just before waters enter Lake Sakami, where the banks have undergone major changes, we consider this diversion a major success.

d. Effects of the Flow Cut-off Downstream from the Diversion Points

In April and July 1980, respectively, the diversion canals for the Opinaca and Eastmain rivers were closed, resulting in a major reduction of flow downstream from the cut-off points. The reduction was such that the annual average residual flow at the mouth is no more than 10%.

Diversion of the waters of the upper basins of the Eastmain and Opinaca rivers has caused major physical and biological changes downstream of the cut-offs and disturbed the ecological balance of these environments.

Following the flow reduction, major decreases in water levels were observed in various sections of the Eastmain and Opinaca rivers. Depending on the section, the drops varied between 1 and 4.3 m. This caused the emergence of about 36 km² of banks along the Eastmain and 10 km² along the Opinaca.

Moreover, various phenomena have caused banks to emerge and the embanking of several tributaries. More than 40 000 m³ of material has been washed away in both rivers, 33 000 m³ of it in the Eastmain.

The nature and scope of the remedial works undertaken were based on these observations. Thus, four river sections, two on the Eastmain and two on the Opinaca, will be protected by the construction of weirs which will make it possible to maintain water levels close to those occurring under natural conditions. These are sections with a high ecological potential where a large proportion of banks are uncovered.

The two weirs planned on the Eastmain river will make it possible to reduce the emergent bank areas by about 80% and decrease the amount of eroded material by about 75%, while on the Opinaca, these reductions will be 80% and 60% respectively.

Two weirs will be built in 1981, Weir No. 5 on the Eastmain and Weir No. 8 on the Opinaca. The other two will be installed in 1982.
Weir No. 5

Weir No. 5 is located at km 135.4 of the Eastmain, on either side of a rock island. The work consists of wood gabions filled with rock and protected upstream by a mound. There are several sections linking rocky hillocks. A discharge channel is installed in the right arm of the river.

Weir No. 8

Weir No. 8 is located at km 60.4 of the Opinaca. The weir is a concrete structure containing two passages: a winter channel located on the right side of the river and the main channel, located on the left side.

These two weirs have been designed to respect the following criteria:
- the annual average level and the maximum level must be similar to those observed under natural conditions;
- the annual drawdown pattern must be close to that of a natural lake;
- the annual average drawdown must not exceed 1 m;
- unpredicted rises in the water levels and icing of the weir are to be avoided by concentrating the flow during winter;
- resistance to ice pressures must be about 10 kips to withstand all discharge conditions.

In addition, the new banks will be seeded and planted in other sections of the river which are suited for such treatment. The program will involve a total area of about 53 ha, on which some 200 000 young shrubs will be planted.

Finally, the monitoring program begun in 1980 during the cut-off of these rivers and continuing until 1984, will make it possible to determine the impact of the flow reduction and better evaluate the effectiveness of the remedial installations presently being considered.

e. Impact of the Exploitation of Sand and Gravel Pits

The use of natural materials (moraine, sand, gravel and rockfill) in the construction of the 192 dikes and dams of the La Grande Complex and the 100 km of access roads has meant stripping and digging of 6000 hectares of land.

Ecologically, this represents a loss of biomass and habitat for some animal species. Moreover, the physical changes (excavation, filling) which these sites have
undergone have slowed down the recovery process for native vegetation. Without planting, some sites would remain bare for decades, and surface erosion would deteriorate the sites even more. The presence of such sites near the main structures and permanent roads also poses an esthetic problem. That is why over the last 4 years, SEBJ has undertaken a program of studies for developing a restoration policy for affected sites and defining the most appropriate methods for revegetating this nordic region. The experimental tree nursery at Lake Hélène has make it possible to define a 2-stage site recovery program involving the physical restoration of the site and then the actual planting or seeding. In the first stage, each contractor must physically restore the sites he has used, generally cleaning them up and spreading over the organic material accumulated along the edges during stripping operations. Slopes which are too steep must be made more gentle and compacted surfaces scraped.

In the second stage, planting and seeding is carried out by the Environment Dept., using a master plan for each jobsite which sets the priorities and prescribes the necessary measures.

At the La Grance Complex, about 7 million young trees and bushes will be planted on the sites affected by construction and more than 200 hectares will be seeded with grasses.

**CONCLUSION**

In conclusion, I would like to point out the innovative nature of the experience SEBJ is now undergoing within the framework of the execution of the major hydroelectric projects in Québec.

We have developed a new system of managing large projects which integrates every aspect contemporary developers must face in executing them. The control by the Corporation of the environmental and social impacts of these projects has made it possible to increase the ecological awareness in a work sector which was initially unreceptive to these concerns and to significantly improve the environmental protection measures on our construction sites.

Moreover, the Corporation has approved several completely unprecedented programs of remedial works and mitigating measures. The monitoring network developed at the La Grande Complex will make it possible to evaluate the success of such investments and the results achieved will act as guides for future projects.
While pioneering in these areas, SEBJ is only fulfilling the undertakings it assumed at the beginning of the project and demonstrating that it is possible to develop the resources of this territory in harmony with the natural environment and in agreement with the native populations of the region.
DOME PETROLEUM OPERATIONS IN THE BEAUFORT SEA

Bengt M. Johansson  Dome Petroleum Limited  Canada

I would like to start this session about frontier transportation by giving some background of what Dome has been doing in the Arctic so far. Hopefully this will support the main message of my short talk which is that we think that it is technically possible to design and build Arctic petroleum drilling production and transportation systems that are both economical and environmentally safe.

Dome Petroleum is one of the large Canadian oil companies and it has an ambition to grow further. It was started in 1950 as a one-man operation and to-day it has a yearly turnover of about $1.2 billion and a staff of about 4000 people.

There are four oil and gas basins in Canada, i.e. Western Canada which is already producing, offshore the Canadian East Coast, Beaufort Sea and the Arctic Islands. Potentially these four basins are of the same importance and could yield equal recovery of oil and gas.

In the total Arctic perspective the sedimentary basins are truly vast
with the Soviet Union having about 70 percent of the potential while Alaska and Canada shares the rest about equally.

At the moment Dome is exploring with four drillships in the Beaufort Sea. The season is about three and a half months starting in early June and ending in the middle of October. These four drillships are ice-strengthened to ice-class 1 A Super and use fairly conventional mooring systems to stay on location. The water depth is fairly shallow, between 20 and 100 metres. Dome and its partners have been drilling in the Beaufort Sea for five years and have made several oil and gas finds. The two most promising ones are called Kopanoar and Tarsuit and during the 1981 drilling season, Dome will complete delineation drilling on these structures to evaluate if they are large and productive enough for commercial development.

At Tarsiut Gulf and Dome are building a concrete walled island in 23 metres of water at a location close to the original oil find. The island will be finished in October this year so that we can start drilling in November or December.

One of the main differences between the Tarsiut Island and the earlier sand islands built in the Beaufort Sea is that the Tarsiut Island is beyond the shore-fast ice and thus will be subject to the forces of the moving pack for the whole year. We anticipate to get very important full scale test data from this island which will be used in the design of more permanent production islands in deeper waters of the Beaufort Sea.

To date, dredging systems have not been developed for operation in water depths of up to 80 metres. Dredging systems, capable of operating year round in ice-covered waters, must be designed to withstand ice forces, year round, for up to 25 years or more. Construction techniques for reducing fill requirements must be developed to reduce costs.

In addition, Dome will be constructing and evaluating a new design, ice-breaking "superdredge". The dredge, a cutting suction hopper dredge, will have a payload capacity of 25,000 tonnes, approximately 2.5 times the size of the largest hopper dredges in existence today. The vessel will be an Arctic class icebreaker capable of year round Beaufort Sea operations. The dredge will be able to dredge material in 80 metres of water, compared with a maximum dredging capability of 35 metres waters depth today.
At the present time Dome is also designing a year round floating system for drilling in the Beaufort Sea. A year-round drilling system is required to (i) extend the current, short open water drill season to year-round, and hence provide a more economical use of expensive capital equipment, with a resultant decrease in exploration well costs, (ii) accelerate the discovery delineation and production of oil and gas by increasing the number of exploration and delineation wells drilled each year, and (iii) provide same season relief well drilling capability. The new design, floating drill system, the Round Drill System (RDS), has a symmetrical hull form and the anchor lines are well below the ice. To stay on location year round you need the assistance of powerful icebreakers. The earliest we could have one of these units in the Beaufort Sea is 1983.

It is Dome's opinion that Arctic Tankers and Arctic LNG Carriers will be used initially for the transport of hydrocarbons from Arctic waters. There are several reasons for this:

- Threshold reserves for economic delivery of oil and gas with Arctic shipping are approximately 1/10 of those required for a large diameter pipeline system;

- The lead time required for tanker delivery is shorter than pipeline completion, so that returns on investment are realized sooner; and

- Arctic shipping delivery systems allow much greater flexibility in marketing, hence, producers can quickly and easily adapt to changes in market demand.

To test the concepts of Arctic Marine Transportation Dome decided to build an experimental icebreaker, the "Canmar, KIGORIAK", which was ordered in 1978 and delivered in 1979. This 16400 SHP icebreaker has several features that could favourably be incorporated in both tankers and LNG-carriers for Arctic service.

One of the novel features of this ship is the short spoon shaped bow which ends in a reamer that is wider than the rest of the ship which is built totally of flat plates. For the KIGORIAK this was important due to the very short delivery time which could not accept any complicated hull lines. This feature is very important for an Arctic tanker because it has to have a very long mid body where the cargo tanks are, and we wanted to test if you can
develop this into a suitable icebreaking form.

Another special feature on the Kigoriak is the single propeller with a controllable pitch mechanism. The importance of this mechanism is that you reduce your pitch when the propeller encounters ice and thus keep the machinery from stalling. The propeller is also protected by a heavy steel ring in such a way that large ice pieces cannot get into contact with the propeller.

The KIGORIAK was delivered in August of 1979 and it started from St. John, New Brunswick, where it was built in early September and it arrived at the western part of the North West Passage in late September. 1979 was a very difficult ice year - the worst year in ten - and the ice didn't really open in the western part of the North West Passage. It was a fairly exciting exercise coming straight from the shipyard with a new ship and the thickest ice we had to go through was about ten meters thick solid ice, and I quite frankly didn't think we were going to make it, but the Master was extremely skillful and he made it.

Once we got in to the Beaufort Sea we made some tests in the Polar Pack in October of 1979. The thickest ice we saw there was about 30 metres thick and may have been 50 to 100 years old. Naturally we couldn't break that ice.

Then in January and in March of 1979 we made some winter tests in the southern part of the Beaufort Sea where the maximum ice thickness is about 1.8 metres. Because of high winds it sometimes compresses up into big rubble fields which may be grounded in 20 metres of water. We went through some ridges that were grounded in 12 metres of water and the sails were seven metres above the water line.

The KIGORIAK test proved very clearly that the most dangerous period for the ship is not in the winter; the most dangerous period is in the summer. We had no problem at all in January and March but we have got some damage on the KIGORIAK in August. When you think about this it's quite natural because the multi-ice is still there in the summer and there is open water around it so you can charge very hard at the ice and thus create tremendous forces. The damage on the KIGORIAK wasn't extensive but it was enough to teach the designers that they had to be a little more careful.

After testing the Kigoriak since 1979, Dome has learned how to make significant improvements in the Kigoriak design. These design improvements are to
be incorporated on the Supplier 9, an Arctic Class 3 icebreaking supply vessel that Dome is currently constructing in Vancouver. Dome will take delivery in early 1982 and will test and evaluate the design improvements. Dome has also started designing a large icebreaker, the AMLX-10, that will incorporate the most improved icebreaking design concepts to date. The AMLX-10 will be capable of navigating in all Arctic waters at all times of the year. This vessel will demonstrate that the technology for year-round Arctic shipping has been developed. The proof of this concept is essential for the development of year-round shipments of hydrocarbon products in Arctic waters.

When you are trying to marry an ice-breaker and a tanker you have to study the history of both ships from a safety point of view. The ice-breakers are extremely strong ships and they are usually double-skinned. The record, as far as we can find out, shows that no ice-breaker has been lost in the world in a hundred years and also that there have been extremely few cases of pollution from ice-breakers.

There have been several accidents with ice-breakers; groundings and collisions. One typical example was a Swedish ice-breaker which was in collision with a Finnish ice-breaker. They happened to be in the same channel and they couldn't get out of it. Because nobody was living in the bow there was really no risk for the crew because only the part above the ice strengthening was damaged on these ships.

When you study the record of the tankers in the world you only have to read the papers to find out it's not nearly as good as that of the ice-breakers. Dome commissioned a study to be made by Det Norske Veritas in Oslo and they went through ten years of records of tanker accidents that led to oil spills. They found about 200 cases of oil spills that were bigger than 200 tons each. When you went through some typical examples they showed that the main cause of the pollution was human error, very often combined with an initial technical difficulty like a faulty steering gear or similar. If the right action had been taken the pollution incident might not have happened.

When we are taking our first steps towards year-round navigation in the Arctic I think we have to be very cautious and make a "virtually indestructible" design for the first ships. We also have to design them so that even if they should fail there is no danger for the crew or the environment.
Once we get experience we can start optimizing and save money. This is opposite to normal human behaviour, where you start in cheaply and make it stronger as you learn through accidents.

Dome has made a preliminary design of an Arctic tanker for year-round navigation. It has the following main particulars:

- Length 370 m  
- Beam 52 m  
- Depth 38 m  
- Draft 20 m  
- Cargo deadweight 200,000 tons  
- Power 150,000 SHP

The hull strength is considerably above the present Arctic Class 10 requirements to enable the tanker to hit multi-year ice at high speed. The oil is carried in inside tanks with U-shaped water ballast tanks around them. To increase the reliability of the ship it has two separate propulsion trains, two separate and independent rudders, etc.

It is possible to design the side of the ship strong enough and the wing-tanks wide enough so that in case of a collision the cargo oil tank will not be punctured. It is not possible to guarantee that the two bottoms may not be holed in case of a severe grounding, especially in open water where the waves may pound the ship against the rocks and continuously increase the damage to the bottom.

To study what may be done in this case Dome made some simple model tests where we had one section of the proposed tanker floating in a basin with the center tank full of oil. In the first test series we just opened holes in the inner and outer bottom. Due to the high hydrostatic pressure in the oil tank some of the oil rushed out into the sea and we lost about 20% of the oil.

The solution to this problem was very simple. We just reduced the size of the air pipe to the oil tank and tried again. Now the oil was coming out very slowly from the center tank and had plenty of time to float up into the wing tanks. In the second set of tests we did not lose any oil into the sea.

When the oil has reached a state of equilibrium it is possible to start pumping it into empty ballast tanks in other areas of the ship. At the same time compressed air can be delivered to the tanks and get the water out thus making it possible for the tanker to get off the ground.
When ships are travelling year-round through ice covered water, it will be necessary to develop systems that will facilitate route selection in the easiest and most efficient ice conditions and to detect hazardous ice conditions in sufficient time for the ship to avoid these ice features.

Dome, with the Canadian Government and other companies, is designing a combination of shipborne, airborne, and satellite borne remote sensing systems that will be required to meet the differing Arctic drilling and shipping needs. Ice forecast capability over the various sections of the route is needed where the ice conditions are expected to change while the vessel is in transit. An elaborate satellite communications network and data handling system will be designed to transfer the various information sets to and from vessels and operation centers.

In summary we can make the following observations. It is possible to make Arctic shipping and drill systems environmentally safe without spending excessive amounts of money just for this purpose. This is a fact because most of the features which are important to meet the ice-breaking requirements are also advantageous from the anti-pollution point of view. By making minor adjustments it is possible to combine these two requirements in a very economical manner.

If the present plans for development of the Beaufort Sea materialize it will create a large demand on the Canadian shipyards. At present, Canadian shipyards can process about 60 - 70,000 tons of steel per year but the demand for the Beaufort Sea will peak at about 400,000 tons of steel per year. Thus there will be a tremendous possibility for Canadian shipyards and associated industries.
1. INTRODUCTION

This appendix presents a preliminary analysis of the strain measurements carried out on the icebreaker HMS Ymer on the Swedish Arctic Expedition Ymer 80. The final report on the strain measurement will be completed during the autumn 1981.

The aim of the measurements was to study the character of the ice impacts on the hull and to study the structural response in the hull structure in order to extend the knowledge about ice impacts on ships.

2. STEEL STRUCTURE

For orientation a brief description of the hull structure is given.

The shell plating is supported by transverse frames which have a spacing of 400 mm. The frames are supported by stringers or by decks. The stringers are welded to vertical webs or bulkheads 2400 mm apart.

The ice belt extends about 3 m below and 1.5 m above the design water line.

The thickness of the plating in the ice belt in the fore body is 32 mm. In the mid body the thickness is 28-30 mm and in the aft body it is 30 mm. The thickness of the plating below the ice belt is 25 mm except in the fore body were it is 28 mm.

In the bottom the shell plating is 17 mm. It is supported by floors spaced 800 mm and longitudinal girders or longitudinals also spaced about 800 mm.

The hull members can withstand about the following equally distributed pressures without permanent setting.
Ice belt \( \text{(N/mm}^2 \text{ or MN/m}^2 \text{)} \)

<table>
<thead>
<tr>
<th></th>
<th>Plating</th>
<th>Frames</th>
<th>Stringers</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fore body</td>
<td>8</td>
<td>6</td>
<td>1,0* 2,5**</td>
</tr>
<tr>
<td>Mid body</td>
<td>7</td>
<td>5</td>
<td>1,0* 2,0**</td>
</tr>
<tr>
<td>Aft body</td>
<td>7</td>
<td>5</td>
<td>1,0* 2,0**</td>
</tr>
</tbody>
</table>

Bottom plating about 1,0 N/mm\(^2\)

* The stringers yield due to shear before they develop three plastic hinges

** Three hinges developed.

3. **MEASURING SYSTEM**

By means of 27 strain gauges the stresses in the following structural members were measured:

- Plates fields: 8 gauges
- Transverse frames: 13 gauges
- Stringer: 3 gauges
- Web: 3 gauges

The gauges in the plate fields were located at the middle of the field and orientated perpendicular to the framing. The gauges on the frames were located at the top of the frames and at the mid span.

At one stringer and at one vertical web three gauges gauged were placed. One gauge at the middle of the span and the other two just before the brackets at the ends of the members.

The measurements were carried out in three areas of the hull, see Figure 1. Measurements on the plating were carried out in all areas. Stresses in the frames were studied in the rear and middle area. At the forward area the measurements on the stringer and on the web were carried out. Between the middle and forward area one plate field was studied (gauge No 19).

The gauges were divided into 8 groups, 6 of which were used for recording on an Ultra-violet (UV) printer. Two groups were connected to a tape recorder. One gauge could belong to several groups. In Table 1 the groups are shown.

The main intentions behind the grouping were:

- **Group No 1**: To study the character of the impacts in the three areas.
- **Group No 2**: To study the vertical extension of impacts and possible differences in character at different depths.
Group No 3: To study the horizontal extension of impacts.

Group No 4: To try to estimate the ice load on a frame by measuring the shear forces.

Group No 5: To study the response in a stringer. The responses at the mid span of the web connected to the stringer and at a plate field above and below the stringer were studied in this group for comparison with the response in the stringer.

Group No 6: To study the response in a web. The responses at the mid span of a stringer connected to the web and at a plate field above and below the stringer were studied for comparison.

Group No 7: Same as group No. 1.

Group No 8: To study the character of the impacts in frames, a stringer and a web. An example of an UV-recording is shown in Figure 2.

4. RESULTS, UV-PRINTER

The most interesting parts of the UV-recordings have been analysed. This analysis comprise about three hours of measurements. The total time of UV-recording was about six hours.

The results are shown in figures which indicate how many times a stress level is exceeded (Figure 3, 5, 6, 10 and 11). The stress axis (vertical axis) is linear. The horizontal axis shows how many times a stress level is exceeded. This axis has logarithmic scale.

A secondary aim of the UV-recordings was to obtain knowledge of the character of the ice impacts in order to find a suitable method for analysing the magnetic tapes by means of computer.

Group No 1. Results from measurements during 43 minutes in different ice conditions are shown in fig. 3.

The impacts occur mostly in the forward area and seem to decrease towards the aft. The highest peaks occurred in the rear area.

In each area signals from two gauges have been included, i.e. the measuring time is 2 x 43 minutes for each area.

Group No 2. The gauges were located at depths about 0,3 - 2,0 m below the operating water line.
13 minutes of the UV-recordings were analysed. The ice conditions were open pack to close pack, the ice thickness 0.6 - 2.0 m and the speed 2 - 8 knots. No significant differences between gauges at different depths were found.

**Group No 3.** Figs 4 and 5. This is probably the most interesting one of the UV-groups as the horizontal extent of impacts can be studied.

Fig. 4 shows how many times a specified number of gauges, located beside each other, are instantly stressed. Each cross means one occasion and the maximum stress in the frames at that moment can be read on the vertical axis. The maximum extension, where the stresses in the frames were high at the same time, was about 2000 mm during one hour measurement.

In fig. 5 the number of stress peaks exceeding certain stress levels are shown. A straight line in the diagram fits the points very good. In the figure the results from all six gauges have been included.

**Group No 4.** The shear stresses were quite low and thus the recorded results are somewhat unreliable. This method of estimating the ice loads should therefore only be used in connection with a computer that calculates the load on the frame directly.

**Groups Nos 5 and 6.** Fig. 6. The stress at the mid span of the stringer and web are plotted. For comparison the stresses in the plate field below the stringer are shown.

For high loads the distribution is similar in the stringer, the web and the plate field, but for low loads the stringer and web have more peaks than the plate field.

5. **RESULTS, TAPE RECORDER**

The magnetic tapes are still undergoing analysis at this time.

A special computer program has been developed in order to analyse the magnetic tapes. The computer is of hybrid type. It contains an analog and a digital computer with an interface. The program reads the four signals on the tapes simultaneously. The sampling frequency for each channel is about 100 Hz. The program was developed by Systemteknik O.M., Gothenburg. An outline of the program is shown in fig. 9.

The method of analysis is based on the level crossing principle, i.e. the results obtained show how many times pre-selected stress levels are exceeded.

The main problem in developing the program was to separate disturbances from the signals representing the hull responses.
Results from the first analysis by means of the computer program are shown in figs 10 and 11.

6. HULL DAMAGES

6.1 Frequent damages

Before departure to the Arctic the hull of Ymer was without any visual damages, such as plates dented or frames, stringers, webs and brackets buckled.

After the arrival from the Arctic the main parts of the hull were inspected at a dry dock. Beside two large dents at each side of the ship, see section 6.2, the following damages occurred frequently:

- 5 plates dented between frames mostly in the lower part of the ice belt and in the mid body. Most of the dents were small.
- Many plates dented in the bottom. In the fore body the number of dented plates was very large.
- Frames were buckled where the plates were dented in the ice belt. At all locations where frames were buckled the adjacent stringer was damaged, see below. A total of 22 frames were buckled in the side structure, 8 in the fore body and 12 in the mid body. Nearly all of these damages were located in the lower part of the ice belt.
- 12 stringers were damaged, 3 in the fore body and 9 in the mid body. The damages occurred mostly at the stringer just below the ice belt. A typical damage is shown in fig. 7. Plastic areas have developed above the cutouts for the frame. Lugs located in these cutouts had improved the strength.

The damages in the ice belt in the mid body are numerous compared to the fore body. One contributing reason might be that it is easier to inspect the mid body thoroughly.

6.2 Large damages

Two large damages occurred during the voyage in the Arctic. At both occasions the ship was turning and the sides had a relatively high speed perpendicular to the length direction of the ship and due to the narrow open water channels the ship's sides suffered impacts from the fast ice.

The damages are shown in fig. 8. The maximum dents were about 0.1 m for both damages. The shell plate, steel of grade E, buckled without any cracks in base material or welds.
7. DISCUSSION

The distribution of the stress levels fairly well fits the formula:

$$\sigma = \sigma_1 (1 - \log N/\log N_0)$$

where N is the number of times $\sigma$ is exceeded. ($\sigma_1$ and $N_0$ are shown in Fig. 12). But for low stress levels there is some disagreement, $\sigma$ is larger than indicated by the formula. At high stress levels the number of peaks decrease. Thus the statistical material is not as good as for low and medium stress level. At low stress levels there were some difficulties to count the peaks due to disturbencies and due to difficulties to estimate the "zero stress level".

As the response in the steel material is linearly related to the loads, the loads within a specified area, i.e. the ice impacts, should follow the same formula or:

$$P = P_1 (1 - \log N/\log N_0)$$

where N is the number of times $P$ is exceeded.

In normal icebreaking conditions, the horizontal distribution of ice impacts seems, to have been less than about 2 - 3 meters.

The maximum pressures have at least on 12 occasions been above 1,0 N/mm$^2$ at horizontal lengths of about two meters (or greater on shorter lengths) as indicated by 12 damaged stringers.

As most of the hull damages on the side structure occurred at the lower part of the ice belt or below the ice belt, ships navigating in similar ice conditions should have icebelts which extend deeper below the water line. The large amount of damages on the bottom structures shows that the strength of the bottom should be well above 1,0 N/mm$^2$ and in the fore body much above 1,0 N/mm$^2$ for ship with about the same draught.

As uncontrolled impacts on the hull structure against fast ice edges or large floes cannot be avoided, ships navigating in similar ice conditions should have a double skin structure. Uncontrolled impacts may otherwise be hazardous to the ship. For same reason the side plating should be of steel with good toughness at all service temperatures.

None of the damages decreased the hull strength to such level that the voyage had to be interrupted. However it is not satisfactory that stringers, etc, are allowed to yield due to shear, causing possible buckling of adjacent frames.

The side plating on Ymer have sufficient strength for the operating conditions encountered. The number of dents (5), most of which insignificant, in the
side shell was acceptable. The maximum stress measured in the plate fields was 260 N/mm².

The strength of the framing seems to be somewhat too low. However the deflections of adjacent stringers might have cause frames to buckle.

The aim of the measurements reported was also to try to relate the impacts to the ice conditions and to the speed of the icebreaker. However this becomes a very complicated work as the number or parameters is very large. An example of parameters describing the ice conditions is shown in table 2. As can be seen, the number of parameters soon becomes very large. Further these parameters should be combined with some speed intervals, say 1-2, 3-5, 6-8, 8-12 knots, i.e. 320x4=1280 different conditions.

A simplified approach is to reduce the parameters drastically. For example using the ice thickness and the speed as parameters only. By this method we obtain about 20 conditions.

At present none of the above methods have been used for the analysis.

Further analyses by means of the UV-recording, will probably be carried out with the ice thickness as the sole parameter.

The above mentioned items will be analysed in more detail during the final work with the material from the voyage to the Arctic. The final report will hopefully be completed in fall 1981.
Table 1

GROUPING OF GAUGES

<table>
<thead>
<tr>
<th>UV-PRINTER</th>
<th>TAPE RECORD.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Group No.</td>
<td>1 2 3 4 5 6 7 8</td>
</tr>
<tr>
<td>1</td>
<td>1 16 9 3 22 25 1 6</td>
</tr>
<tr>
<td>2</td>
<td>2 17 10 4 23 26 16 11</td>
</tr>
<tr>
<td>16</td>
<td>16 18 11 5 24 27 20 23</td>
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<tr>
<td>17</td>
<td>17 19 12 6 26 23 21 26</td>
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<tr>
<td>20</td>
<td>20 11 13 7 20 20 20 20</td>
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<tr>
<td>21</td>
<td>21 15 14 8 21 21 21 21</td>
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</tbody>
</table>

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<tr>
<th>Location</th>
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<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
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<tbody>
<tr>
<td>Plating horizontal, global</td>
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<tr>
<td>Plating &amp; Framing vertical, local</td>
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<tr>
<td>Frames horizontal, local</td>
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<tr>
<td>Frames shear, local</td>
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<td>Stringer &amp; Plating</td>
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<td>Web &amp; Plating</td>
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</tr>
<tr>
<td>Framing, Stringer &amp; Web</td>
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<td></td>
</tr>
</tbody>
</table>

1161
Table 2

Example of parameters describing ice conditions

<table>
<thead>
<tr>
<th>First year ice</th>
<th>Open pack</th>
<th>Small floe</th>
<th>Thickness 0.5 - 1.0 m</th>
</tr>
</thead>
<tbody>
<tr>
<td>Multi year ice</td>
<td>Close pack</td>
<td>Normal floe</td>
<td>&quot;-&quot; 1.0 - 1.5 m</td>
</tr>
<tr>
<td>Hard packed snow on top</td>
<td>Fast ice</td>
<td>Large floe</td>
<td>&quot;-&quot; 1.5 - 2.0 m</td>
</tr>
<tr>
<td>Combination of above</td>
<td>Combination of above</td>
<td>Combination of above</td>
<td>&quot;-&quot; 2.0 - 3.0 m</td>
</tr>
</tbody>
</table>

| Number of parameters | 4 | 4 | 4 | 5 |

Total about 320 different conditions.
DETAILED ARRANGEMENT OF STRAIN GAUGES IN THE FORE BODY.
FIGURE 2
TYPICAL EXAMPLE OF HULL STRESS RECORDING FOR GROUP NO. G3.
Group No. 1, time of measurement 43 min
O.P. - F.I., 0.7-1.9 m, 2-10 knots.
The sum of the signals from two gauges is plotted.
Fig. 4  Distribution of horizontal load extension and maximum stresses as function of load extension.

8/10-10/10, 1.0-1.5 m, 4-8 knots.
Fig. 5  Results from six gauges located on frames. Group No. 3.
Group Nos. 5 and 6. Comparison of distribution of peaks between web, stringer and plate. Time of measurement 35 min, O.P-C.P ice, 0.5-2.0 m.
Fig. 7  Typical damages on stringer.

Plastic areas due to shear stress and cracks in welds between stringer and shell plating.
BUCKLED BULKHEAD AT FRAME NO. 68 PORT

BUCKLED DECK NO. 3 IN ENGINE ROOM PORT

FIGURE 8
Fig. 9. System for analysis of magnetic tapes.

Fig. 10

Results from one gauge located in the rear area and one in the mid area, both on frames.

Time of measurement 150 min. (Tape recording.)
Results from measurements at the mid span of web.
Time of measurement 130 min. (Tape recording.)
Fig. 12. Formula fitting the distribution of stress levels.

\[ \sigma = \sigma_0 \left(1 - \frac{\log N}{\log N_0}\right) \]
DEVELOPMENT AND IMPLEMENTATION
OF SHIP ICE CERTIFICATES

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Yu. N. Popov, Arctic and Antarctic Research Institute
Senior scientist USSR

ABSTRACT

Annual growth of cargo traffic and the lengthening of the navigation season in the Arctic, as well as the USSR participation in Antarctic research, demands a large number of cargo and research vessels suited for ice navigation, i.e. with adequate icebreaking capabilities and ice-strengthened hulls. Because of the possibility of ice damage, each ship should have, in addition to the usual documentation, an "Ice Certificate" containing the information which would facilitate the better choice of tactical and technical elements ensuring the fail-safe operation of the ship under ice conditions. This paper describes the background and requirements for "Ice Certificates" in the USSR.

Cargo carriers employed for ice navigation should be specifically designed for operations in icebreaker convoys under various ice conditions and should have those ice qualities which would ensure the fail-safe passage.

Unlike the navigation in the open water when the ship speed is determined by the ship power, the choice of the optimum speed for a ship operating in the ice covered waters has some of peculiarities. Even for cargo carriers of UL USSR Register ice class ice speed can be limited by the ice strength of the hull, particularly at the bow. In such a case the ship cannot move with full power for fear of damage. That is why ships for ice navigation should have compatible hull strength and power, in other words the strength of the hull should ensure, if need be, the full use of the ship's propulsion plant without any fear for damage of the hull.
In order to efficiently use the ships for ice navigation in convoys and in autonomous operation, to arrange the order of convoys and to choose correctly optimum ship speeds under particular ice conditions it is necessary to know their ice capabilities, that is their ice performance and strength, manoeuvrable and inertial characteristics of the ship in ice, the state and protection of the propeller complex and the like. Otherwise unnecessary delays and idle standings of individual ships or convoys occur. There is always a possibility of ice damage. Thus in addition to the usual documentation each ship should have a special paper, containing the information which would facilitate the better choice of tactical and technical elements ensuring the failsafe operation of the ship under ice conditions. This paper has got the name of an ice certificate or an ice passport.

The idea of this certificate regulating the speed and other characteristics of ship ice navigation originated in the AARI Ice Ship Research Laboratory as early as 1960. It have taken, however, more than 10 years to implement it. It was necessary to define more specifically those parameters which ensure safe navigation and it was important to develop methods of their qualitative and quantitative assessment.

At present when ice certificates are devised the results of theoretical studies of the ice capabilities of the ship are used together with full-scale test data. In the course of full-scale tests ship ice performance and strength are studied as well as ship manoeuvrability, inertial characteristics under different ice conditions; the experience of ship captains is also taken into consideration, and all known ice damage events. The tests include a study of several sister-ships in different geographical areas and at different periods of a navigation season.

The main criteria determining the ship efficiency in the ice and which constitute the essence of the ice certificate are safe limit speed, minimum safe distance in the convoy and the hull strength, i.e. its ability to withstand ice pressure.

The safe limit speed is the maximum speed under given ice conditions in autonomous navigation or in the icebreaker wake which ensures safe navigation. It is determined by the so-called power limit speed and strength limit speed (PLS and SLS).

The power limit speed is the ship speed under given ice conditions and at full capacity of the propulsion plant. It depends on the power, principal dimensions, hull shape lines and the ice conditions. The ship operation in the ice with power limit speed, however, is not always safe, the ship can be damaged by ice. In such cases the captain should reduce the power in order to lower the speed, this would lessen the danger of ice impact. The speed in the ice which guarantees safe navigation is called the strength limit speed. It depends on the ship mass, hull shape lines, strength of the hull and the ice conditions in the area of ice impact.
Thus, the power limit speed is determined by ship's performance in the ice and the strength limit speed - by the strength of the hull. These two speeds are estimated in accordance with the methods developed in the AARI Ice Research Laboratory on the basis of full-scale test results and long-term operation of ships under ice conditions.

The safe limit speed is found from the relationship between the power limit speed and strength limit speed. The simplest and the most vivid way of doing this is by means of graphic diagrams. At one diagram the curves of PLS and SLS as functions of a most typical ice parameter, for instance ice concentration or thickness, are plotted. The safe limit speed is found as the minimum of SLS and PLS. To enable the captain to use the full power of the ship without any fear for damage of SLS, it should be somewhat larger than the PLS, the latter in this case determine safe limit speeds. This is possible, however, if the power of the ship and her hull strength are compatible. Fig. 1 offers us an example of this situation. The safe limit speed for broken ice with concentrations from 0 to 2,5 and from 7,5 to 10 (single hatching) is determined by the PLS while for the concentrations from 2,5 to 7,5 it is determined by the SLS. When operating in ice conditions defined by double hatching the captain should use caution and even decrease the speed down to the value determined by the lower line.

It should be noted here that the rational design of the hull shape lines of the bow would lead to diminishing ice pressures on the hull and at the same time to improve ship's performance in the ice, other things being equal, thus increasing the safe limit speed of the ship in the ice.

Distance between ships in the convoy is the distance between the icebreaker stern (or the stern of any other leading vessel) and the bow of the following ship. The largest speed in the ice convoy the ship is known to have if she follows the icebreaker at a close distance. In this case the channel is not filled with broken ice yet and the channel itself is broader, particularly if the convoy moves in the drifting broken ice. As the distance between the icebreaker and ship increases, the ice concentration in the channel also increases and the channel tends to close. A very small distance, however, is dangerous, since a stop short of an icebreaker would or might result in a collision as the ship following the icebreaker is not able to completely stop (due to inertia).

The safe distance between ships in ice should be chosen in such a way that ensures effectiveness of the convoy if the distance is equal to the minimum distance between the icebreaker and the ship which excludes the possibility of collision in case of an icebreaker stop short. This distance depends on ice conditions, convoy speed and inertial ship characteristics needed to manoeuvre in the channel.
(e.g. reverse, stop). The schematic diagramme for the determination of the minimum safe distance in the convoy is shown in Fig. 2. It follows from this diagramme that for the passage through a channel made in the ice $h_1$ thick with a speed of $V_1$ the safe distance in the convoy is $l_1$.

Heavy pressure in thick ice is known to be dangerous for the hull of ships of UL and L1 class (for L1 class in particular). In such a case even the crushing of ice edges around it by a heavy icebreaker would be useless and the ship would continue to drift with the ice under pressure. The criterion of safe operation in these cases can be the maximum ice thickness, whose pressure the ship hull can withstand.

To ensure the failsafe convoy it is necessary in each individual case to objectively estimate the desirability of icebreaker convoy. Obviously the icebreaker convoy should be arranged only if it facilitates ice passage. This is possible if under given ice conditions the icebreaker is able to develop the speed equal or higher than the ship's safe limit speed without an icebreaker. The optimum convoy case is when icebreaking speed (passage making) of an icebreaker operating almost in a full power regime, and the ship's speed in the channel are almost equal.

The ice certificates for various cargo carriers are being developed in the Ship Ice Research Laboratory of the Arctic and Antarctic Research Institute since 1972. At present this certificate has been generally approved, since it contains complete information for solving major practical problems of ice navigation.

The Ice Certificate contains the following information:

1. The list of ships for which Ice Certificates have been already made with the name of the ship, her date of launching and home port.

2. Table giving main particulars of the ship, her icebreaking capabilities, ice class by the USSR Register, main dimensions, coefficients and features of hull lines, ice strengthening information and propulsion plant characteristics, data on screw, rudder and their ice protection and the like.

3. Diagrammes for determination of safe limit speed in autonomous ice operations. These diagrammes include those ice forms in which the ship of a given class can move without an icebreaker. These are: fast ice, giant, vast and big floes, though not very thick and preferably snow bare, as well as not pressurised ice cakes (see Fig. 1).

4. Diagrammes for the determination parameters of safe icebreaker convoy arrangement, defining safe limit speed and safe minimum distance in the convoy.
5. Diagrammes for the estimation of practicability of the convoy by existing arctic icebreakers and the choice of the regime of ship's propulsion plant. All the diagrammes are given for the operations in full-load and in ballast.

6. The recommendations to the captain emphasize the difficulties of ice navigation.

The contents of the Ice Certificate cannot include the complete variety of ice conditions, nor can the recommendations foresee everything. Thus the captain in each case should find the best possible mode of operation using the ice certificate as the basis.

Ice Certificates are successfully adopted into ice navigation practice, they are highly praised by the captains and ship-owners, they are also widely used by ice ship designers.

FIGURE CAPTIONS

Figure 1 - The diagramme for the determination of safe limit speed in the autonomous operation in ice cake: 1 - PLS, 2 - SLS.

Figure 2 - The diagramme for the determination of safe distance in the convoy moving in the channel made in medium and small floes of different thickness $h$ ($1 - h = h_1; 2 - h = h_2; 3 - h = h_3; 4 - h = h_4; 5 - Open water$) at full power of the propulsion plant.
Fig. 1. The diagram for the determination of the safe limit speed in the autonomous operation in ice cake:

1 - PLS, 2 - SLS
Fig. 2 The diagram for the determination of the safe distance in the convoy moving in the channel made in medium and small floes of different thickness $h (1 - h = h_1; 2 - h = h_2; 3 - h = h_3; 4 - h = h_4; 5 - Open water )$ at the full power of the propulsion plant.
DEVELOPMENT AND IMPLEMENTATION
OF SHIP ICE CERTIFICATES
BY: D.D. Maskutov and Yu. N. Popov
Vol. III

DISCUSSION BY:
Audience

1) Is figure 1 a schematic diagram only or have you used full-scale data?

2) Are these ice certificates valid in other countries, or in Soviet Union only?

AUTHORS' REPLY

For the first question, we have used calculations and also full-scale data.

For the second question. We don't know, but as well as we know - in Soviet Union only.
LA TELEMESURE DE L'ÉPAISSEUR
DES GLACES DE MER À L'AIDE DE RADAR

Maurice Audette, ing.  
Agent principal de développement des transports  
Centre de développement des Transports Canada

Résumé
A la demande de la Garde côtière canadienne, le Centre de développement des transports (CDT) entreprenait en 1977 une étude de faisabilité sur la télémétrie de l'épaisseur des glaces de mer. Ces travaux préliminaires ont conduit au choix du système tel que proposé par MPB Technologies, Inc. de Montréal, c'est à dire un radar VHF combiné à un radar à impulsion synthétique.

L'équipement devait mesurer des épaisseurs de glace variant de 0.3 à 6 m et être opéré d'un hélicoptère. On devait également considérer la possibilité de l'utiliser à bord d'avion de patrouille à long rayon d'action.

Le présent article décrit le processus de développement, les résultats obtenus en laboratoire et ceux de la mer de Beaufort. Tout laisse croire que nous aurons un instrument au point pour 1985.

Introduction
Le besoin se fait de plus en plus urgent pour un instrument qui permettrait de mesurer, à distance et en tout temps, l'épaisseur de la glace de mer. Un tel instrument serait un atout précieux soit avec les hélicoptères de la Garde côtière canadienne, soit à bord d'avions de patrouille à long rayon d'action. De l'information sur la distribution et l'épaisseur de la glace de mer faciliterait la navigation parmi les glaces, permettrait d'allonger la saison des opérations en Arctique, résulterait en une sécurité accrue pour les navires et améliorerait la rentabilité du transport des nombreuses ressources du grand nord vers les marchés du sud.
Historique
En 1977, le Centre de développement des transports (CDT) de Transports Canada fut mandaté par la Garde côtière canadienne pour réaliser un programme de recherche visant à développer un système de télémétrie de l'épaisseur des glaces de mer. Il fallait avant tout identifier une approche plausible. Ensuite le processus de développement suivrait son cours normal soit la construction d'un prototype expérimental, la vérification des possibilités de l'équipement en laboratoire, la conduite d'essais sur place dans le grand nord et enfin le développement d'un prototype opérationnel.

Avant d'établir un programme précis de recherche, on définissait une spécification type basée sur les besoins opérationnels de la Garde côtière et du Service de l'environnement atmosphérique (SEA). De nombreux échanges eurent lieu entre les différents organismes impliqués tels le Centre de Recherches sur les Communications (CRC), le Centre Canadien de Télédétection (CCRS), le Service sur l'environnement atmosphérique et la Garde côtière canadienne (GCC). Le résultat est représenté par la spécification type au tableau No. 1.

Tableau No.1: Spécification type - Instrument de télémétrie de l'épaisseur des glaces de mer (pour hélicoptère)

<table>
<thead>
<tr>
<th>Critères de performance</th>
<th>Minimum requis</th>
<th>Valeurs recherchées</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Temps</td>
<td>presque tous</td>
<td>tous</td>
</tr>
<tr>
<td>2. Épaisseur de glace à mesurer (m)</td>
<td>0.5 - 3</td>
<td>0.3 - 6</td>
</tr>
<tr>
<td>3. Précision de la mesure (le plus grand % ou cm)</td>
<td>± 15</td>
<td>± 10</td>
</tr>
<tr>
<td>4. Changement d'épaisseur minimale à résoudre (cm)</td>
<td>15</td>
<td>10</td>
</tr>
<tr>
<td>5. Résolution horizontale (m) @ (m) altitude</td>
<td>15 @ 30</td>
<td>10 @ 30</td>
</tr>
<tr>
<td></td>
<td>100 @ 200</td>
<td>100 @ 300</td>
</tr>
<tr>
<td>6. Echantillonnage à tous les (m)</td>
<td>15</td>
<td>10</td>
</tr>
<tr>
<td>7. Poids maximum à bord (kg)</td>
<td>50</td>
<td>25</td>
</tr>
<tr>
<td>8. Altitude (m)</td>
<td>30 - 200</td>
<td>30 - 300</td>
</tr>
<tr>
<td>9. Vitesse jusqu'à (noeuds)</td>
<td>150</td>
<td>200</td>
</tr>
</tbody>
</table>
La spécification établisait aussi que l'instrument serait facilement démontable, que la présence d'un technicien qualifié ne devait pas être requise pour interpréter les résultats, et que l"output" serait de forme digitale et compatible avec un microprocesseur.

Le programme de recherche tel que défini comprenait trois phases: (1) identifier une technique de mesure probable et en vérifier les principes, (2) développer un prototype opérationnel, (3) soumettre ce prototype à des essais rigoureux pour en évaluer la valeur. La spécification type et le programme ainsi définis servirent de base pour un appel d'offre. La compagnie MPB Technologies, Inc. fut choisie comme la plus propice pour mener à bien le programme envisagé.

En 1978, une étude analytique confirmait la possibilité d'un instrument opérant dans la bande VHF. Il s'agissait d'une conception nouvelle permettant de mettre à profit un radar à impulsion modulée avec un radar à impulsion synthétique. Un prototype expérimental fut mis au point en laboratoire et au mois d'avril 1980 une série d'essais dans la mer de Beaufort permit de conclure que l'instrument pouvait donner des résultats satisfaisants.

Les études préliminaires
Bien que plusieurs techniques aient été essayées, la mesure de l'épaisseur des glaces de mer n'a jamais donné de résultats satisfaisants. Les systèmes acoustiques/seismiques, électromagnétiques passifs et même les systèmes plus récents tel le radar holographique se sont avérés soit trop limités soit trop compliqués. Pour ce qui est des systèmes électromagnétiques actifs, ils sont les plus prometteurs malgré le caractère imprévisible de la constante dielectrique de la glace marine.

Les radars à impulsion modulée développés pour mesurer l'épaisseur des glaces d'eau douce ne donnaient pas les résultats espérés avec la glace marine, il fallait chercher une autre technique. En Russie, des chercheurs réussirent à obtenir des résultats intéressants avec un radar à impulsion modulée de 50 nanosecondes sur une onde portée de 100 MHz. Ils rapportèrent avoir mesuré des épaisseurs de 1.5 mètres et plus. Pour les glaces plus minces, les mêmes chercheurs se servirent avec succès d'un radar à impulsion synthétique. Cette information nous aida grandement à délimiter notre choix.
Le système choisi
Pour les épaisseurs de glace de 2 mètres et plus, il fut convenu d'utiliser un radar à impulsion conventionnelle sur une onde porteuse entre 100 et 150 MHz. Ce choix est possible parce que le temps requis pour que l'onde traverse la glace permet une impulsion conventionnelle et il est possible d'utiliser les techniques de traitement des signaux telles que développées pour radar d'eau douce. Il est à noter qu'à ces fréquences la dimension de l'antenne n'est pas encore trop encombrante et il est possible de la transporter par hélicoptère.

Pour les épaisseurs inférieures à 2 mètres, on choisissait un radar à impulsion synthétique. Un tel choix est impératif parce que le temps requis pour que l'onde électromagnétique traverse un mètre de glace est si court (typiquement 10 à 20 nanosecondes) qu'il faut une impulsion inférieure à 10 nanosecondes pour pouvoir distinguer les signaux permettant de mesurer l'épaisseur de la glace. Or, une telle impulsion nécessite une antenne des plus efficaces sur une très large bande de fréquences. Sans quoi, l'antenne modifierait l'impulsion et cacherait ainsi l'information recherchée. Avec une impulsion synthétique, on peut se servir de filtre pour former l'impulsion et traiter les signaux.

Les études conduisirent à la mise au point d'un instrument de télémétrie opérant sur la bande de fréquence VHF et fonctionnant selon deux modes d'opération. Le premier mode consiste en la transmission d'impulsions modulées en amplitude d'une durée égale à 50 nanosecondes, répétée à la fréquence de 50 KHz sur une onde porteuse de 100 MHz. Le deuxième mode transmet une série d'impulsions vidéo de durée égale à 7 nanosecondes et une période de 200 nanosecondes. Cette série d'impulsions est générée par la combinaison de signaux harmoniques de 30, 60, 90, 120 et 150 MHz. Pour les épaisseurs de glace inférieures à 2 mètres, cette combinaison permet de compenser pour la dispersion à travers la glace et de donner des échos plus clairs et plus facilement identifiables.

Le processus de développement
Une fois le choix arrêté sur une approche définie, il fallait étudier le problème plus à fond et formuler un design approprié. Au fur et à mesure que les différentes possibilités étaient approfondies, plusieurs contraintes s'avéraient incompatibles telles les épaisseurs à mesurer et la puissance permise. Il fallait alors relâcher quelque peu ces critères de la spécification. De plus, il devint évident qu'il fallait limiter les travaux au développement d'un instrument pour hélicoptère quitte à reprendre plus tard pour l'instrument d'avion de patrouille.
On entreprenait alors la construction de circuits électroniques, la sélection et l'acquisition d'une antenne, la poursuite d'essais en laboratoire avec de la glace marine simulée, et la conduite d'essais préliminaires en préparation pour l'Arctique.

En laboratoire, on conduisit un grand nombre d'essais en remplaçant l'antenne par une ligne de transmission. Il était possible ainsi d'introduire des discontinuités à volonté pour mettre au point les caractéristiques transitoires des circuits électroniques. Par la suite, cette même ligne de transmission fut immergée dans une colonne d'eau salée qu'il était possible de geler à volonté. Les résultats obtenus démontrèrent que le radar à impulsion synthétique pouvait servir à mesurer une épaisseur de 0.5 mètre et plus de glace marine et que le radar à impulsion modulée se comporterait comme prévu.

Une colonne de glace saline de 1.6 mètre fut créée en maintenant une chambre froide à -50°C. Avec un chauffe-eau électrique, il fut possible de maintenir le bas de la colonne à l'état liquide. Tout au long de la colonne des thermocouples mesuraient la température de façon continue. En variant la température de la chambre froide, il devenait possible de simuler les conditions désirées. Les mesures obtenues démontrèrent que l'écho de l'impulsion synthétique ne contenait pas de distorsion sévère. On obtenait deux signaux réfléchis bien distincts avec une colonne de glace de 160 cm. Lorsque la colonne de glace était moindre que 50 cm les deux impulsions commençaient à se marier pour n'en faire qu'une. Toutefois, tel que rapporté par les chercheurs russes, il demeure possible d'évaluer les épaisseurs inférieures à 50 cm si l'on compare la largeur de l'impulsion reçue à celle de l'impulsion émise. Les résultats confirment aussi que plus la glace est froide plus l'on reçoit électromagnétique circule vite: soit 13 cm/nanosecondes à -10°C et 16 cm/nanosecondes à -30°C. La figure No. 1 est un exemple typique des signaux obtenus au cours de ces essais.

L'obtention d'une antenne appropriée fut un des problèmes techniques des plus difficiles à résoudre. Le choix initial était pour une antenne de type "Log-periodic". On réussissait par la suite à faire le design d'une antenne qui répondait à la presque totalité de nos besoins. Cependant, la connaissance des caractéristiques transitoires de l'antenne en question nécessita des investigations très poussées.
Quant à la conduite d'essais préliminaires en préparation pour les essais dans l'Arctique, elle présenta un assez grand nombre de difficultés. Pour pénétrer la glace marine, il fallait choisir des fréquences entre 30 MHz et 150 MHz. Or, dans les grandes villes, la radio FM, les postes de télévision et les radio 'CB' y sont déjà très actifs et certains avec beaucoup de puissance. Avec un petit émetteur de moins de 1 watt, il n'est pas surprenant que l'interférence électromagnétique fut sérieuse. Il fallait donc prendre des précautions spéciales et le plus souvent faire nos essais la nuit.

Malgré ces inconvénients, il fut possible de vérifier que la sensibilité des équipements était adéquate pour répondre aux besoins. On put également établir que la même antenne pourrait servir pour les deux types d'impulsions. Toutefois, des modifications à l'électronique devinrent nécessaires afin de compenser pour certaines modifications des signaux causés par l'antenne.

Les premiers essais en hélicoptère, un Bell 206L, eurent lieu à 40 km de Montréal les 17 et 19 mars 1980. On survola des glaces sur la rivière Outaouais et sur le fleuve St-Laurent. Malgré une interférence électromagnétique sèvère, il fut possible d'obtenir des mesures d'épaisseur de glace.
Les essais en mer de Beaufort

Pour la dernière étape du développement du prototype expérimental, on conduisit des essais dans l'Arctique canadien. Afin d'en assurer le succès on sollicitait l'aide de spécialistes tels que C-CORE, Dome Petroleum (Canmar) ainsi que celle du Projet sur le plateau continental polaire (PCSP) du Ministère de l'énergie, mines et ressources du Canada à Tuktoyaktuk, Territoires du Nord-Ouest. MPB Technologies, Inc. avait la responsabilité technique du projet alors que C-CORE, Canmar et PCSP se chargeaient de la planification, de la consultation, de la vérification au sol et de tout support logistique.

Les essais en mer de Beaufort eurent lieu entre le 1er et le 14 avril 1980 et on se servit de trois emplacements spécifiquement préparés:

a) Glace saumâtre (brackish) 8 km à l'ouest de la base, 4 et 6 avril;
b) Glace de première année 75 km au nord-est de la base, 7 et 9 avril;
c) Glace de plusieurs années 300 km au nord de la base, 8 et 14 avril.

Une fois l'équipement à bord de l'hélicoptère Sikorsky S55, une calibration initiale fut suivie d'essais à 90 m et à 240 m au-dessus de chacun des sites. Seuls les résultats à 90 m permirent de mesurer l'épaisseur de glace. À 240 m, l'empreinte du signal était trop grande et les signaux trop brouillés pour donner des résultats fiables et compatibles avec les épreuves prises au sol.

Glace saumâtre: la surface de cette glace était relativement plane et peu couverte de neige, soit de 0.1 à 0.5 m d'épaisseur. Les épreuves au sol étaient quasi-uniformes entre 1.4 et 1.6 m. La salinité mesurée était moindre que 0.5%. L'atténuation anticipée devait donc être faible et permettre une bonne réflexion du fond de la glace. La figure No.2a présente une trace de l'écho obtenu avec le radar à impulsion synthétique.

Glace de première année: pour la glace de première année on utilisait trois tracés distincts. Tous étaient à quelques kilomètres les uns des autres. Au site No.1, l'épaisseur variait de 1.6 à 1.8 m. La température était d'environ -5°C et la salinité variait entre 4 et 6%. La figure No.2b indique un délai de 21 nanosecondes entre les deux échos. Ceci est en accord avec 1.6 mètre de glace. Au site No.2, l'épaisseur était d'environ 1.1 m, tandis qu'au site No.3 elle était d'environ 0.3 m. Un exemple des résultats obtenus au site No.2 est présenté à la figure No.2c. Pour ce qui est du site No.3 il ne fut pas possible de résoudre 0.3 m.
Glace de plusieurs années: encore une fois nous utilisons trois sites bien distincts à quelques 300 km au nord de la base. L'épaisseur de la glace variait entre 4 et 7 mètres. La figure No.2d est typique des résultats obtenus. Il est à noter que beaucoup de fluctuations avaient lieu dans les échos reçus.

Figure No.2 - Exemples de résultats obtenus en Arctique
Tableau No.2: Résumé des résultats de l'Arctique (Altitude 90 m)

<table>
<thead>
<tr>
<th>Type de glace</th>
<th>Site No.</th>
<th>Salinité (‰)</th>
<th>Mesure/radar (m)</th>
<th>Contrôle (m)</th>
</tr>
</thead>
<tbody>
<tr>
<td>A. Saumâtre</td>
<td>-</td>
<td>0.1 @ 0.5</td>
<td>1.4 @ 1.5</td>
<td>1.4 @ 1.6</td>
</tr>
<tr>
<td>B. Première année</td>
<td>1</td>
<td>4 @ 6</td>
<td>1.6 @ 1.8</td>
<td>1.6</td>
</tr>
<tr>
<td></td>
<td>2</td>
<td>6</td>
<td>1.1</td>
<td>1.2</td>
</tr>
<tr>
<td></td>
<td>3</td>
<td>6</td>
<td>0.3</td>
<td>0.75</td>
</tr>
<tr>
<td>C. Plusieurs Années</td>
<td>4</td>
<td>-</td>
<td>5 @ 7</td>
<td>5.0 @ 7.9</td>
</tr>
<tr>
<td></td>
<td>5</td>
<td>-</td>
<td>5 @ 7</td>
<td>4.2 @ 7.6</td>
</tr>
<tr>
<td></td>
<td>6</td>
<td>-</td>
<td>5 @ 7</td>
<td>4.2 @ 7.6</td>
</tr>
</tbody>
</table>

Conclusions

Le résumé présenté au tableau No.2 démontre clairement que l'instrument utilisé peut mesurer l'épaisseur des glaces marines. Il est donc permis de conclure que nous avons atteint le principal objectif du programme, soit l'identification et la vérification d'une technique valable. Certes, il n'est pas encore possible de satisfaire à toutes les exigences de la spécification mais les résultats obtenus sont convainquants et permettent de croire en la possibilité d'un instrument des plus utile.


Références:

1. Development of a Remote Sea-Ice Thickness Sensor
   - TP 1277 - Recommended Programme - November 1977
   - TP 1573 - Antenna and Horizontal Resolution Studies - June 1978
   - TP 1574 - Electronics Design - June 1978
   - TP 2677 - Sea-Ice Thickness Radar Measurements in the Beaufort Sea - Dec. 1980
Question No. 1: Have you noticed any characteristics of the radar signal which may be used to differentiate first-year and multiyear ice?

Answer: The work undertaken to date has not given much attention to the above question. Our prime objective was to develop a technique which would allow us to obtain some estimate of sea-ice thickness from an helicopter. It is realized that knowing the type of ice the readings obtained would be more reliable.

However, from observations it is possible to state that first-year ice tends to have high absorption rate. Consequently, a large amplitude difference between the top and the bottom echo signal associated with a short time delay usually indicates the presence of first-year ice. Less amplitude difference and/or long delays can be indicative of multiyear ice.

In any event, our results tend to indicate that not knowing the exact dielectric characteristics of the ice introduces a 10 to 20% error in the estimated ice thickness. Since the main purpose of the instrument is to find a path of least resistance in a given ice field for the ice-breaker to proceed, the error in thickness measurement is not too inconvenient for the time being.
Question No. 2: What form of output does the user desire?

Answer: The user requires a digital output such that the ice observer can read the thickness directly on a digital display.
ABSTRACT

Traditionally many types of remote surveillance have produced vast amounts of qualitative data, such as photographic, radar, and sonar imagery. Interpretation of these data sets is highly subjective, and it is usually not possible to extract the actual parameters of interest easily. The advent of reliable, portable, high speed digital computers is making it possible to process data in a quantitative manner. The result of this revolution is that it is becoming possible to answer directly questions posed by users, and to determine "best" numerical estimates of target characteristics.

In this paper some of the important aspects of quantitative modelling of remote sensing data are reviewed - the formalism of general inverse theory, the effect of noise, the improvements achievable by use of wide bandwidths and multiple sensors. It becomes clear that a sound understanding of the physical relationships between observations and parameters of interest is essential. Several examples relevant to arctic and ocean problems are presented.
"...in glaciology, as in other branches of science, there is a place for both the theoretical and the experimental approach. But the two should be coordinated; the experiments designed to investigate specific problems. Too often in the past, glaciological measurements have been made on the premise that the mere acquisition of data is a useful contribution in itself. This is seldom the case."

- W.S.B. Paterson [1]

INTRODUCTION

There are many types of situation for which the only available information is that obtained remotely. These situations include, for example, almost all astronomical observations, most types of geophysical measurement, and increasing numbers of ocean related studies. A complete field of remote sensing has grown up from the days of aerial photography, and it now includes satellite observation using a host of techniques and technologies. In this paper "remote sensing" will refer generally to observations made at some distance from a target of interest using indirect means.

Remote observations have often provided only qualitative data; for example, photographic or infrared imagery. Useful as these types of data are, they do not lend themselves to modern analytical techniques. However, it is now possible to collect large amounts of digital data and to process the data quickly on high speed computers. This fact has resulted in a re-examination of ways in which remotely sensed data are analysed in order to extract the full range of information that they can contain. This move toward quantitative remote sensing will, I believe, result in a quantum step in the effectiveness of many remote sensing techniques.

Because of the harsh conditions faced in arctic areas, and the vastness of the oceans, remote surveillance is already playing an important role in understanding these environments and in monitoring operating conditions. Often the only information addressing many
of the problems we are trying to solve comes from remotely sensed data. Therefore it is only sensible that we collect and use these data as well as possible.

My goal in this paper is to review some aspects of quantitative remote sensing. I will use examples with which I am personally familiar, although there are many others. Unfortunately, I cannot claim that the paper presents an exhaustive compendium of all aspects of this topic, nor that the examples are the most appropriate. However, I hope that I will be able to demonstrate some of the principles of quantitative measurement, the processing that can be performed on quantitative data and the kinds of additional information that can be extracted.

**SIGNS AND NOISE**

Any remote sensing technique, by definition, receives information from a distance, and therefore is, at least in part, a communication problem. One of the fundamental theorems of communication theory, the Hartley-Shannon Law, is given by [2]:

\[ C = W \log_2 \left( \frac{1 + S}{N} \right) \]  

where  
- \( C \) = channel capacity (bits s\(^{-1}\))  
- \( W \) = bandwidth (Hz)  
- \( S \) = signal power  
- \( N \) = additive noise power

Equation (1) implies that if \( N=0 \), one can transmit and receive an infinite amount of information with any bandwidth. But of course noise is never zero, and the effective information rate available through any given information channel is limited by the bandwidth of the channel, the noise present and the available signal strength. This relation is usually true for ambient, environmental, or thermal noise. In principle one can exchange bandwidth for signal-to-noise. However, there are many other types of noise that must be contended with before we can make optimum use of remotely sensed data.
Some examples are:
- distortion of the signal through the propagation medium (atmosphere, ocean, etc.)
- motion of the detector (or of the target)
- unknown variations in source signal characteristics
- uncorrected distortion in instrument electronics or processing procedures
- clutter, scattering, or reverberation received from the background against which detection is being attempted.

Note that none of these types of noise can be improved one whit by increasing the signal strength; i.e. they represent multiplicative rather than additive noise. In order to obtain more information from our expensively collected data, we must apply more subtle techniques - by directly reducing the sources of noise where possible and by examining the data using techniques that explicitly allow for such noise.

Example - Towfish Body Motion Compensator

An example of removal of body motion effects is the body motion compensator of the Huntec Deep-Tow Seismic Profiling System (DTS). The towfish moves up and down vertically by up to several metres under rough sea conditions. This motion limits the resolution of the received signal which is otherwise on the order of $0.10 \text{ m}$. However, by adjusting the timing of the transmitted pulse so that it fires earlier or later depending on the altitude of the fish relative to a datum the full information of the signal is retained [3].

The motion detection is inexpensively and reliably achieved using a pair of complementary motion sensors in the towfish: a pressure transducer which measures low speed vertical movements, and a vertical accelerometer which measures rapid fluctuations (heave). These two are combined to give corrected output over a wide range of conditions.

**Inverse Theory**

There are general theoretical techniques for analysing noisy or incomplete data. A number of excellent papers have been written in the past few years which describe the formalism and its application
to specific problems [4] [5] [6]. It is not my purpose to provide a tutorial on inverse theory here, but I will outline a framework.

In general terms we face the following situation:

\[ y = Ax \] (2)

in which \( y \) is a set of observations, \( x \) is a set of parameters to be determined, and \( A \) is the known functional relation between \( y \) and \( x \).

The forward problem is to discover \( A \) and hence be able to solve (2) for an unique \( y \), given \( x \). We will assume for the moment that this can be done.

Following the format of Jackson [4], equation (2) can be described in a linear, discrete matrix form, so that \( y \) is a vector of length \( n \); \( x \), a vector of length \( m \); and \( A \), an \( n \times m \) matrix. This linear form can often be applied to non-linear functionals that are nearly linear over some range of interest.

Our goal is of course to solve the inverse problem: what estimated parameters \( \hat{x} \) are "best" described by our observations \( y \), knowing full well that the \( y \) are imprecise, and that our estimate may not be unique.

In matrix form we want to find matrix \( H \) (\( m \times n \)) so that

\[ \hat{x} = HAx = Hy. \] (3)

We would like to determine an \( H \) so that:

(a) our model fits the data available, i.e. \( AH \approx I_n \) (the \( n \times n \) identity matrix);
(b) our solution is, in some sense, unique, i.e. \( HA \approx I_m \);
(c) the variance in our estimate, \( \text{var}(\hat{x}) \), is not too large.

In general the requirements of best fit and low variance are not simultaneously achievable. Hence an uncertainty principle operates - we can either estimate \( \hat{x} \) closely and accept large variance, or we can choose poorer resolution in \( \hat{x} \), but lower variance. Naturally the "best" estimate is based on an understanding of the trade-off for the particular problem being investigated. The important point is that a solution based on the assumption of perfect data may give incorrect results, either because the resolution is implicitly weighted by the least constraining observations, or by numerical instability in the computations [7]. It was assumed in equation (2) that the
functionals, \( A \), are well known, preferably defined by a sound physical relationship. One limitation to quantitative interpretation of many remotely sensed data is that the physical relationships between the observables and the parameters of interest are not always well understood. Therefore, although work in understanding these relations may, at first blush, seem academic, it can lead to great strides forward in quantitative interpretation of practical results.

Example - Ice Thickness Sounding by Impulse Radar

Let me illustrate with a simple example based on work carried out at the Centre for Cold Ocean Resources Engineering in Newfoundland using impulse radar to estimate the thickness of sea ice and icebergs [8] [9]. A radar pulse is sent into the ice and the travel time through the ice and back can be measured. Using the format of equation (2):

\[
 t = \left( \frac{2}{v} \right) d 
\]

(4)

where \( t \) is the measured time delay,

\( v \) is the speed of the pulse in the ice,

and \( d \) is the ice thickness to be estimated.

Obviously if the speed of the signal is well known and is not a function of frequency, we have a trivial, one-dimensional, one-parameter inverse problem - the variance in \( \hat{d} \) will be limited only by our ability to measure \( t \). This situation probably applies to fresh water ice - on rivers, lakes and in icebergs.

Sea ice is not so simple [10]. The speed of an electromagnetic wave in sea ice depends weakly on the frequency and rather strongly on the brine volume in the ice, which depends, in turn, on the temperature and salinity of the ice. Therefore, in order to estimate \( \hat{d} \), we need to specify \( v \).

Several approaches have been suggested for doing this [11] [12]. The most promising technique, which is to also estimate the attenuation of the signal through the ice to estimate \( v \) [13] [14], has not yet been implemented operationally. There are no conceptual difficulties in processing the data (this is possible even in real-time with modern micro-computers), but there is not complete understanding of the many factors that contribute to the attenuation of radio waves in ice, such as dielectric absorption of the signal energy, scattering both at the surface and by volume processes within the ice, and unusual
anisotropy effects (Figure 1) [15] [16] [17].

Figure 1. Magnitude of the reflections of an impulse radar signal taken from a helicopter over 1.15 m thick first year ice in the Beaufort Sea. The upper trace is from the air-ice interface; the lower, the ice-water interface. The horizontal axis represents approximately 700 m of travel distance. The large variations in amplitude are caused by many effects - e.g. interference due to snow cover, variable attenuation in the ice, and scattering. Although in principle these results can be inverted to give ice thickness and properties, in practice, a more detailed understanding of the various mechanisms of attenuation may be required [Ref. 14].

The point is not that understanding the physics of sea ice is the only barrier to remote sounding using radar; of course there are significant problems of practical instrument design and effective processing of results. However, by casting the problem into a general inverse format the weakness in our knowledge of the underlying physics becomes readily apparent. The case is even worse for inferences of ice thickness from ice surface characteristics. Since there is no known quantitative physical relation between the observables and the parameter of interest the results are perforce qualitative.

**BANDWIDTH**

It almost seems axiomatic that any remote surveillance technique should be designed to utilize as wide a bandwidth as possible. In effect, a range of frequencies often introduces a range of independent
observations. Wide bandwidth can be accomplished in several ways; for example, by using short, impulsive sources, by sweeping through a range of frequencies (e.g. "chirping"), or by using a number of discrete frequencies (see [18] for an interesting discussion). The choice depends usually on the exact nature of a specific application. The important aspect is that a narrow bandwidth provides relatively little intrinsic information.

Example - Sonogram Analysis of Seabed Sediments

The use of the wide-band impulse radar for probing sea ice has already been mentioned. A similar example is the successful use of a boomer source high resolution, sub-bottom seabed profiler which is towed near the sea floor [19]. Sonograms, which display the amplitude of the received echo on a contour plot as a function of time and frequency, have been used to interpret attenuation in the upper 10 to 15 m of the seabed [20].

An example is shown in Figure 2, showing the bottom and sub-bottom reflections. By taking spectra of these two echoes (Figure 3) the average sediment attenuation can be extracted. After excluding portions of the spectral curves that contain anomalies due to high noise or scattering effects, the attenuation in the layer as a function of frequency can be estimated. As shown, the straight line indicates attenuation proportional to frequency - consistent with theory of anelastic losses in sediments. This type of analysis has been used to compute attenuation rates for a number of sediments, and differences between these in situ values and previous laboratory measurements are believed to be significant.

The work is now continuing to examine surface and volume scattering. Since these phenomena are likely to have a very different frequency dependency compared to attenuation, e.g. (scattering by small scatterers is proportional to the fourth power of frequency), it appears possible to explicitly model both attenuation and scattering. In addition, it appears that surface irregularities are often larger than internal volume scatterers. Hence surface scattering can be examined independently by analysis of shot-to-shot coherence [21] [22]. The key to these types of analysis is use of a well-characterized wide-band source.
Figure 2. A 30-shot stacked sonogram showing amplitude (in nepers) as a function of time and frequency for high resolution acoustic profiler sounding seabed sediments off the Canadian East Coast. The first arrival and sub-bottom echoes at 8 and 19 ms are shaded. Spectra at time 0 and 7 to 9 ms are shown in Figure 3. [Ref. 20].

Figure 3. (a) Spectra from Figure 2 for the seabed echo, the 8 ms "target" arrival, noise before the first arrival, and scattered energy near the target arrival.

(b) Ratio spectra of target/bottom power showing the range of validity for an attenuation estimate which is based on the slope of the straight line indicated. [Ref. 20].
Example - Non-linear Acoustics

An interesting method for generating wide-band acoustic signals is the use of non-linear acoustic effects, or so-called parametric sources. The concept utilizes the non-linear propagation properties of compressional waves at high intensities, in which the positive and negative half-cycles have different propagation speeds. Driving a transducer at high intensity with two closely spaced primary frequencies sets up a pattern of interacting waves which constitute a tapered line array of virtual sources along a collimated beam, so that the effective propagation is at the secondary difference frequency [23]. The result is a narrow beam-width with no side lobes from a physically small aperture, and wide relative bandwidths can be obtained merely by varying the difference frequency. The price paid is the low efficiency of conversion from the primary to the secondary.

A prototype is being developed at Hunttec which contains a parametric source in a confined tube [24]. It is being designed in order to make bathymetric measurements through an ice cover and simultaneous ice thickness measurements. However, it is likely that the device will have application to studies of sea ice and to detection of oil in or under ice. It has potentially very high resolution because pulses as short as 10 μs (approximately 1 cm in ice) can be used, and the insonified spot size is relatively small, even over a range of frequencies from 15 to 120 kHz (Figure 4).

Just as in the sediment analysis example given previously, the use of wide bandwidth has potential for examining a wide range of ice properties in situ. For example, it is possible that some of the questions about the structure of brine channels, the location of air pockets and the consolidation of broken ice might be resolved by analysis of their acoustic signatures as a function of frequency.

MULTIPLE SENSORS

The section on inverse theory indicated the importance of independent observations. We have seen how this can be accomplished using a range of frequencies. Another approach is to use data from a variety of complementary sensors. If there is a unifying theory relating the several types of data, the inverse formalism described earlier can be appropriate. However, even when the detailed relationships between
Figure 4. Radiated spot-sizes measured 62 cm from the end of a non-linear acoustic source in 20 cm diameter, 60 cm long tube, using a 500 kHz, 10 cm diameter transducer. Measurements are for four different frequencies normalized to axial pressure levels. Note relatively narrow spot size and the absence of side lobes [Ref.24].
several types of measurement are not fully known, useful progress can be made. Even large data sets can now be correlated easily in large computers, and the degree of independence of various observations can be tested.

Example - Ice Classification by Radar Signatures

One of the problems facing ice reconnaissance is obtaining accurate, unambiguous classification of the many forms of floating ice. Traditionally classification has been made by direct observation from aircraft, but this approach is slow, expensive, impractical under conditions of darkness or cloud, and highly subjective. Therefore automated ice classification using all-weather sensors is very attractive.

To date no single radar sensor, either active or passive, has been shown capable of unambiguously distinguishing many ice types. However recent work combining a variety of sensors has shown that certain sets are complementary; i.e. their backscatter signatures give independent information about different ice types. Use of like- and cross-polarized scatterometers and a microwave radiometer has been used over a wide variety of ice types in the Canadian Arctic [25]. Although any two sensors did not result in an unambiguous classification scheme, data from three sensors was able to separate ice types in an attractive three-dimensional "feature space" (Figure 5). These results do not indicate where other ice types might lie in feature space, but are very useful in comparing sensors.

Of course there is no reason to be limited to a three-dimensional feature space. Classification can be extended to as many features as required, although the determination of which features will give independent information is guided largely by experience. Just as in the impulse radar example, work on the physics of the backscatter will doubtless result in a more effective combination of data.
Figure 5. A three-dimensional feature space to classify ice types in the Canadian Arctic. It is based on microwave emissivity at 19.4 GHz, like-polarized backscattering coefficient at 13.3 GHz and 45° incidence angle, and depolarization ratio, also at 45°. A number of ice and water classes are represented by ellipsoids whose volume show the spread in the data. The projections onto coordinate planes are also indicated. [Ref. 25].
CONCLUSIONS

Analysis of remotely sensed data will change rapidly during the next few years, as the opportunities to improve the effectiveness of measurements by using high speed digital computation are realized. Qualitative display and interpretation will give way to quantitative numerical characterization of the targets of interest.

These improvements will come not merely by collecting data digitally, but by introducing to the measurement and analysis process the physical relationships that relate the observations to the parameters of interest. Data can be improved by reducing non-environmental noise, and by making measurements with as many independent observations as necessary through use of wide bandwidth and multiple sensors. "Best" estimates of the required parameters can be determined for each problem based on the tolerable variance in the parameters sought and the accuracy of the observations.

These improvements in methodology will have several advantages for users of remotely sensed data:
- compression of data into the parameters of interest
- repeatable numerical results independent of operator
- near real-time output of results.

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REFERENCES


RIDGE STATISTICS FROM AERIAL STEREOPHOTOGRAPHY

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ABSTRACT

Statistics on the heights and frequency of ice ridges are of interest in structural design and logistics planning for arctic and subarctic areas. Ridge statistics can be gotten from profiles of the ice surface, much as wave statistics are gotten from profiles of the water surface. Ice-air surface profiles can be gotten by laser profilometer or aerial stereophotography. Height and frequency statistics developed from a profile are used to characterize ice conditions over an appreciable area around the profile. For risk-weighted structural design, the ridge heights of most interest are those in the small-risk/large-height tail of the ridge-height distribution. Usually, estimation of risk in the design range will require extrapolation of the ridge-height distribution function beyond the measurements taken. It seems desirable to guide and minimize the extent of such extrapolation by utilizing every available measurement on big ridges.

An aerial-photo flightline samples the ice surface along a path that is typically 9000 feet wide. Visual scan of photomosaics shows, not unexpectedly, that many of the largest ridges are not traversed by a linear profile across the photographed area; e.g., down the photo centerlines. This paper describes a grid-analysis procedure that includes in development of ridge-height statistics the largest ridges found in a photographed area. It provides an estimate of statistics from all the photographs taken. The grid analysis is in practice an extreme-value approach. The interval over which an extreme is taken is the area of a grid element. Ridge definition along a profile and ridge spacing along a profile are not used in the grid approach. Ridges are identified visually and measured stereoscopically in each grid element. Rubble fields, multiyear ice, floe sizes, ridge length, open-water fraction, etc., could be identified and measured at the same time. The area interval replaces spacing on a profile in estimating the frequency of loading events. This area interval can also be used to accumulate information on areal coverage by rubble, open water, multiyear ice, etc., by code numbers punched as the grid measurements are made. The grid approach seems potentially useful in its own right and may be helpful in estimating the length of photo-centerline or laser profile needed to obtain ridge statistics that adequately characterize the structure-loading potential in a photographed area. Statistics developed from Bering Sea flightlines via linear-profile and grid approaches are compared.
Introduction

Several investigations have approached the statistics of ice-ridge dimensions via application of a ridge-definition recipe to ice-air profiles measured along roughly linear paths using a laser profilometer or stereophotography (e.g., Refs. 6,9,10). An intuitive problem with such an approach is that no statistical use is made of large, rare ridges that appear in photographs to the side of the linear profile path. The risk-weighted load resistance that will be required of structures whose design is governed by annual or multiyear ice-ridge loads will be sharply affected by the low-risk/high-ridge tail of the probability distribution function for ridge height. To minimize unguided extrapolation of this probability function in the high tail, it seems desirable to make use of every measurement on big ridges that is available in the imagery that has been taken.

In the present paper an ideal case is defined for statistics applicable to structural design in an effort to clarify both the statistical status of information from photographs and the effect of approximations that must be made in realistic photo programs. Following discussion of the realistic case, a sampling procedure is outlined and brief comparison is made between ridge statistics gotten from centerline profiles along some 1977 Bering Sea flightlines and statistics developed from the same photography using a grid approach.

Three points summarize treatment of the realistic case.

1. Statistics of structure loading imposed by ice are dependent on both the profile of the ice surface along the path swept out by a structure and on the movement of the ice canopy. For waves, the motion that loads a structure, and so the hazard level, can be inferred from the surface profile; for ice, it cannot.

2. Absence of any information on movement path of the ice seen in aerial photographs makes every photographed ridge a candidate to impact a structure, placed in the area photographed, as the structure sweeps a path through the ice cover. A systematic and random sample of ridge heights that might be encountered is gotten by sampling the maximum ridge height in each rectangular element of a grid that spans a photographed area. The photographed area is itself a small random sample of the total ice cover.

3. Approximate counting of structure-loading events through the ice seasons of a year is needed to develop structure-load statistics. The loading event in grid analysis is a sweeping out, a sampling, by a structure of an area of ice cover equal to the area of a rectangular element in the analysis grid. The number of such events is determined for any ice-movement trajectory by structure width, the concentration of ridged ice, and cumulative ice movement in the ice seasons of a year.

STRUCTURE-DESIGN STATISTICS: IDEAL CASE

The term "ideal" is used here to imply the absence of practical restrictions on the data available to estimate quantities needed to risk-weight calculations of structure loading. The information ideally in hand for risk-weighted structure design is not ice conditions over some area on the sea surface but ice
conditions actually experienced by a structure at some specified site during the data-taking period. Quantitatively, "ice experience" consists of the ice measurements needed to calculate structure loading. These are given in the following table.

<table>
<thead>
<tr>
<th>Item</th>
<th>Found in Photos</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Ridge size</td>
<td>Yes</td>
</tr>
<tr>
<td>2. length</td>
<td>Yes</td>
</tr>
<tr>
<td>3. interval</td>
<td>Yes (for profiles)</td>
</tr>
<tr>
<td>4. age</td>
<td>Yes</td>
</tr>
<tr>
<td>5. strength, failure mode</td>
<td>No</td>
</tr>
<tr>
<td>6. Ice motion</td>
<td>No</td>
</tr>
<tr>
<td>7. Sheet thickness</td>
<td>No</td>
</tr>
</tbody>
</table>

Ridge interval is discussed below. One procedure for calculating risk-weighted structural loading (Ref. 3) employs equations of the following form.

\[
PYR(F^*) = 1.0 - \sum_{n=1}^{N} (1.0 - P(F > F^*)) \cdot p(N)
\]

where \(PYR(F^*)\) is the probability that loading will exceed \(F^*\) one or more times in a year. \(P(F > F^*)\) is the probability that loading by any single ridge will be greater than \(F^*\), and \(p(N)\) is a probability weighting for the number, \(N\), of ridges impacting the structure in a year.

Where to Photograph

The first decision is where photographs are to be taken. Ideally, map locations of offshore drilling and production sites would be known exactly for this decision. The next item ideally available would be long-term measurements on maximum ice excursion (Ref. 1) around each drilling location. Maximum ice excursion is the diameter of the smallest circle that contains the trajectory of ice movement around the specified drilling site during an ice season. Knowledge of maximum ice excursion would permit definition of an area around each drilling site where ice conditions are of interest for structural design. There would be no need for photography outside this area, because the ice outside will not reach the drillsite.

When to Photograph

The next decision is timing of the photo flights. The photos taken will provide the only representation of ice conditions in the drillsite area for the entire ice season. At one extreme, a single photo flight could be made, late in the ice season, to provide a conservative estimate of ice conditions through the season. In the ideal case, photos would be taken around each drillsite every day during the ice season. The ice-movement trajectory would also be recorded daily so that ice cover and ice motion could be matched to give a sample of ice experience for a structure at the drillsite.
Selection of Photographs for Analysis

Ideally, all photographs taken would be analyzed to give the largest possible sample of ice conditions around the drillsite.

Analysis of Individual Photographs

Each photograph taken on a flight samples ice conditions at a particular time in the ice season around a particular location in space. The entire photograph is a sample of the ice surface. The sample is not random in space or in time-during-the-season, because these were specified. The sample is random over the range of possible yearly variations in ice conditions at the specified time in the season and around the specified location.

Given the daily path of actual ice movement and photos of the ice surface that exists as the movement takes place, stereoprofiles can be gotten of the ice surface actually encountered by a structure in the photographed area. This statement assumes that ice motion is the same after structure installation as it was when the motion data is taken. A similar assumption is usually made for waves: motion inferred from the surface profile does not allow for the effect of the structure that the waves are loading. Structure presence in waves is allowed for via empirical drag and inertia coefficients. The structure trajectory in ice can be regarded as a path swept through the ice cover, as indicated in Figure 1. The partial ice cover shown in Figure 1 is treated as if it were a continuous ice cover; i.e., the partial ice cover is taken to move as a rigid body during the period between photographs. Any part of the path in open water is treated simply as more space/time between ridges. Some such conservative treatment seems necessary until a mechanical/kinematic model is developed to tell whether each impacting floe in a partial and/or fragmented ice cover will fail against the structure, pile up in front or be diverted around the structure.

The time profile gotten along the path of ice movement at the drillsite is similar in content to the time trace obtained from a wave staff in three ways:

1. The time series of ice elevations is gotten from the actual motion past the structure location.
2. The surface features in the profile are unlikely to have been measured at their maximum heights.
3. The actual-motion trajectory may not bring the largest ice features in the photographed area over the structure location.

The time profile of the ice surface at the structure location differs from a wave profile in that the motion that brings about structure loading cannot be inferred from the ice profile.

For interaction with a structure, ridge sizes can be characterized by "encounter heights" that are defined by the structure path through the ice cover. In the ideal case, the statistics of these encounter heights are known because movement of the photographed ice cover is measured.
ONE POSSIBLE STRUCTURE PATH

FIGURE 1

OPEN WATER OR SMOOTH ICE

FLOE

RIDGE

STRUCTURE PATH

FIGURE 2

ONE POSSIBLE STRUCTURE PATH
The other quantity needed for Eq.(1) is the count of ridges passing the structure location in a year. In the ideal case, daily data on ice motion together with photos of the ice surface give directly the number of ridges passing the structure location in each year of data-taking. Several years of such data would permit estimation of the probability distribution of N for use in Eq.(1). Risk calculations made to date indicate that risk-weighted results are appreciably less sensitive to N than to the form of \( P(F > F^*) \) in the design range of ridge heights (Ref. 3).

Before addressing a more realistic case for aerial photography, it is interesting to note that the items of information obtained from the "ideal" case defined here would be obtained from an upward-looking sonar unit at a fixed location on the sea floor (Ref. 7), except for identification of multiyear ice. Such a unit gives direct measurement of ridge-keel drafts and thus avoids the assumption inherent in laser and stereophoto work of statistical correspondence between the ice-air surface that is measured and the ice-water surface whose dimensions dominate structure loading. One question is the increase in power required with water depth. A second question with the bottom-fixed sonar is the narrowness of view. This is narrowness at two levels: scan width and areal coverage. A single sonar unit provides a laser-like profile, bringing in the need for ridge definition. Scan width at the site might be improved by a scanning unit or an array of units, which could give velocity information as well. Given these improvements, a question remaining is the area over which statistics determined at a pre-exploration measurement site will apply. This question could be addressed by deployment of a number of sonar arrays over a prospective area, with allowance for being unable to locate all the units at recovery time. At issue is adequate coverage of a prospective area before drilling locations are known.

STRUCTURAL DESIGN : REALISTIC CASE

The term "realistic" is used here to imply something like the situation faced in setting up an aerial photo program for, say, the Bering Sea. Roughly in order of importance, three factors distinguish the realistic from the ideal case.

1. There is no information on ice motion to associate with photos of the ice cover, and therefore ice experience at a structure location cannot be gotten directly. The absence of information on ice motion actually associated with any particular ice-cover photo makes reasonable the assumption that all ridges in the photo are candidates for impacting a structure placed in the same general area as the photo.

2. Funding limitations exist for motion measurement, photo flights and photo analysis.

3. The general prospective area is known but not the exact location of drillsites.

Where to Photograph

With exact drillsites unknown, some coverage of the entire prospective area is desirable in the initial years of photography to get overall coverage of ice
conditions that may have to be dealt with. Funding limits tend to make this coverage spatially sparse. Some guidance on flightline location can be gotten from ice observers and water-depth contours.

In summarizing statistics from a photography program, the prospective area may be subdivided into geographic zones. These zones are analogous to the areas of interest defined in the ideal case by ice motion around specific drillsites. They are analogous in that ice statistics are developed to apply over the entire zone and in that the zone boundaries are at the same map location every year, just as a production structure is.

When to Photograph

One survey may be made late in the ice season to pick up what are likely the worst ice conditions that will have to be dealt with. One or two additional surveys may be made earlier in the ice season to assess changes in ice conditions through the season.

Selection of Photographs for Analysis

Funding limits may motivate a second level of sampling to get from the total photo set a smaller set from which measurements may be gotten. Results desired from this smaller photo set are the same as desired from the total photo program: broad coverage of ice conditions across the prospective area. With this goal, the smaller set of photos will be selected to span the prospective area. Selecting stereophoto pairs at equal spacing along the flightlines is one possible procedure. This provides a sampling of the original photo set that is systematic (Ref. 4). Systematic sampling of photos in a specified geographic zone makes no reference to ice conditions and will provide an unbiased (i.e., random) sample of ridge heights from the total photographed, unless there is periodicity in ice-surface roughness comparable to the spacing between stereo pairs (Ref. 5).

Analysis of Individual Photographs

As in the ideal case, each photo samples an area of ice cover at a specified location in space and specified time in the ice season. A statistical characterization is sought of the structural ice experience that is depicted in the photo. Ice experience here is the height distribution (which gives the force distribution, \( P(F > F^*) \)), and number, \( N \), of ridges that pass a structure location. In the realistic case, the actual ice experience associated with a drillsite in the photographed area is unobtainable because the ice motion is unknown. Encountered ridge heights are not available, and it seems reasonable, reproducible and conservative to characterize ridge size by a height that is determined solely by the ridge itself, e.g., an effective maximum height.

Total uncertainty about the ice motion associated with the photographed ice surface implies that the structure could be located anywhere within the photo at any randomly chosen instant during the course of the actual motion. Possible structure locations within an ice-cover photo can be approximated discretely by a grid of rectangles, as in Figure 2. Each rectangle represents a possible structure location and has ideally an area roughly that of...
the structure at some mean depth where ridge keels impose load. Call this Area A'. One possible path through the ice is represented in Figure 2 by a sequence of rectangles that sweeps out a broad path through the ice, as a structure would. In order for the structure to occupy a rectangle, the structure must fail the largest ridge within the rectangle. With actual motion of the photographed ice cover unknown, it is reasonable to assume that each rectangle in the grid has equal likelihood of being occupied at least once as the ice in the photograph moves past the structure location; i.e., each rectangle has equal likelihood of lying on the actual structure path across the photographed ice cover. Repeated occupancy of a rectangle would not cause additional ridge failure because the ridges would not reform for a time after having been failed once. With the equal-likelihood assumption, the population formed from the maximum ridge height in each rectangle is suitable for random sampling in a simulation of ice motion.

In grid sampling as in profile sampling, heights less than some cutoff (e.g., 3 feet) are discarded. Smooth ice and open water fall below this cutoff. Approximate area percentages of smooth ice and open water would then be picked up by the grid sampling. Also, in grid sampling, a single ridge may provide the maximum height in two or more rectangles. Sampling such a ridge twice makes approximate allowance for the fact that, the longer a ridge is, the greater chance it has of hitting a structure. The maximum height in a grid rectangle need not be searched out to the last foot of accuracy because the maximum height in a rectangle may not persist in space for a distance comparable to the structure width. An approximate maximum may be nearer to an effective ridge height appropriate for load calculations.

The height appropriate for characterizing a ridge need not be the same where ridge-height statistics are sought for purposes other than structural design. For planning over-the-ice logistics, the motion of interest is that of a vehicle. This motion is subject to operator control, as ice motion is not, and the ideal path is in this case linear. Sail heights rather than keel depths, are the quantities of primary interest. In shallow water, the relevant ridge-sail statistics for logistics may differ from those relevant to structure design. For example, the sails of grounded and immobilized ridges and rubble piles must be considered in logistics. An approach to logistics planning has been made by Hibler and Ackley (Ref. 2). Their approach employs an area-measure \( R_p \) of ridging intensity and a distribution of ridge lengths. Determination of the length distribution requires imagery over an area, rather than a line. Determination of \( R_p \) also requires areal imagery, unless random orientation of ridges can be assumed.

The path width that a structure sweeps through an ice cover need not necessarily be the same as a grid-rectangle dimension. Therefore, the grid procedure involves an area-equivalence assumption: sampling the maximum ridge-sail height over a continuous area of size A' gives the same statistics for any shape of the sampling area. This assumption could be checked by extensive measurement work on existing aerial photos. The area-equivalence assumption performs similar service for grid analysis as the tacit assumption made in getting sail-height statistics from photo-centerline or laser profiles that sampling ridge-sail heights encountered along a continuous path gives the same statistics for any path trajectory. This assumption is distinct from the question, raised below, of adequate path length for characterization of ice conditions in a zone.
The grid-sampling procedure gives extreme-value statistics for ridge-sail heights. Extreme-value statistics depend on the underlying distribution of the random variable and on the specified sample size over which an extreme value will be gotten for inclusion in the population of extreme values. In hydrology the sample size is specified by a time interval, frequently one year. In getting ridge statistics from laser profiles, Tucker et al., (Ref. 6) specified a 20-kilometer distance interval. In grid-sampling of aerial photos, the sample size is an area interval, the area \( A' \) of a rectangular element in the grid. Specification of an area interval eliminates consideration of ridge spacing along a profile in getting a count of loading events.

A procedure for developing a ridge-sail-height histogram from grid measurements is discussed below. For the moment, assume that we have the probability distribution for the following event: "the maximum ridge-sail height in a random sample of \( A' \) square feet of ice surface exceeds a specified value, \( H^* \). With one-to-one mapping of ridge-sail height onto structure loading, we have also then \( P(F > F^*) \) for use in Eq.(1).

To estimate \( N \), the number of events defined above which a structure would experience in a year, it is assumed that a structure will sweep a path through a surrounding ice cover as indicated schematically in Figure 1. The area of ridged ice sampled by the structure is given approximately by \( W(D)(1.0-S) \). where \( W \) is the structure width at waterline, \( D \) is the mean yearly distance of ice movement past the structure, and \( (1.0-S) \) is the annual mean fractional area coverage by ridged ice. Therefore, the number of loading events in a year is

\[
N = \frac{W(D)(1.0-S)}{A'}
\]  

(2)

If possible, a weekly time series of \( D \) and \( S \) values would be used in Eq.(2), as was done in Ref. 3. for distance and total ice coverage. Cumulative ice movement is assumed the same before and after structure installation. Eq. (2) does not depend on movement trajectory.

**SUGGESTED GRID-SAMPLING PROCEDURE FOR RIDGE STATISTICS IN A ZONE**

1. Sample X% of all flightline photographs in the zone with evenly spaced stereophoto pairs. Using a grid, get the approximate maximum ridge-sail height in each rectangular grid element from these photo pairs. If the maximum height is less than some cutoff, say, 3 feet, count the rectangle as smooth ice or open water. Let \( S \) be the fraction of such grid rectangles.

2. Scan all photos in the zone to locate visually prominent ridges that are not in the selected X% of stereophoto pairs. Measure the height of all prominent ridges.

3. Form a histogram of sail heights in the zone.

The procedure is illustrated in Figure 3. A height is gotten in each grid element in the stereo pairs that are equally spaced along the flightlines in a zone. In addition, prominent ridges are noted visually outside the equally spaced stereo pairs. A grid is placed on the photo pair containing a large ridge and a maximum height is measured is each grid square where a ridge is large enough to be visually prominent.
A first need in implementing the above procedure is quantification of what constitutes a "visually prominent" ridge. Qualitatively, these are ridges that are easily seen in the photography by virtue of their size. Toward discussion of bounding factors on prominent ridge size, let $H_{vp}$ be the minimum sail height for visually prominent ridges. Two points argue for a fairly high $H_{vp}$, perhaps 15-20 feet. First, the smaller the value of $H_{vp}$, the less readily apparent and the more numerous will prominent ridges be in a visual scan. Results will be less certain and more time-consuming and costly to obtain. The cost of photo analysis is the only reason why all the photography would not be analyzed completely in the first place. Second, the histogramming procedure to be described assumes that all ridges exceeding $H_{vp}$ in zone photography have been detected by visual scanning. Therefore, $H_{vp}$ must be large enough for ridges exceeding it to stand out clearly in the photography, with very few being missed because of snow cover on the shadow side or ridge axis parallel to the incoming rays of sunlight. On the other hand, if $H_{vp}$ is too large, the sail height histogram will be based entirely on grid analysis of the equally spaced $X\%$ of stereo pairs, and $X$ may be only 5-10\% because of funding limitations. Considering these points, an $H_{vp}$ value of 15 feet was specified for data analysis discussed below. Sail-height probability distributions that have been examined are not critically sensitive to the $H_{vp}$ value. It is noted also that there can be several ridges with sails exceeding $H_{vp}$ in any one of the stereo pairs outside the equally spaced $X\%$, and different values of $H_{vp}$ can be used for different geographic zones.

A second need is specification of rectangle size ($A'$) in the analysis grid. As noted, this size would ideally be that of the structure area near the water plane. Increasing $A'$ lowers analysis costs, but too large an $A'$ raises the possibility that two or more prominent ridges will appear in a single grid rectangle. This possibility is not allowed for in the present formulation. Additional work with photo data will show where balance between analysis cost and refinement of the grid formulation should be struck.

A third need in implementing the above procedure is recipe for forming the population of sail heights that will be histogrammed. For height ranges exceeding the minimum height for a prominent ridge ($H_{vp}$),

$$N(i) = N_{xp}(i) + N_{vp}(i)$$

where $i$ indexes the height range-bin in the histogram, and

- $N_{xp}(i) =$ the number of ridges in the equally spaced $X\%$ of stereo pairs with sails exceeding $H_{vp}$ and lying in the $i$th height range
- $N_{vp}(i) =$ the number of ridges outside the $X\%$ of stereo pairs that have sail heights exceeding $H_{vp}$ and in the $i$th height range
- $N_p(i) =$ the total number of ridges in photographed area in the zone with height exceeding $H_{vp}$ and in the $i$th range

For ridge-sail heights less than or equal to $H_{vp}$, the statistics determined from complete grid analysis of the $X\%$ of stereo pairs are assumed to hold everywhere in the zone for grid rectangles that have a sail height greater than
the cutoff but less than Hvp. The number of these "non-prominent" ridges in
the ith height range is calculated as

\[ N(i) = \frac{(T(1.0 - S) - NP) \cdot Nx(i)}{(T(1.0 - S) - NXP)} \]  

(4)

where

\[ T = \text{the total number of grid rectangles in the zone} \]
\[ S = \text{fraction of grid rectangles containing smooth ice or open water} \]
\[ A' = \text{area of grid rectangular element} \]
\[ NP = \text{total number of prominent ridges in zone photography} \]
\[ NXP = \text{total number of prominent ridges in the } X\% \text{ of stereo pairs} \]
\[ Nx(i) = \text{number of ridges in the equally spaced stereo pairs having sail} \]
\[ \text{heights greater than cutoff, less than Hvp and within range } i. \]

Eq.(4) is formed from two ratios for ridges with sail height less than Hvp. One
is the ratio \( N(i)/Nx(i) \) of the number of ridges in the ith height range for the
entire zone to the number in the \( X\% \) of stereo pairs. The second ratio is the
area ratio: \( (\text{total ridged area})/(\text{ridged area in the equally spaced } X\% \text{ of stereo pairs}) \). Eq.(4) assumes these two ratios are equal and that the area fraction
\( S \) of smooth ice and open water is equal, throughout the geographic zone, to
its value in the \( X\% \) of stereo pairs.

The total number of grid rectangles in the population is \( T(1.0 - S) \), and
the probability of exceedance for the ith height range is calculated as

\[ P(H > H(i)) = \sum_{k=1}^{\infty} \frac{N(k)}{T(1.0 - S)} \]  

(5)

Eqs.(2)-(5) give \( P(H > H(1)) = 1.0 \), as is required for normalization.
Eqs.(2)-(5) give an unbiased estimate of exceedance probability that would be
obtained if all photos over the zone had been completely grid analyzed. The
service of the grid approach is to include in the statistics all large ridges
in the photography. The service of Eq.(4) is to reduce analysis cost via the
assumption that ridges smaller than Hvp are relatively plentiful so that their
statistics stabilize quickly. This assumption is analogous to the practice of
reducing computer costs in Monte Carlo estimates of probability distributions
by checking line-out of estimates with moderately high exceedance probabilities
and omitting further estimation of these probabilities as soon as the estimates
are stable.

COMPARISON OF RIDGE STATISTICS FROM GRID AND PROFILE ANALYSIS

Two items are of interest in considering analysis of flightline imagery,
whether stereophotography or laser.

1. The fraction of total flightline that must be analyzed to give
lined-out statistics for that flightline.
2. The flightline length over which lined-out statistics must be obtained in order to have a ridge-sail height distribution that adequately represents structure-loading potential in the area of a drilling location.

The first item can be addressed by analysing progressively greater fractions of a portion of the imagery and assuming that conclusions drawn apply to all other imagery. The first item does not arise if all the imagery is routinely analyzed, because statistics are then based on the entire measurement population. The second item tells the smallest length of flightline that must be flown to provide adequate ridge statistics for a geographic zone. If the line length flown within a geographic zone is less than this amount, statistics developed may not represent structure-loading potential in the zone. Because very large ridges are rare, statistics developed from too little flightline seem likely to be biased low, or at least be uncertain in the high-ridge tail of the distribution. It is always difficult to judge when enough data has been taken. This uncertainty motivates inclusion in development of statistics all extreme values present in imagery that is currently in hand.

The stereophotography data available for this paper was acquired in April, 1977, at four locations shown in Figure 4. Locations are designated by the number of the first photo in the segment of flightline where centerline ice profiles were obtained. The photo program has been described previously (Ref. 7). Additional information from the flightlines is given in Table I and discussed below. Grid analysis was performed on photos from which centerline profiles were taken. The grid used in this initial work had 18 square elements 1320 feet on a side, six squares long across the flightline and three squares wide, along the flightline. Contiguous stereo pairs in the profiled sections of flightline were specified to be the X% of photos that are completely grid-analyzed. Ridge sails exceeding 15 feet that were detected in zones defined around and outside the profiled sections were also added to the data base. This data base, together with use of Eqs.(3) and (4), gives an estimate of statistics for all sail heights in photographs of the zone, eliminating item (1), above.

Figure 5 shows three different ridge-sail height distributions on log-normal probability paper for flightline segment 130, southwest of St. Lawrence Island. In 8.5 miles of profile, 73 ridges were detected. Profile statistics are shown by open squares in Figure 5. The ridge definition used for profile analysis was of the Rayleigh type: a ridge was begun when the profile rose 3 feet above the surrounding ice and ended after the profile passed a maximum and fell to 20% of the maximum height. The distributions obtained are all truncated at 3 feet. Lines are drawn through the larger sail heights for extrapolation to heights appropriate for long recurrence intervals. The intent here is not to suggest the analytical form for any of the distributions but to compare the distributions in a reasonably consistent way. The maximum sail height along profile 130 was 9.6 feet.

The open circles in Figure 5 show the distribution from grid analysis of the same photographs across which the centerline was drawn. The largest sail height detected in this approach was 17.6 feet. The solid circles, beginning at 10 feet, show the distribution obtained from applying Eqs.(2)-(5) with $H_{vp} = 15$
for a zone extending approximately 60 miles around profiled section 130. For heights where only one circle is shown, the circles very nearly coincided. The relatively small difference between the open-circle and solid-circle distributions follows from the 17.6-foot sail and several other large sails being located within the profiled segment, though off the centerline.

The difference between profile and grid distributions is apparent in Figure 5 but difficult to interpret directly because the events to which the distributions refer are different. The profile distribution gives probability of exceedance for random sampling of a single peak height along the centerline of the 8.5-mile segment. The grid distributions give probability of exceedance for the maximum sail height in a randomly sampled area of ice surface along the flightline, the area size being $1320 \times 1320 = 1,742,400$ square feet or 40 acres. Toward some quantitative comparison of the two distributions that is relevant to the overall purpose of getting ridge statistics, Echert (Ref. 8) has noted that the relationship $P_{sing}(H > H^*) = 1.0/Lf$ may be used in the manner of Tucker et al., (Ref. 6) to estimate the expected length(L) of profile that would have to be flown and analyzed in order to observe one ridge sail of 17.6 feet or higher. For profile 130, the ridge frequency ($f$) is $73/8.5 = 8.59$ ridges/mile. $P_{sing}(H > H^*)$ is the single-event exceedance probability plotted in Figure 5. The profile-distribution line in Figure 5 crosses a height of 17.6 feet at a $P_{sing}$ value less than 0.0001. Therefore, the miles of profile one would expect to analyze to obtain a sail as large as 17.6 feet is more than $1.0/(0.0001\times8.59) = 1164$ miles. This length of profile would be flown within the defined geographic zone for which the statistics are being developed. For comparison, the total length of flightline in the Bering Sea in Figure 4 is 1800 miles. This discussion does not indicate that it is necessary to observe a 17.6-foot sail along a profile in order to have adequate statistics for the 60-mile zone around segment 130. However, it does indicate that to get a measurement as close to the design range as 17.6 feet is, substantially more flightline must be profile analyzed than grid analyzed.

Another comparison between the profile and grid distributions in Figure 5 can be gotten by estimating the 100-year sail height corresponding to the hand-drawn distribution lines. It is emphasized that this estimate is fictitious and can have no quantitative bearing on realistic design considerations because neither the form of the single-event distributions in Figure 5 nor the number of single events in a year has been in any way established. The probability that a sail height $H^*$ will be exceeded one or more times in a year can be written as

$$P(H > H^*) = 1.0 - (1.0 - P_{sing}(H > H^*)) = N$$

where $N$ is the number of events in a year. Eq.(6) makes the usual assumption that the same single-event statistics apply through the ice season of every year. For the 100-year event, $P(H > H^*) = 0.01$, if there is ice in the area every year. If an estimate is made of $N$, the value of $P_{sing}(H > H^*)$ corresponding to a 100-year recurrence interval can be gotten from Eq.(6). Inverting $P_{sing}$ then gives an estimate of the 100-year height. If the open-triangle, profile statistics in Figure 5 are used with the assumption that as many as 5000 ridges pass a structure location in the area during each year, the...
100-year height is about 18 feet. This exceeds by 87% the maximum profile measurement of 9.6 feet. A corresponding and similarly fictitious 100-year height can be gotten from the open-circle, grid statistics in Figure 5 by associating ridge count with movement distance via the mean ridge spacing of 311 feet found along the centerline profile of segment 130. This gives a value for D in Eq. (2). The sea surface is conservatively assumed to be 100% covered by ridged ice, and the structural-column width is assumed 60 feet. Eq. (2) gives then the number of 40-acre areas corresponding to any specified number of single ridges. For ice-movement distances greater than \((311\times5000)/5280 = 290\) miles in a year, the fictitious 100-year height is about 22 feet, which exceeds the maximum measurement of 17.6 feet by 25% or roughly one-third the profile data gap.

Figure 5 shows statistics for the northernmost segment in Figure 4. Another factor in assessing profile and grid statistics is illustrated in the southernmost imagery around segment 402, southwest of Nunivak Island. Cloud cover and other measurement difficulties limited centerline profile in this segment to 1.4 miles in 2 stereo pairs. Along this profile there were a total of 4 ridge sails higher than 3 feet. The maximum was 4.3 feet. Histogramming with the same 1-foot interval as for other lines gave the single open-square point plotted in Figure 6. The histogram interval could be reduced, but the narrow measurement range precludes estimate of the height distribution firmly enough for extrapolation. Grid analysis of the same two stereo pairs gave 24 heights with a maximum value of 10.3 feet and a distribution estimated by the open circles in Figure 6. Completing the grid analysis over flightline roughly 4 miles to the north and 16 miles to the south of the profile section produced 4 additional prominent sails with a maximum height of 17 feet, giving the solid circles in Figure 6. The same line represents both grid analyses reasonably well. It cannot be said that the grid distribution line in Figure 6 is the correct one for the area. Imagery at only one point in time and in one year is available. The point here is that the ability of area sampling to make use of all the imagery may sometimes be necessary to provide a large enough population for useful estimation of ridge statistics.

For segments 130, 210 and 402 there is not great difference between height statistics developed by grid for the profiled sections and for the arbitrary zones defined around the profiled sections. This lack of difference may be a consequence of the profiled segments having been selected in areas of relatively heavy ridging. Such agreement seems unlikely where the segments to be profiled are equally spaced to give an unbiased sample of the entire photo set. An example of the type difference that can occur is shown in Figure 7 for segment 191 south of St Lawrence Island. Visual scanning of 21 stereo pairs to the east of this segment picked up 8 ridge sails exceeding 15 feet. The smallest of these additional sails was 17.5 feet. This gave a data gap from 11.7 to 17.5 feet. Figure 7 is given to illustrate that extending a grid analysis by visual scanning can make a difference in the statistics obtained. As indicated by the three open squares, centerline profiling did not provide a firm estimate of the height distribution. There were 43 ridges in the profile but 32 of these were between 3 and 4 feet in height and the range was only 3.0 to 7.5 feet.

Table I summarizes information on the flightline segments and the illustrative statistics developed for them. For each segment are given results from segment profile analysis, segment grid analysis and the zone analysis. The total
<table>
<thead>
<tr>
<th>Segment</th>
<th>Number</th>
<th>Latitude</th>
<th>Longitude</th>
<th>Length (Mi)</th>
<th>Water Depth (Ft)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>130</td>
<td>63°10'</td>
<td>172°10'</td>
<td>8.5</td>
<td>180</td>
</tr>
<tr>
<td></td>
<td>191</td>
<td>62°30'</td>
<td>172°40'</td>
<td>7.8</td>
<td>180</td>
</tr>
<tr>
<td></td>
<td>210</td>
<td>61°30'</td>
<td>168°</td>
<td>25.2</td>
<td>90</td>
</tr>
<tr>
<td></td>
<td>402</td>
<td>59°30'</td>
<td>168°</td>
<td>1.4</td>
<td>120</td>
</tr>
</tbody>
</table>

**TABLE I**  
**SUMMARY OF PROFILE AND GRID ANALYSES**

<table>
<thead>
<tr>
<th>Segment</th>
<th>No. of Stereo Pairs:</th>
</tr>
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<tbody>
<tr>
<td></td>
<td>To N or E</td>
</tr>
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<td></td>
<td>86</td>
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</table>

<table>
<thead>
<tr>
<th>Segment</th>
<th>No. of Sails</th>
<th>Max. Height (ft)</th>
<th>Hand-Drawn Log. Norm</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>73</td>
<td>9.6</td>
<td>4.4</td>
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</table>

<table>
<thead>
<tr>
<th>Segment</th>
<th>50% - Tile</th>
<th>84% - Tile</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>10.6</td>
<td>10.6</td>
</tr>
<tr>
<td></td>
<td>2.3</td>
<td>4.0</td>
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<tr>
<td></td>
<td>5.7</td>
<td>7.6</td>
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<tr>
<td></td>
<td>6.9</td>
<td>10.4</td>
</tr>
<tr>
<td></td>
<td>4.4</td>
<td>7.3</td>
</tr>
<tr>
<td></td>
<td>11.3</td>
<td>13.9</td>
</tr>
<tr>
<td></td>
<td></td>
<td>13.6</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Segment</th>
<th>Fictitious 100-Yr. Sail Ht (ft)</th>
<th>Smooth or Open-Water Frac. (S)</th>
<th>X%</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>18</td>
<td>0.07</td>
<td>100</td>
</tr>
<tr>
<td></td>
<td>22</td>
<td>0.07</td>
<td>12</td>
</tr>
<tr>
<td></td>
<td>18</td>
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<td>0.33</td>
<td>100</td>
</tr>
<tr>
<td></td>
<td>21</td>
<td>0.33</td>
<td>4.3</td>
</tr>
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</table>
The number of stereo pairs in the zone is the sum of the three numbers shown under "Zone Grid." The entry "50%-tile" is the 50th percentile value from the hand-drawn log-normal lines used in developing the fictitious 100-year values. The smooth-ice and open-water fraction(S) does not enter profile analysis. Neither does the percent of zone flightline (X%) that is completely grid analyzed. X% is 100 for the segment grid analysis because every stereo pair that was profiled was given complete grid analysis. The 100-year heights in Table I are designated "fictitious" because they are based on a single year's data at a single point in time and because the most appropriate analytic form for fitting and extrapolation of the sail-height distribution has not been determined. Further, the flightline segments discussed were not randomly sampled from the entire available photo set.

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AN ICE HAZARD DETECTION SYSTEM -
PRELIMINARY INVESTIGATIONS

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ABSTRACT:

With the increasing hydrocarbon development in ice infested waters there has come
a need to upgrade and enhance existing methods of ice detection. Recognizing this
requirement, a co-operative venture between the Transportation Development Centre
and Petro-Canada Exploration Inc. was established to evaluate the effectiveness of
available remote sensing devices to detect icebergs, ice floes, bergy bits and
growlers.

A research vessel, the M/V Polarhav, was equipped with X-band (3 cm) and S-band
(10 cm) marine radars, an infrared imaging scanner and a variable depth towed
sonar system. The vessel operated in the Lancaster Sound, Jones Sound and
northern Baffin Bay regions during the open water season of 1980 and performed
detailed studies of a selected inventory of ice hazards representative of the
area. The sensor observations were supported by an extensive groundtruthing
program (including sidescan sonar draught measurements, stereo aerial photography
and target visits) and a complimentary program of oceanographic and meteorological
observations.

A description of the project scope, hardware, and field deployment is presented.

* now with
Bombardier Inc.
Boucherville
Quebec
1.0 INTRODUCTION

A co-operative venture was undertaken between Petro-Canada Exploration Inc. (PEX) and the Transportation Development Center (TDC) to evaluate the effectiveness of various ship mounted remote sensing devices for the detection of ice hazards in open water.

The objectives of the program were:

1. to evaluate the available remote sensor packages on the market (radar, sonar, infrared and low light level television),
2. to test the operational capability of available remote sensor packages as part of a ship mounted system, and
3. to obtain a basic data set for each remote sensor to evaluate the sensitivity of each sensor to variations in target size and shape, range, and environmental conditions.

This program was undertaken by PEX in support of exploratory drilling operations and offshore development studies.

Due to the scope and costs associated with the program, and the recognizable benefits to the marine transportation industry, PEX submitted an unsolicited proposal to the Department of Supply and Services (DSS) in July 1979 for the "Evaluation and Development of a Ship Mounted Ice Hazard Detection System". The Department of Transport supported this development, and as a result a contract was executed between DSS and PEX in April, 1980. PEX became the prime contractor and Project Manager for the development program, and TDC became the Scientific Authority for the federal government.

The program evolved through the following phases.

Phase I was a technical study to evaluate the theoretical capabilities of radar, sonar, infrared (IR), and low light level television (LLLTV) systems in detecting ice hazards. A market search was also conducted to evaluate existing sensor packages. The technical study was initiated during the first quarter of 1979 and was completed in the first quarter of 1980.

Phase II consisted of sensor system selection and field testing. This phase was initiated in the fourth quarter of 1979. The sensors were mobilized and field tested during the summer of 1980. A final report detailing test results and interpretations, preliminary specifications for prototype ice sensors, and recommendations for operating sensor combinations is forthcoming.

Phase III involves the development of an all weather day/night ship mounted and computer controlled integrated ice hazard detection collision avoidance system by 1986.

This paper details the activities associated with selecting and field testing the sensors during Phase II of the program development.
2.0 PHASE II

2.1 Program Management

Project management for the program was handled by PEX.

The Scientific Authority for the federal government, and representing the Transportation Development Centre (TDC) of Transport Canada, is Mr. Maurice Audette who replaced Mr. Claude Durand.

A Steering Committee was also formed from available technical expertise within government to advise TDC with respect to overall policy, direction, and monitoring of the project. The steering committee consisted of representatives from:

- Department of Supply and Service (DSS)
- Department of Transport - Strategic Services (TDSS)
- Canadian Coast Guard - Technical Services (CGTS)
- Canadian Coast Guard - Operations (CGGO)
- Defense Research Establishment Valcartier (DREV)
- Atmospheric Environment Services - Ice Branch (AES)
- Canada Centre for Remote Sensing (CCRS)

The technical consultant for the program (Phase I and II) was Remotec Applications Inc. They were responsible for technical specifications, hardware acquisition, modification and integration, data collection; and preliminary analysis.

Industry participants in the Phase II program were:

- CN Marine Inc.
- Mobil Oil Canada, Ltd.
- Aquitaine Company of Canada Ltd.

2.2 Area of Operation

The study area for Phase II was northern Baffin Bay. The M.V. Polarhav was operational in the study area from August 2 to October 6, 1981. The main areas of operations, as shown in Figure 1, were eastern Jones Sound, eastern Lancaster Sound, and the Philipots Island area. The M.V. Polarhav departed Halifax on July 24, and arrived back at Halifax on October 16, 1981.

The study area was chosen because it was known to have a large variety of ice type targets. Target types for sensor system testing were icebergs, bergybits/growlers, ice floes (first-year and multi-year), ice pack and ice edge. It was also an area that PEX had a large baseline data set due to its EAMES program in 1978 and 1979. In addition PEX had logistic support lines established in the area, and a good relationship with the neighbouring community of Pond Inlet.

2.3 Hardware

Based on the theoretical evaluation and market survey conducted in Phase I, sensor systems were recommended for field testing in Phase II. The criteria used in making recommendations in Phase I were: technical requirements for the sensor system, reliability, ease of operation, and system costs. The Phase II field tests were restricted by program costs and sensor system availability to a radar system, a sonar system, and an infrared system.
In evaluating the radar system four companies were identified which could supply comparable standard radar equipment. The final system, as shown in Figure 2, was selected to provide enough flexibility to evaluate the performance of shipborne radar systems in detecting ice hazards. The system configuration shown in Figure 2 was designed for simultaneous operation at two frequencies, X-band (9.4 GHz) and S-band (3.0 GHz). The X-band system consisted of two antennas with one operating at selectable HH or CP, and the other providing HH polarization only. Both X-band antennae were equipped with variable speed control from zero to thirty rpm. The S-Band utilized an HH polarization antenna operating at thirty rpm. Three pulse lengths (short, medium, and long) could be selected. Three 16 inch diameter PPI scopes were available with each being able to display radar returns at either frequency. Radar system specifications are shown in Table 1. Ancillary radar equipment included: a power meter for measuring average transmitted power, an A-scope and camera for displaying and recording radar return pulses, power attenuators, and 35 mm cameras to acquire PPI photographs. Both X- and S-band data were recorded on Sony video tape recorders through EMI interfaces. The B&W radar display was used to play back recorded data.

The criteria for selection of the radar video tape recorders for use in the field program were:

- ability to record raw radar data,
- compatibility to the marine radar for recording purposes, and
- compatibility to the display unit to allow playback of the recorded data in the field.

Due to the requirement to have as much of the field data as possible available for detailed analysis at the end of the field season it was decided to record the data in an analog format in the field with subsequent digitization in the laboratory. The radar recording configuration is shown in Figure 3. Analog recording allows the appropriate raw data to be recorded corresponding to various radar parameter combinations, types of targets and environmental conditions. The resultant analog data is then available for selective digitization in the laboratory to allow detailed quantitative analysis of the data. The raw radar data was recorded for only the medium and long pulses because of the commercial recorder bandwidth for recording radar data. Radar data recording requirements are: (20 MHz for .05 sec pulse; 4 MHz for .25 u sec pulse; and 1 MHz for 1.0 u sec pulse). The decision to utilize analog recording in the field was also based on reliability of equipment, ease of operation, and deliverability. The EMI recorders allow the data to be played back into a PPI display to check data quality in the field.

As seen in Figure 3 the analog-to-digital converter used in the laboratory has an appropriate bandwidth and sampling rate to provide digital data for storage on a high density tape recorder. The recorder can then play the data back at a rate compatible for storage on a computer magnetic tape and/or disk. This system provides the ability to digitize and analyze the radar data from the field program.

Based on the Phase I technical evaluation and market search, a optimal design sonar system does not exist for the detection of ice hazards in open water. To allow field evaluation of a sonar system, PEX mobilized the Westinghouse HS1001 Variable Depth Sonar (VDS) system. The HS1001 sonar is a lightweight, single cabinet, medium-powered sonar system designed to operate in conjunction with a variable-depth towed transducer system. Two transmission modes are provided; pulse CW and pulsed pseudo-random noise. The system operates at a 10 KHz frequency and a 216 dB source level with a 40 milli second pulse length. The
sonar system configuration is shown in Figure 4. The standard PPI display was used in the field to provide visual observations and a photographic record. Sonar data could be recorded on tape for subsequent playback and analysis. The B-scope provided photographic records of individual target pulses. The ancillary equipment included a Sippican recorder (salinity and temperature versus depth probe), and a small computing system with plotter capability for ray path calculations.

The Phase I technical evaluation showed that shipborne uses of IR sensors had been limited to military applications, and that there was a lack of targeted infrared data of icebergs and sea ice. As a result an IR target signature program was committed to using the AGA 780 thermovision system. This system was chosen to provide basic data on ice target emission and detectability as a function of environmental and oceanographic conditions and target characteristics such as type, size, shape and temperature. The AGA Thermovision 780 Longwave (LW) system was mounted on a remotely controlled pan/tilt system and enclosed in an environmental housing. The detector operated in the 8-14 um wavelength region. The system was equipped with interchangeable lenses having fields of view of 3.5° x 3.5° and 7° x 7° with eight different aperture positions ranging from f/1.8 to f/20. The system also had a filter wheel equipped with a 7.2 um cut-on filter, and a 10.5 um narrow pass band filter. The display consisted of a black and white monitor with a thermal profiling package. The system was equipped with a black and white photo recording package and an analog recording (video tape recorder) package for storage of data in the field. The system included an image processing package for quantitative analysis. A digital package for laboratory use included hardware (OSCAR) and software (SOFTA) packages for data recording and playback and quantitative analysis.

Two Barnes precision radiation thermometers, a PRT-5 and a PRT-6, were used in support of the IR program. Background measurements were taken of the ice target, the sky at zenith and above the horizon, and the water near the target and near the ship.

2.4 Field Deployment

Field operations were carried out on board the M.V. Polarhav (M.V. Chester). The ship has a gross tonnage of 590 tons, a length of 51.7 meters, a breadth of 9.6 meters and a horsepower of 1320. She has an ice class of type C, per the Arctic Special Pollution Prevention Regulations. The variable depth sonar system was located on the stern, below the helicopter deck. The three radar antennae were mounted vertically above each other on the forward mast, with a radar transmitter-receiver unit shed at the base of the mast on the shelter deck. The IR sensor was mounted on the port king post, above the bridge house. The laboratory containing the displays and recording equipment were located in No. 3 Hold. Two generator units, supplying 100% back-up for the ice hazard detection system, and independent of the ship's electrical system, were located on the main deck below the shelter deck.

Throughout the course of the field program data were collected in different operational modes. The mode selected at any given time was dependent upon ice and environmental conditions, as well as specific target priorities.

Search mode (Mode 2) was used as the standard data collection mode while the ship searched for selected targets. All targets within sensor range were considered equally. If suitable targets were present, the decision would normally be made to go to an intensive mode. A decision flowchart for intensive modes is shown in Figure 5. An intensive moving mode (Mode 4M) was entered when passing through
heavy ice concentrations or moving past several targets of interest. When individual targets were selected for study, a selected target mode (Mode 4S) was entered. This mode was the most important in terms of future data analysis. The intensive modes were multi-sensor modes where each sensor was operated to obtain the optimum data for that sensor. At the end of Mode 4S a stationary or side scan mode (Mode 3) could be entered to obtain close-up data for selected targets.

To supply verification and calibration data for the various sensors several complementary truthing programs were carried out. Visual and photographic observations were performed during the field program to provide a record of ice targets and conditions around the ship. In addition, environmental conditions were observed and recorded in the master log every thirty minutes. The observations included: wind speed and direction, ice cover, sea state, precipitation, barometric pressure, cloud cover, cloud type, and visibility. AES selected ships weather data was also recorded and reported at three hour intervals.

When the ship was operating in the selected target intensive mode the visual observer photographed the target from all four sides, and a sketch of each side was entered into the daily journal, along with a size estimation.

A Bell 206B helicopter, equipped with a 70 mm camera and mount, was used to acquire aerial photography. Whenever possible, vertical photography was obtained for selected targets to allow subsequent stereoscopic measurement of size and shape. On occasion flight lines were flown during intensive moving modes to provide photographic correlation for other sensors. A side-scan sonar was also deployed on selected bergs to obtain draft measurements. PRT-5 (9.5 to 11.5 um wavelength) and PRT-6 (2.0 to 20.0 um wavelength) measurements for temperature and radiations were taken during selected target operations. A Sippican recorder was used to obtain temperature (XBT's) and velocity (XSV's) profiles of the ocean. Calibrated radar reference targets were deployed during intensive measurements of ice targets to allow determination of the radar cross-section of the ice targets. Wave rider buoys were deployed, with limited success due to the nature of the ship's operations and the limited communication range of the VHF transmitters. An oceanographic program was run off northeastern Devon Island, between Philpots Island and Jones Sound. Current meter moorings were deployed and CTD lines were run. Three remote weather stations were also deployed on Philpots Island, SE Devon Island, and Bylot Island. In August, an AES Ice Branch aircraft (NDZ) overflew the M.V. Polarhav while operating its SLAR (side-looking airborne radar). This was a co-operative program between AES Ice Branch and PEX to supply truthing to each others ice detection system.

3.0 PROGRAM EVALUATION STATUS

Data tabulation and correlation with the various environmental support programs has been completed. This has included data quality control and preliminary analysis of individual system and target parameters. Quality control and analysis of target dimensions has resulted in reclassification of approximately 50% of the targets, the majority of which were underestimated in size in the field. The meteorological data has also been summarized to allow correlation to sensor performance.

Analysis of sensor performance for the final report is concentrating on the selected target data from Mode 4S. A data summary for selected targets from the field program is shown in Table 2.
The radar system analysis, based on A-scope photos and digital data analysis, will evaluate signal to noise, signal to clutter, probability of detection, and return power as a function of target type, range, sensor parameters, and environmental conditions. Preliminary radar system specifications, related to an ice hazard detection system, will also be prepared for the final report.

The infrared system analysis, based on display scope photos and digital data analysis, will evaluate image contrast, target detectability, and signal received as a function of target type, range, sensor parameters and environmental conditions. Preliminary IR system specifications will also be prepared for the final report.

The sonar system analysis was limited by the HS1001 VDS system failure early in the field program due to water penetration into the towed cable and/or transducer body. It is felt that a VDS system could be designed specifically to operate against low velocity targets located at the upper boundary of the ocean. This will require further detailed study of the system parameters to optimize detection of ice hazards under the prevailing conditions.

The preliminary results to-date show the usefulness of both radar and infrared systems in detecting a variety of ice hazards under different environmental conditions. Statistical analysis does indicate that an integrated sensor system should improve both the detection capability and reliability of an ice hazard detection system.

4.0 FUTURE DEVELOPMENT

PEX and TDC have committed to Phase III of the development program which involves the development of selected sensors designed for detecting ice, and the integration of these sensors with a processing system for real time analysis for reliable detection and tracking of ice hazards.

PEX is in the process of developing an overall systems requirement specification, and along with TDC, selecting a prime contractor for system development and integration. By early 1983 a prime contractor should be selected and committed to the project development. This should result in prototype tests of a "ship mounted and computer controlled ice hazard detection/collision avoidance system" by 1985/86.

5.0 SUMMARY

A need exists in the marine shipping industry and in the offshore oil industry for the development of an Ice Hazard Detection System. Industry and government co-operation have made the program a success in Phase I and II. It is still necessary to test the sensor systems in winter ice cover conditions to meet all the needs of the Arctic shipping industry.

Phase III, the development and testing of a prototype, integrated ice hazard detection system has been committed to by PEX and TDC. The continued support of the industry, to ensure all user needs are met, will ensure the success of this program.

ACKNOWLEDGEMENTS

Our appreciation for the success of the program is extended to the steering Committee, Captain K. Maro and the crew of the M.V. Polarhav, John Miller and
Karen Parker, PEX field representatives, and Marg MacLeod, PEX material co-ordinator.

REFERENCES


M.V. Polarhav - Study Area Summer 1980

Figure 1
Final System

Radar Recording

In Field

In Lab

Sonar System

* Modifications to be carried out by Decca

** Off-the-shelf ancillary equipment
AN ICE HAZARD DETECTION SYSTEM
PRELIMINARY INVESTIGATION
BY W.B. JONASSON & C. DURAND

DISCUSSION
by:
J.B. Mercer, Dome Petroleum Ltd., Calgary

1. How was the marine radar calibrated and were you able to accurately determine the radar cross sections of the ice targets?

2. Over what range and in what seastates were targets being observed?

AUTHOR'S REPLY
by:
W.B. Jonasson, Petro-Canada Exploration Inc., Calgary

1. To confirm the validity of the Radar's backscatter coefficients for ice targets, calibrated radar reflectors were deployed near the targets. To account for signal fluctuations from scan to scan, single pulse measurements from four consecutive scans were averaged for each target. Average radar cross-section values were then plotted as a function of range. Standard error calculations for selected targets indicated that fluctuations in the return power from a target could be as large as 15dB.

Response to Question 2
Posed by Brian Mercer

2. One objective of the field program was to evaluate the environmental parameters affecting ice hazard detection. Consequently data were collected over a wide range of environmental conditions. Targets were observed at ranges of 0-12nm and in sea states ranging from 0-5.
DISCUSSION
by:

J. Rossiter, Huntex Ltd., Scarborough

1. Are there any sensors (of those tested) which you would not consider useful for shipborne ice hazard detection?

2. What other sensors should be considered for future ice hazard detection programs?

AUTHOR'S REPLY
by:

W.B. Jonasson, Petro-Canada Exploration Inc., Calgary

1. A thorough evaluation on the utility of each sensor used in this program cannot be made until sufficient data have been analyzed. To date only 5% of the data collected have been reviewed. This limited data set precludes any statement on the utility of each sensor for ice detection until a larger statistical ensemble is examined.

2. A low light level television might be considered for future programs.
DESIGN FACTORS FOR RUBBLE MOUND STRUCTURES UNDER ICE AND WAVE ATTACK

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ABSTRACT

This paper presents an overview of the factors affecting the design of rubble mound and slope protection structures in ice-infested waters. Discussions center on long service life structures in exposed waters where wave and ice conditions impacting the structure are severe. Specific concerns of ice-structure interaction are discussed with reference to some cases found in nature and on manmade structures. A design-development approach is outlined which is being used to determine appropriate combinations of slope geometry and slope protection for various imposed design conditions.

INTRODUCTION

As arctic and subarctic areas are developed for various natural resources such as minerals, oil, gas, fisheries, and others, various marine structures are necessary for port and harbor facilities and for offshore work platforms. Rubble mound structures have been found to be functional and economical throughout the world and their design has developed from this extensive experience coupled with laboratory and analytical investigations. In addition to harbor structures, slope protection is also required for the artificial islands used for petroleum exploration and production. In this regard, rubble mound design technology is being extended by ongoing investigations to include a broader range of slope protection devices including various fabric containers, mat systems, and other devices.
In addition to the broader range of devices being investigated, ice conditions must be considered in the design process. Although some rubble mound structures have been built before in ice-infested waters, these have been exposed to relatively stable ice. Moving ice presents a whole new dimension to the shore protection problem in view of the alternating ice and wave action experienced by the structure over the years of service. The subject of arctic slope protection has been addressed rather extensively over the last decade through Beaufort Sea artificial island experience, but these cases have all been for structures with intended short service lives in nearshore waters where ice and wave conditions are less severe. AOGA Project #126 was commissioned to develop slope protection designs for long service life structures and for structures located in water depths where the full force of Beaufort Sea wave and ice conditions will be encountered.

The purpose of this paper is to provide an overview of the design factors for rubble mound structures in arctic areas. Of these factors, ice-structure interaction considerations will be discussed in greater detail, including some insights drawn from experience on natural and manmade structures. Also, the design-developement approach used in AOGA Project #126 will be outlined. Only generalized results can be presented due to private funding of these investigations.

PERSPECTIVE - FOCUS

Ice and waves are not the only design considerations for arctic marine structures. The short summer open water season, the difficulty of mobilization and transportation, and (for some sites) the lack of nearby construction materials all add up to making the design problem very complicated indeed. Table 1 presents planning considerations for arctic rubble mound structures that provide perspective on the full dimension of the problem. Table 2 presents design factors requiring technical and economic evaluations which focus on the engineering of arctic rubble mound structures.

ICE-STRUCTURE INTERACTION

This section examines some of the ice-structure interaction concerns affecting rubble mound structures. Since so few of these have been built in arctic areas, particularly in areas of moving ice sheets, some naturally-occurring features (Photos 1 and 2) are discussed for their relevance to rubble mound structure design. This paper deals with two kinds of ice-structure interactions due to moving ice sheets:

(1) ice-rideup/pileup/overtopping
(2) displacement of armor units
### TABLE 1

**PERSPECTIVE:**

**PLANNING CONSIDERATION FOR RUBBLE MOUND STRUCTURES IN COLD REGIONS**

#### A. Site Conditions

1. **Wave Conditions**
   - Degree of exposure
   - Normal and storm wave characteristics
   - Water depth-breaking wave conditions
   - Variations during tide
   - Variations during storm water level setup or setdown conditions

2. **Ice Conditions**
   - Sea ice conditions and movement
   - Floating ice fragment conditions and movement
   - Grounded ice conditions - ridges
   - Ice characteristics and pressures

3. **Water Depth Considerations**
   - Effect on fill requirements
   - Distance offshore - construction techniques

4. **Water Level Conditions**
   - Tide range
   - Storm setup and setdown

5. **Current Conditions**
   - Hydrodynamic forces
   - Construction considerations
   - Scour considerations
   - Effect on ice conditions

#### B. Construction Factors

1. **Viable Construction Methods**
   - Expertise required
   - Time required
   - Equipment required
   - Duration of construction season
   - Extreme weather downtime during construction season
   - Mobilization factors
   - Cold weather construction implications on manpower, equipment, and materials

2. **Construction Materials Available**
   - Quantity
   - Quality
   - Distance and difficulty of transport
   - Types (rock, sand, gravel, concrete, etc.)
   - Access routes

#### C. Cost Factors

1. **First Cost**
   - Manpower - skill levels and amount
   - Equipment-mobilization; operations; demobilization
   - Materials
   - Cost vs economic benefit of structure

2. **Maintenance Considerations**
   - Risk vs convenience (allowable damage vs overdesign)
   - Intended service life of structure
   - Future cost of manpower, equipment and materials
   - Equipment availability once demobilized

#### D. Regulatory Considerations

- Applicable Federal, State and local ordinances, stipulations, CSM guidelines, etc.
- Allowable construction techniques
- Allowable operational seasons
- Removal and restoration requirements
- Environmental impacts - physical, biological, sociological
TABLE 2

FOCUS: DESIGN FACTORS FOR RUBBLE MOUND STRUCTURES IN COLD REGIONS

A. Wave-Structure Interaction

1. Cross-Section Design
   - Geometry-wave runup and overtopping
   - Slope-wave runup and fill quantity factors
   - Dimensions for functional usage of structure
   - Elevation-overtopping considerations
   - Backslope drainage features
   - Ease of construction and maintenance

2. Stability Design
   - Armor type, placement, size, and density
   - Armor porosity
   - Armor slope
   - Crest stability design-overtopping aprons
   - Toe stability design
   - Underlayer considerations
   - Tradeoffs with ease and cost of construction and maintenance
   - Damage sensitivity-failure modes
   - Service life and maintenance philosophy

B. Ice-Structure Interaction

1. Cross-Section Design
   - Geometry as relates to ice rideup and pileup events
   - Structure slope and volume requirements to resist ice loading
   - Crest configuration for ice overtopping design
   - Ice pileup storage considerations
   - Ease of construction and maintenance
   - Effect of ice foot and adfreeze on ice pileup and rideup

2. Stability Design
   - Local slope failure-freeze front and geotechnical factors
   - Armor displacements by ice sheet
   - Armor displacements by ice fragment impact
   - Armor displacements by plucking
   - Armor breakage
   - Effect of adfreeze and ice foot
   - Armor or toe gouging by ice ridge grounding
   - Crest stability during ice pileup-rideup events
   - Tradeoffs with ease and cost of construction and maintenance
   - Underlayer considerations
   - Damage sensitivity-failure modes
   - Service life and maintenance philosophy

C. Construction and Operational Scenario

1. Construction Stage
   - Floating construction
   - Land-based construction
   - Over-ice construction
   - Length of construction season and season extension techniques
   - Storms during construction
   - Armor and underlayer fabrication
   - Fill-first construction techniques
   - Fill retaining construction techniques
   - Stages construction

2. Operational Stage
   - Functional downtime vs various wave, water and ice events
   - Periodic maintenance and inspection
   - Snow and ice clearing
   - Operational season vs year-round operation
PHOTOGRAPH 1
NATURALLY – OCCURRING BOULDER PAVEMENT ON THE MACKENZIE RIVER
(COURTESY OF D.K. MAC KAY, ENVIRONMENT CANADA)

PHOTOGRAPH 2
ICE PILEUP OBSERVED ON A ROCKY BEACH
(COURTESY OF AUSTIN KOVACS, U.S. ARMY COLD REGIONS RESEARCH AND
ENGINEERING LABORATORY)
Ice Pileup/Rideup/Overtopping: Ice pileup commonly occurs on arctic beaches as advancing ice sheets fail near sea level in a manner that causes piles of ice rubble to form. The occurrence of a pileup essentially prevents ice from advancing farther onto the beach. In an ice rideup event, combinations of ice characteristics and shore characteristics are such that the ice, though failed into blocks, gets pushed inland overriding the underlying surface. Events of ice rideup are potentially destructive to backshore features or nearby manmade dwellings or structures. Ice overtopping is a term that can be used to describe ice which topples over a retaining device after pileup in much the same way that waves overtop a structure.

Kovacs and Sodhi (1979, 1980) have examined the historic occurrences of ice pileup and rideup, and the physical factors governing their occurrence. They point out that Taylor (1977) observed an ice rideup scar that extended 185 meters inland from the shoreline along Somerset Island on the Canadian Beaufort coast. Observations show that the period of pileup or rideup may be of short duration, less than 30 minutes, and can occur any time of the year but more frequently in the Fall and Spring, when the ice is the most mobile during freeze up and break up. Tsang (1975) reports on a witnessed ice pileup event on a Canadian lake during which ice piled to a height of about 2 meters above lake level in thirteen minutes. The ice pileup-rideup events summarized by Kovacs and Sodhi (1979, 1980) contain several references to very large ice encroachments on beaches, on structures, and up and over bluffs. Because of the wording used in the original (sometimes very old) references, it is not always clear if the event described was rideup or pileup, or possibly even some ice overtopping. Photo 2 (provided by Austin Kovacs) shows an ice pileup on a rather steep rocky beach which may be considered somewhat similar to a rubblemound structure.

It should be noted that ice rideup or pileup will occur only when the island is directly subject to large scale movements of level ice. Ridges which ground on the island slope will effectively block the island from further attack by level ice and therefore to further opportunity to experience ice rideup or pileup. Previous pileups act in much the same manner in that subsequent pileups will likely occur seaward of the original pileup.

It is clear from all the reported historical evidence that rideup/pileup/overtopping must be considered in rubblemound structure design. For structures that are used only during the ice free season, such as loading or unloading causeways at arctic ports, it may be permissible to allow rideup to occur. This approach has been recently used in the design of a causeway to serve the expanded Port of Nome. At least for the foreseeable future, the causeway will be used
only in ice free seasons for vessel loading and unloading. In this design, all critical utility services and pipelines are located in a utilidor below the surface of the causeway to permit winter rideup to go safely over top. Later, if winter operations are desired, the structure can be altered to include the rideup prevention features described below.

For structures that will be used or occupied during the winter or if not all critical facilities can be shielded from ice rideup, it is necessary to induce ice pileup at the perimeter of the structure so that no ice invades the working surface. The literature, field observations, and laboratory tests indicate that ice pileup may be induced by sharp breaks in slope geometry, by slope roughness sufficient to cause the advancing ice sheet to fail and induce a pile, or by a combination of slope geometry and slope roughness.

Croasdale, et al, (1978) suggested breaks in slope and jam features to induce ice piling. A break in slope from a steep to flatter slope induces a pileup at the apex of this slope by flexural failure. An extension of this concept is a horizontal berm in the slope profile which is used to accommodate the ice pile. Such berms have been used for riverine ice environments (Danys, 1979), and in Sounds (Brunn and Johannesson, 1971). A jam feature, or break in slope from a flat to a steeper slope is thought to induce ice piling by bending, crushing or buckling. Both features can be included by using a berm lower on the structure profile and a jam feature higher on the profile. In the case of the Port of Nome, should port operations be expanded to include winter operations, the causeway can be altered by building a low bench on the seaward side of the profile which will result in an overall profile with berm and jam features.

Another geometry which will resist ice rideup is the steep-sided or vertical-sided structure. Brunn and Johannesson (1977) studied ice rideup along coastal structures and concluded that vertical walls do not favor ice rideup if the toe depth is greater than some "critical depth". It can be inferred from their paper that the value of this "critical depth" is greater than 5 meters. Observations of rideup on ice islands grounded in over 10 meters of water in the Beaufort Sea indicate that the "critical depth" concept is not the only factor determining whether or not ice override will occur upon meeting vertical faces. Grounded ice island fragments typically have vertical walls and a freeboard of three to four meters. It is noted that the existence of a critical depth is probably a function of ice thickness, strength, orientation to wall, direction and magnitude of ice movement, velocity, integrity of the ice floe, and other factors. It is clear, however, that a vertical face of some height is effective in preventing ice rideup.
The volume of ice to be accommodated as a pile on the structure is related both to the design event of ice movement and whether the ice piles landward or seaward once piling starts. It should be remembered that ice encroachment on to the structure will happen only if a previous pileup did not occur, or if a grounded ridge does not prevent the level ice sheet from impinging directly on the structure. Given such a clear "shot" at the structure, the volume of ice in one event is related to the intensity, duration, and direction relative to the structure of the forces moving the ice sheet.

Since winds seem to cause the largest movement of ice sheets, the design event is the joint probability of wind speed, wind direction, wind duration, and the lack of previous pileups or grounded ridges on the structure. Without these statistics, an "extreme" design event can be selected on the basis of the longest documented or expected ice sheet movement.

Once island design features induce pileup, the piling process and the pile itself are the next elements of design concern. Ice piling on coastal structures has been described by Brunn and Johannesson (1977) to heights of ten meters above the still water level. Kovacs (1975) reports measured ice pileups as high as about 14 meters on an exposed portion of Banks Island in the Canadian Beaufort when the offshore ice was about 7 feet thick. Ice pileups have been estimated as high as 100 feet. In the Alaskan Beaufort, ice pileups observed on Cross and Narwhal Islands had mean heights and peak heights in excess of seven meters and 12 meters, respectively. Ice pileup also reached to 23 meters above sea level against the cliffs at Barrow, Alaska (Kovacs and Sodhi, 1979, 1980). Kovacs and Sodhi describe the ice piling process on flat beaches as proceeding generally seaward after the pile apex forms over the shoreline. By using pile height, a 30° side slope (Kovacs and Sodhi, 1979, 1980), and the apex position, the design volume of ice can be accommodated in the design. There is some laboratory evidence that on smooth steep slopes with a thick and fairly strong ice sheet, the original pile may be submerged by the advancing ice sheet, causing the pile to grow landward instead of seaward. However, thick ice sheets move only in Spring near breakup when they are usually weak. Thus, this event in nature would seem to be rather unlikely. Of course different slope geometries will accommodate different volumes of ice in "storage" on the structure. It may be necessary to clear ice pileups from the structure if it "fills in" the slope geometry. Otherwise the next advancing ice sheet may see only a constant slope and not the slope break or jam features.

Slope roughness can also be considered as a way to induce ice pileup. As the advancing ice sheet accommodates
roughness, its leading edge is retarded while the driving force continues, leading to a buckling/bending failure, which induces a pileup. Again some maintenance or clearing operations might be in order because a subsequent ice movement may ride over the previous ice, thus rendering the slope roughness ineffective. The size of the roughness feature relative to the ice thickness would seem important or bridging over the armor units would effectively cancel the roughness. However, the degree of roughness or smoothness presented by the armor units is also a concern in causing local slope failures described below. Another scheme is to use "tank traps" or "parapets" (Vaudrey and Potter, 1981) which present both a projection from the surrounding surface (roughness) and the opportunity for bending failure in two directions.

Ice overtopping is easily accommodated in the design by allowing a buffer zone for the ice chunks. Anything damaged by falling ice should not be placed within the buffer zone. The volume of overtopping ice must be accommodated in order to keep it from spilling onto the work surface of the structure.

Displacement of Armor Units: Displacement failure of the armor layer under ice action can occur by "bulldozing" during ice push or "plucking" when the ice freezes around armor units and then retreats away from the island. There is no directly applicable experience recorded which would indicate how various armor units interact with intensive ice conditions such as those found in the Beaufort Sea. However, some insight may be gained from the following reports.

Studies of naturally occurring boulder pavements subjected to ice floes along the MacKenzie River showed that some boulder pavements are remarkably stable, and may persist for a century or more (Mackay and MacKay, 1977). These pavements were characteristically composed of boulders tightly pressed into stony river muds with a preferred boulder long axis orientation parallel to the river bank, that is, in the direction of ice motion (See Photo 1, provided by D.K. MacKay). It is significant that the natural configuration of these ice-attacked shores is a smooth-topped "pavement"; it was concluded that loose boulders become pressed into the mudbank to form the pavement. It was noted, however, that although the boulder pavement as a whole was stable, individual boulders were occasionally removed from the pavement and replaced by a traveling boulder from an upstream source. Thus, it seems that a smooth pavement of armor units of sufficient size could be the preferred natural condition under the impacts of ice against a slope.

The stability of armor units in coastal ice environments is not well documented, although it is known that ice push
has moved boulders and trees along natural coastlines (Mackay and MacKay, 1977, and Peterson, 1965), and that ice rideup on a breakwater in Denmark caused "considerable damage" to the armor layer (Brunn and Johannesson, 1971). Danys (1979) reported that the loose rubble armor on the artificial islands in Lac St. Pierre along the St. Lawrence River were subject to combined forces of ice pileup, wave attack, and settlement action. The islands required extensive maintenance and were completely rebuilt eleven years after original construction. The armor rock used is estimated from published drawings to be about 1 to 2 tons. Other experience, however, of breakwaters located in the Gulf of Saint Lawrence shows that wave spray tends to freeze armor units in place, in effect forming a protective ice foot which forms before the presence of large ice floes and remains until after breakup (Ploeg, 1980). A tetrapod breakwater in northern Finland also experienced the protective ice coating (Kjelstrup, 1963). Reliance on this effect is not reasonable for some locations such as the Beaufort Sea since impact by large ice floes can occur when air temperatures are above freezing.

"Bulldozing" of armor units by ice is not restricted to the water level. Moving ice sheets often contain ice ridges with keels which scour the sea bottom as they move. Kovacs and Mellor (1974) show side scan sonar survey records of the bottom of the Beaufort Sea which are covered with scour furrows dug by ridge keels traversing the area. Kovacs and Sodhi (1981) report firmly grounded ridges even at the steep-sided Fairway Rock in the Bering Strait. Scour at the toe of an armored slope by ridge keels could cause the slumping downslope of the armor layer by wave attack during the following summer season. Although a rubble mound structure is "flexible" in that dislodged units are often compensated for by the shifting of nearby units to fill the gap, damage to the supporting toe structure may lead to the collapse of large sections of the armor layer above it. Concerns of scour by ridge keels also applies to arctic submarine pipelines (Pilkington and Marcellus, 1981).

In areas where the tides are high and the current action is strong, rubble mound structures are subject to impacts by ice floes and to "plucking" by ice action. Plucking occurs when armor becomes frozen to a piece of ice which is subsequently lifted by a high tide or a storm surge. The armor unit is "plucked" out of the rubble mound slope, floats away with the ice, and is dropped elsewhere when the bond is broken. High currents in rivers or tidal areas may cause ice impact damage. Ice floes carried by the current, and also influenced by the waves, may batter a rubble mound revetment causing displacements of armor units. Several bridge crossings and railroad revetment slopes in Cook Inlet suffer from such ice actions; some of the armor units can be seen deposited on the mudflats at low tide.
DESIGN DEVELOPMENT APPROACH

From the foregoing review of ice-structure interaction, arctic marine structure design can be divided into two main concerns:

1. Geometry of the structure (to induce ice pileup, to prevent rideup, or to permit rideup with safety).
2. Armoring of the structure (to maintain the geometry against ice and wave attack).

Since it is not clear from available information how these two parameters should be treated in the design of arctic slope protection, a "design development" approach was used in Tetra Tech's AOGA Project #126 to determine the relative effectiveness of various combinations of geometry and armor in providing good designs for a range of uses.

Geometry Considerations: The review of the available literature on ice-structure interaction, combined with knowledge of ice pileup/rideup processes and wave-structure interaction processes leads to a full range of candidate geometries. These alternative geometries progress from simple slopes to vertical walls with combinations of large-scale roughness included. The main concern with geometry features is to find the appropriate design for the degree of ice management required for the structure's function and site.

GEOMETRY ALTERNATIVES

1. Simple Slope (various steepnesses)
2. Simple Slope with "Tank Trap" Parapets
3. Bench (or "Berm" Design)
4. Bench with "Tank Top" Parapets
5. Simple Slope with Vertical Wall at Crest of Structure
6. Simple Slope with Vertical Wall at Crest of Structure and "Tank Trap" Parapets
7. Vertical Wall at the Waterline (Caisson Design)

It must also be kept in mind in selecting a geometry that all the factors in Tables 1 and 2 apply in deciding overall if the structure is functional and economically justified. For some geometries, wave and ice behavior may run counter to each other. For example, on flat simple slopes, wave runup is reduced, necessary armor weights are generally less, but ice rideup is more possible, and, if the slope is smooth, possibly even enhanced. For other geometries, wave and ice behavior may both be improved. On
the bench design for example, wave runup is reduced and the slope breaks may also cause ice pileup to occur. From another point of view, on vertical caisson-type designs, ice pileup is definitely initiated at the cost of high ice pressures, and fill requirements for a specific structure size are reduced at the cost of higher costs for steel or concrete for the caissons.

Armoring Considerations: The often conflicting requirements of wave and ice concerns are also manifested in the choice of slope protection armor. Again a full range of alternatives are available, some of which are appropriate to only certain of the geometry alternatives. The main objective of the armoring is to effectively maintain the slope geometry so that the ice management features designed into the structure continue to work:

<table>
<thead>
<tr>
<th>ARMORING ALTERNATIVES</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Discrete Armor Units</td>
</tr>
<tr>
<td>- Quarrystone</td>
</tr>
<tr>
<td>- Concrete Block</td>
</tr>
<tr>
<td>- Cast Shapes (Tetrapods, etc.)</td>
</tr>
<tr>
<td>2. Fabric Containers</td>
</tr>
<tr>
<td>- Sand Bags</td>
</tr>
<tr>
<td>- &quot;Bolsacreto&quot;, &quot;Bolsaroca&quot;</td>
</tr>
<tr>
<td>- Longard Tubes</td>
</tr>
<tr>
<td>3. Block and Cable Mats (Various manufacturers have somewhat different products)</td>
</tr>
<tr>
<td>4. Binders</td>
</tr>
<tr>
<td>- Soil Cement</td>
</tr>
<tr>
<td>- Bitumen</td>
</tr>
<tr>
<td>- Polyurethane</td>
</tr>
<tr>
<td>5. Gabions (Various material types and shapes)</td>
</tr>
<tr>
<td>6. Vertical Walls</td>
</tr>
<tr>
<td>- Sheet Pile</td>
</tr>
<tr>
<td>- Concrete</td>
</tr>
<tr>
<td>- Built-up walls of various other armor types above (appropriate to crest walls only)</td>
</tr>
</tbody>
</table>

Again, all the factors in Tables 1 and 2 must be brought to bear on the decision of armor selection for each individual structure case being considered. Also, it is again true in armoring considerations that the requirements to armor against waves and against ice may not necessarily agree. For
example, slope roughness and porosity are conventional ways
to dissipate wave forces on a rubble mound structure. However, in ice, it seems that slope roughness might be
appropriate to trigger ice pileup, but it might also lead to armor displacements until a "smooth" surface is presented to
the ice. It is not clear if armor displacement by ice in winter would be severe enough so that summer wave action
will find a "weak" spot upon which to cause further damage.

Design Selection: Given the multiple choices of geometry
types and armor types, the general lack of specific
knowledge on ice-structure interaction, and the often conflicting requirements of wave protection and ice protec­tion, how does one make a choice of what design to implement? As in most such instances when an optimum is
sought from among several alternatives seeking to satisfy several design conditions, a rating system is necessary. The
following seven criteria are appropriate:

Reliability/Functional Protection Provided

1. Ice management concerns: pileup/rideup/overtopping
2. Sensitivity of structure to damage

Construction Costs

3. Constructability of the design for the site and in arctic conditions
4. Availability of required materials
5. Tradeoffs of material types

Maintenance Costs

6. Durability under wave action
7. Durability under ice action

The relative weights for the seven criteria are entirely case by case dependent. For example, a permanent structure
with an intended long service life in severe wave and ice conditions will require higher weighting factors for
reliability and functional protection and lower weighting factors on, perhaps, construction costs. For temporary
structures in more sheltered areas, perhaps reduced constructed costs would be considered relatively much more important.

In AOGA Project # 126, such a rating scheme was used to examine all the various combinations of geometry and armor
alternatives in order to eliminate from consideration those least applicable to the questions at hand. Then, several of
the more highly rated alternatives were designed and evaluated under wave and ice attack by model studies. Tetra
Tech directed and managed the program and conducted the wave model investigations. Arctec, Inc. conducted the ice model investigations, and Austin Kovacs consulted on matters of ice-shore interaction, prototype ice conditions, and model data interpretation.

SUMMARY

The design of rubble mound structures for use in the arctic involves a broad range of factors including not only wave and ice conditions, but also very intensified constraints on speed of construction, material supply, and benefit-cost economy. These factors are summarized in Tables 1 and 2. In overview, functional design issues can be divided into geometry design (for management of ice-structure interaction) and armor design (for maintaining the geometry selected). Each structure must be carefully examined to determine how ice is to be accommodated in the design and how to best provide the necessary armor protection. A generalized set of criteria for such a design selection scheme is presented.

It is clear that there are very many solutions to the problem of appropriate marine structure design in the arctic. The various alternative combinations of geometry and armor type are appropriate depending upon site and functional conditions to be accommodated such as the intensity of the wave and ice regime and the service life required for the structure. By virtue of the generalized design development approach taken in AOGA Project # 126, there is now available more recent and specific scientific and engineering knowledge applicable to arctic marine structure design.

ACKNOWLEDGEMENTS

During the last several years during which Tetra Tech has been investigating the subject of arctic slope protection and design, many individuals have contributed their ideas and have provided opportunities for the investigations to proceed. To date these investigations have led to over ten reports specifically related to arctic slope protection. We thank Mr. Bob Potter of Sohio Petroleum for authorizing the first Tetra Tech efforts on this subject and all subsequent ones. We also thank the following organizations and individuals for their contributions and support: Amoco, Arco, Chevron, Conoco, Dome, Exxon, Mobil, Shell, Phillips, Arctec Inc., Mr. Austin Kovacs, The City of Nome, Aldek A.S., Longard Pacific, Nicolion Corporation, Control de Erosion S.A., Alaska Railroad, Mr. Ken Vaudrey, Dr. D.K. MacKay of Environment Canada, and numerous Tetra Tech staff members.
REFERENCES


PHOTOGRAPH 1
NATURALLY—OCCURING BOULDER PAVEMENT ON THE MACKENZIE RIVER
(COURTESY OF D.K. MAC KAY, ENVIRONMENT CANADA)

PHOTOGRAPH 2
ICE PILEUP OBSERVED ON A ROCKY BEACH
(COURTESY OF AUSTIN KOVACS, U.S. ARMY COLD REGIONS RESEARCH AND
ENGINEERING LABORATORY)
DISCUSSION

By:

Dr. D.V. Reddy, Memorial University of Newfoundland,
St. Johns, Newfoundland, Canada.

Have Reinforced Earth and geotextile concepts been considered to increase the steepness of the rubble linings making the construction more cost-effective?
DISCUSSION BY D.V. REDDY ON
"RUBBLE MOUND STRUCTURES
UNDER ICE AND WAVE ATTACK"

Paper C2-4, Volume 3

AUTHORS' REPLY
By:
Martin T. Czerniak, Arthur Shak, and J. Ian Collins,
Tetra Tech, Inc., Pasadena,
California, U.S.A.

Several techniques have been and are being investigated for building "fill-retained" structures in the arctic. Considerations to steepen the slopes of arctic structures is an important step toward reducing their cost, especially in the case of artificial islands built for hydrocarbon development and other fill structures. These structures can be built by "fill first" techniques in which the fill is dumped on location either by dredging, barge hauling, of winter truck hauling over ice roads. Then slope protection, such as was presented in the paper is placed over the fill. For deeper water depths, a "fill-retained" approach offers advantages in smaller required fill quantities and shorter construction times. Vertical-sided caissons of concrete or steel are one type of fill-retained structure. However, these are likely most applicable in deeper waters where the costs of fill saved overbalances the high cost of concrete or steel caissons installed in the arctic.

The second type of fill-retained island is the "slope-sided" kind which has side slopes approaching 1:1 and are built using alternating layers of berm building and backfilling. It is the underwater berm building aspect that you have brought up in this discussion. The following are various ways that have been considered to build this fill-retaining structure:

1. Plastic or steel gabions filled with gravel or small sand bags. Experience has been gained using both insitu-filled gabions, and gabions placed after filling.

2. Sand-filled fabric containers, either insitu-filled or placed after filling.

3. Textile formwork used to retain the fill material being placed.


5. Dumped rock and other granular material.
These slope-sided fill retained islands are economically more applicable to shallower water depths than are the vertical-side caissons. For this reason, they are presently receiving some serious consideration. We did not cover these in our paper because we sought to address only the shore protection aspects within the limited space available for the paper.
ABSTRACT

The success, efficiency and economy with which engineering works can be carried out in the arctic marine environment depend directly on the completeness and accuracy of the knowledge of arctic phenomena and process upon which the engineering designs and calculations are based. A very wide range of basic science is vital to the knowledge base for arctic marine engineering and development policies. However, reliable data on arctic conditions are few and in many cases are not representative of the areas or conditions to which interpretations must be applied; and the scientific understanding of many arctic environmental processes is incomplete. Large-scale resource developments have created an urgent need for sophisticated scientific information in many areas that until now have had little study, and thus progress in arctic marine engineering is tied directly to progress in basic arctic science. To use the available and incomplete information successfully, arctic marine engineers must themselves have an understanding of a wide range of geophysical, oceanographic, meteorological, biological and sociological factors, and be aware of the various ways that current scientific knowledge in these fields can be applied to different aspects of arctic engineering work.

Scientific research in the arctic has not kept pace with engineering or development planning. Already there are examples where engineering is handicapped by lack of basic data or ignorance of environmental processes. There is cause for grave concern that in the near future the state of arctic scientific knowledge will be inadequate to meet the requirements of advanced design, operations, and management policy, and that the supply of scientifically qualified persons, trained and specialized in arctic work, will be insufficient to meet national and international needs. Engineers, as the first essential users and beneficiaries of new arctic scientific knowledge, are in a strong position to demand and encourage a strong, broad and forward-looking program in the basic sciences in arctic marine areas; and it is in their own interests that they do so.

1. INTRODUCTION

It is first my duty and pleasure to bring you greetings and best wishes from the International Commission on Snow and Ice. The International Commission on Snow and Ice, or ICSI as we familiarly call it, is one of the commissions of the International Association of Hydrological Sciences, which, if one follows upward through the pyramid of international professional scientific organizations, is a
component of the International Council of Scientific Unions, representing organized science throughout the world. ICSI has national representatives in 47 countries, virtually every country that has ice or snow problems or scientists working on such problems. It has four subject divisions, one of which, dealing with "River, Lake and Sea Ice" is of course directly concerned with the substance and problems of this conference and of the sister symposium down the street, of which we are a co-sponsor. To the extent that I can speak for the Commission, on behalf of the snow and ice scientists around the world I wish the conference every success.

The POAC Conferences have a very simple general objective, expressed at the first meeting in Norway in 1971: "to improve the knowledge of port and ocean engineers on arctic problems". The increasing size and scope of the conferences, and the ever more sophisticated nature of the presentations must mean that port and ocean engineers every year have more and more need to improve their knowledge. From this, one can draw some conclusions: either (1) each year they are getting less intelligent; or (2) each year there is more and more to learn; or (3) the process of coming together to improve our knowledge is enjoyable and profitable. As an ordinary wearer of the Iron Ring, I am not qualified to comment on the first possibility; and so I must assume that the second and third reasons are why you and I and all the rest of us are here today.

The practical problems of identifying and coping with arctic conditions in marine and shore areas are the main substance of this conference. But these problems, and our means of dealing with them, are rooted inescapably in our understanding of the fundamental natural characteristics of the arctic regions, and of the natural processes that determine the arctic environment. It is that understanding, and its relationship to the practical questions of design, operations, and economics, that I would like to discuss with you today.

The Conference Chairman asked me to say something about the sciences of meteorology and oceanography, as they applied to POAC; but I found I could not do that without straying into geophysics at one end and biology at the other, for they make an interconnected base of knowledge upon which the arctic marine engineer must build.

I am not going to pretend to present a thumb-nail sketch of arctic science: this is the wrong place for that, and anyway, no sober person would attempt to do that so soon after lunch. Instead, I would like to ask you to think for a few moments with me on what are the distinctive characteristics of the arctic; what do we know about them; and then reflect a little about the interaction between what we know or don't know and what we do or don't do in technical, operational and regulatory fields concerned with arctic marine and shore activities. In taking this approach I am going to be saying things which are obvious and which everybody knows; but sometimes, because we take for granted things with which we are familiar but don't really understand, we leave ourselves vulnerable to making costly mistakes or following misguided policies.

2. "SCIENCE" and "ENGINEERING"

In these comments I am inevitably going to be speaking about "science", and "engineering", and sometimes about "basic science" and "applied science" as if they were quite different things, and it is only proper that I be asked to define my terms. Like most of you, I dislike quibbling definitions and am impatient with those who attempt to draw artificial distinctions through closely connected activities. But for our discussion today I want to think about the range of activities related to northern marine areas, from the systematic and formulated pursuit and organization of knowledge, which is the essence of science, to the application of knowledge of the properties of matter and the processes of nature to the design and construction of works of public utility, which is engineering. I am going to refer to the development of knowledge that is not directly connected with its practical
application as "basic science", even though the utility of the knowledge may be apparent; and science that is organized in such a way as to serve pre-identified economic or policy ends will be called "applied science". I don't want to make too much of these definitions. These subjects do not form separate compartments of thought or action; many engineers engage in pure science, and the result of their contributions to basic knowledge are subjected to open scientific scrutiny at occasions like this conference, while abstract and theoretical science is often an essential part of a major engineering analysis or design. Rather, they form a continuous spectrum, and it is the flow of knowledge and motivation, in both directions, along that spectrum that has a lot to do with the soundness of our engineering and the utility and economy of our works and operations in northern areas.

3. **ARCTIC CONDITIONS**

3.1 **What are they?**

The term "Arctic conditions" is an integral part of the POAC title; it makes half the name. It is the fact of arctic conditions, and the fact that such conditions differ from conditions elsewhere that bring us here; that is the reason for the POAC conferences. If you ask almost anyone in this room what is special about arctic conditions, that person is apt to reply immediately, "Cold". But it gets cold elsewhere that in the arctic - at Quebec City, for example. So after reflecting a little, he or she is likely to say, "Well, it stays cold a long time", and then after a bit, "and it is dark all winter and light all summer", and so on, bit by bit, a picture of what constitutes arctic conditions begins to take shape.

No one environmental or physical factor determines arctic conditions. It is a combination of factors, which produces a result recognizably different, economically and socially as well as physically, from the conditions in lower latitudes that determines our concept of "arctic". The climate of Tuktoyaktuk has in many ways more in common with the climate of Thunder Bay than with the climate of Archangel or Tromso; yet to most people Archangel and Tuktoyaktuk are unmistakably arctic ports, and Tromso probably too; while Thunder Bay is not. Regardless of where one draws boundaries and definitions, arctic conditions have a principal common cause in the distinctive radiative and geophysical consequences of the earth's tilted axis of rotation with respect to the sun. The obvious manifestations of this tilt, which characterize arctic regions, include:

a) **Low temperatures**, with extensive periods colder than the freezing point of water. This has many consequences, of which two simple ones dominate in making arctic conditions and processes distinctly different from those in most other parts of the world:

i) water at the surface of the ground, rivers and lakes, or the ocean, and in the air, is commonly in the solid phase, rather than liquid; and so energy which flows into or out of the system results largely in change of phase rather than change of temperature. This phenomenon has implications for the direction and speed of chemical reactions; and

ii) the vapour pressure in the atmosphere, and likewise osmotic pressure in organic cells, is reduced. This circumstance has effects as varied and important as the persistence of snow and rive on an air intake, the strength of a field of sea ice under compression by wind, the persistence of spilled oil in a shore lead, or the ability of algae and bottom organisms to become established on the newly deposited underwater part of an artificial island.
b) **Unusual patterns of day-and-night cycles and distorted radiation balance.** With incoming solar radiation, received at a low angle, concentrated in the summer months and outgoing radiation discharged perpendicularly to space at all times of the year. The result is a net loss of radiant energy from arctic regions, which must be balanced by heat imported through atmospheric and oceanic circulation. This circumstance provides the underlying driving force for the weather, sea ice and ocean dynamics with which arctic engineers must deal.

c) **Distinctive magnetic and electromagnetic properties of high latitudes.** Because the spinning earth acts as its own electromagnetic armature in the solar magnetic field, electric and magnetic phenomena near the poles are a distinctive part of arctic conditions. Magnetospheric phenomena such as the aurora, a fluctuating pattern of earth and ionospheric currents about the geomagnetic pole, steeply dipping magnetic lines of force and weak and rapidly fluctuating horizontal magnetic field strength have direct effects on such things as the feasibility and reliability of navigation and communication systems in the arctic, and although the evidence and explanations are not at all clear, may have an influence on weather and climate.

d) **Biological features distinctive of arctic conditions.** Although there are many ways of defining and describing the distinctive and specialized characteristics of arctic marine and terrestrial biology, some of the most typical features of arctic life, which are important to engineering plans, designs and operations, can be listed crudely as follows:

- **Low primary productivity.** The rate at which carbon is fixed into organic compounds through photosynthesis is much lower in the arctic than in lower latitudes. In other words, food production at the most basic level is small, so that no matter what happens farther up the food chain, there just is not enough organic energy in the system to maintain life processes as vigorous as in most other parts of the world;

- **Ecosystems characterized by few species, simple food chains.** Populations fluctuating from few to very abundant, often with remarkable behavioural adaptations, all developed in order to make the best use of the small amount of available energy and the high degree of variability characteristic of the arctic environment;

- **Comparatively high vulnerability to change.** And sensitivity to external influences. This of course is a consequence of the features described above. Arctic populations are typically highly stressed, not because of competition from other life forms but by physical conditions; they exist at the margin of biological tolerance, and there is little or no reserve energy with which to counter a disturbance or to recover after damage. But this does not mean, as is sometimes stated, that arctic life systems are delicate or easily destroyed. Having developed in a part of the world where living is tough, where disasters and local extinctions are commonplace, and where there is no margin for error in adaptations, arctic ecosystems are easily disrupted locally, but very tough and persistent over the arctic as a whole. However, in human terms, recovery or repair may take a very long time, and a single human action, such as dredging a channel in an estuary, which in southern waters would create only minor and temporary disturbance to the biological system, could in arctic waters have devastating effects on arctic life over a wide area for decades. That difference in vulnerability to imposed change, and in the scale and rate of recovery, is a typical arctic condition.
e) The human factor. No cursory summary of the elements comprising arctic conditions can overlook the special features of humans in the arctic. Human beings are part of the environment and the ecosystem, whether they are hunters on the sea ice or technicians repairing a computer; and nowhere does this fact become so apparent as in the Arctic. Putting aside questions of culture, race, history, economics or reasons for being there, it is easy to distinguish two kinds of humans in the arctic. Both kinds are found in all arctic countries. Thomas Berger summed up the differences from an attitudinal and behavioural point of view when he stated that to some, the arctic is a homeland; to others, it is a frontier [4]. For our purposes, in looking at the human element as it affects the arctic environment in a way that is important to marine engineering, we can note a very important difference between the two kinds: there are the low-energy humans and the high-energy humans. The low-energy humans often obtain part of their resources locally, although the proportion of strictly local supply and use is diminishing in all arctic countries. Their activities fit fairly well into the biological and environmental processes of the arctic without much disturbance and in the marine area they are at the top of the food chain. The high-energy humans, on the other hand, depend upon continuous supplies of large amounts of energy (at present, almost all imported) for their own needs and to support their activities. The expenditure of this energy causes disturbance of the land, the rivers, the shorelines and their biology, and is increasingly changing the surface characteristics of the ocean through deposited air-borne pollution. Thus high-energy humans have their main environmental effect much further down in the food chain and in the ecosystem.

In the marine area the main role of high-energy humans may be dominantly with the lowest trophic level, affecting primary productivity and the activities of the simplest organisms. The environmental differences between these two kinds of arctic humans should be kept in mind when one considers the interactions between the arctic environment and engineering.

"Arctic Conditions", then, are a complex of factors, some very inflexible, definable, and set inexorably by the planet itself, others much more subjective, comparative, and flexible. It is this shifting complex of conditions, nearly all of them leading to increased costs and the need for special technologies and approaches, with which arctic marine engineers have to deal. How well they deal with them depends to a considerable extent on how well the basic facts of arctic geophysics, meteorology, oceanography and biology are understood.

And yet, arctic conditions are more than cold facts and geophysical or biological phenomena. Perhaps the best definition of severe arctic marine conditions that I have heard was given by an Inuit friend and colleague. In 1969 we were camped on the sea ice only a few kilometres from the North Pole, and my friend, whose home was in Resolute, Latitude 75°N, which most people would consider well into the high arctic, was very conscious of the marked difference between the North Pole and his home village. The North Pole is as far north from Resolute as Quebec City is from Savannah, Georgia. After we had been there a few days, I asked my friend if he liked it there. He said, "Nope. No fox. No Women. Sun gone crazy." Note these four conclusions; they contain the elements of observation, analysis, and interpretation of all the environmental phenomena with which arctic marine engineers have to deal. First the subjective interpretation, wrapping up all the observations and deductions, into a psychological reaction; then a practical observation on economics and the natural environment; then the human aspect; and underlying it all an observation on the geophysical cause. Note also that some of those characteristics of the arctic can be changed or improved by better knowledge and engineering; - but we can do nothing about the underlying cause.
3.2 What causes Arctic conditions?

In a simple way, the basic causes of arctic conditions are easy to list. To explain the circumstances that led to these causes, and the manner in which the different factors interact and affect the marine environment, is much more difficult. Here are the main causes as commonly understood and described; they are by no means independent, and some are direct consequences from others:

a) The geometry of planet Earth and its orbital motions determine the radiative, magnetic and electrical characteristics of high latitudes, thus leading to arctic temperatures, distinctive day-night cycles, seasonal and year-to-year variations of environmental conditions, low biological productivity, magnetic storms and distinctive ionospheric characteristics.

b) The geological history, in particular the evolution of the continents and ocean basins, has determined the architecture of the arctic regions, which in turn controls the pattern and rate of oceanographic and atmospheric circulation, the movement of sea ice and, to a large degree, of pollutants. The geological history has also determined the composition of materials on the sea floor and shoreline, the changes of sea level and physiographic evolution that have resulted in the location and characteristics of harbours, channels, and undersea permafrost. And most importantly, of course, it has determined the location and nature of mineral and petroleum deposits.

c) The climatic history, which has determined the major fluctuations in sea ice, and in ice and snow on the land in the past hundred thousand years of so, with profound effects on the physiography of arctic regions above and below sea level, and on biological systems. Climate changes in the recent past, and those occurring at present, have effects that influence current engineering; and indeed future changes of climate will have to be accounted for within the lifetime of the amortization of present investments.

d) The biological history, which is a consequence of the geological and climatic histories of the arctic regions, and the history of the evolution of life in lower latitudes, which have provided the reservoir of life forms that have migrated to the arctic and adapted to its environment when conditions were right. Ecosystems in the arctic ocean appear to be relatively young and immature - probably all have developed since the Pliocene -; and the present diversity of species and behaviour appears to be a compromise between the effects of low temperatures and restricted solar energy (which would tend to lead to greater variety of evolved forms, even if at a slow rate), and variability of environmental conditions due to the tilted Earth's axis and the general youthfulness of systems, both of which lead to restricted diversity [8, 20].

e) Human activities, which influence arctic conditions in a number of ways, and at different scales, for example through changes to the shoreline and rivers, thus altering erosion and sedimentation patterns; by influences on arctic biology ranging from virtual elimination of the bowhead whale by hunting in Baffin Bay to local destruction of critical coastal fauna by pollution of shore leads; by altering the arctic thermal balance due to increase of CO₂ concentration in the upper atmosphere, etc.

These various causes have resulted in the present and dynamic Arctic conditions with which we have to deal. We cannot, in engineering structures or activities in the arctic, do much about the causes themselves, although we can have some influence on the character and severity of the local "human factors". What I think
is important is to consider how well we understand these causes and the natural characteristics that flow from them, and how we use that knowledge to bring about economical, resource efficient, environmentally sound and socially acceptable human activities in arctic regions.

4. THE UNDERSTANDING OF ARCTIC MARINE CONDITIONS

I do not intend to review our state of knowledge. Instead, I will leave with you some personal comments on how well it seems to me the researches and experiences to date, including the experiences of people who have lived there for a couple of thousand years, have provided an understanding of the basic natural elements of arctic conditions with which marine engineers must deal. (Some of these elements, as they affect our understanding of the nature and stability of the Arctic Ocean environment, have been reviewed in greater detail elsewhere [28]).

4.1 Geophysics, geological history, and tectonic evolution

The general features of crustal structure in the arctic regions are reasonably well understood, although very little data from the Eurasian side of the basin are publicly available, and we are only beginning to obtain any reliable details of the shape or geophysical character of the prominent ridges that cross the Arctic Ocean basin. The interpretation of the origin and the evolution of some of these features is still a matter of controversy and speculation. The Arctic Ocean basin is relatively young, as ocean basins go; the earliest recognizable parts of it began to open about 300 million years ago; the part presently joined to the Atlantic Ocean is probably not more than 60 million years old and is probably still actively spreading. The ideas concerning the steps of development of these features, and the uncertainty about their nature, are important to arctic marine engineers today because upon them depend, in the absence of detailed data, the explanations for such things as:

- the nature and distribution of marine and shelf sediments;
- the stability of coastlines, the growth or erosion of estuaries and deltas and other features critical to harbour and shoreline installations;
- the changes or fluctuations of sea level (which of course is not the same as stability of coastal areas);
- the geothermal gradient, onshore or offshore;
- the probability and likely distribution of earthquakes;
- the geological response to changes in climate, as manifested in such features as submarine permafrost and pingoes, ice-scoured sea floor, the distribution, nature and future evolution of ice-pushed and ice-eroded shorelines, and the extent and distribution of permafrost on land;
- the variability and predictability of magnetic and electromagnetic effects, which bear directly on the feasibility and reliability of navigation and communication systems.

The blunt facts are that we don't know enough about the dynamic geophysical and geological characteristics of arctic marine areas to give design engineers or planners firm base data on the stability and distribution of the materials, or the behaviour of the natural phenomena with which they must work. Nor do we understand enough to know whether the few quantitative data that exist are representative of the things they purport to be measuring. Most arctic geophysical and geological information with which the marine or offshore engineer has to work are interpretations or extrapolations, based on incomplete understanding and relatively little firm data, and the interpretations are necessarily strongly influenced by the speculative interpretation of the origin and history of major arctic features. Weber [31] has dramatically demonstrated the influence of different schools of geological interpretation on such an apparently objective activity as making bathymetric charts of the Arctic Ocean.
Marine engineers must keep in mind that much basic information about the arctic is not factual, or confirmed, and no one may know how representative the few measurements are. Design engineers and planners have a responsibility to their clients and to their profession to become aware of the state of scientific knowledge and areas of uncertainty or controversy, and to use the available data in the light of this awareness.

4.2 The Arctic Energy Transfer System

Much closer to the day-to-day work of most arctic marine engineers is the knowledge, or lack of it, of weather conditions, ocean currents, characteristics of the ocean water masses, the presence or absence of sea ice and the nature of the ice. I don't need to tell anyone at this conference that our knowledge is incomplete in these areas. To a large degree it is the incompleteness of knowledge, and the apparent variability and lack of predictability of atmospheric and oceanic phenomena that presents engineering difficulties in the arctic, keeps costs high and often makes success uncertain. It is what keeps life interesting for the arctic marine engineer.

The basic characteristics of the arctic oceanic and atmospheric environments and their dynamic nature, have been ably described [17; 2; 32, 21; 24]. What is important in this context is that the distinctive characteristics and variations in arctic weather, sea ice and ocean conditions are manifestations of the great planetary energy system that results from more incoming solar radiation being received in the tropics than is lost by radiation to space, and more loss of heat by radiation to space from the polar regions than is received through incoming solar radiation. The planet is kept in approximate thermal equilibrium by the transfer of heat from the tropics to the polar areas through oceanographic and atmospheric circulation. The circulation of the oceans and the atmosphere are intimately connected, of course: atmospheric movements, or winds, drive the surface ocean currents, and ocean temperature differences heat the atmosphere to generate the winds. The pattern and rate of movement is enormously complicated by the rotation of the earth and the peculiar distribution of continents and ocean basins, which channel the directions of oceanic circulation and heat transfer, and influence the three-dimensional structure and movement of air masses. This global energy transfer pattern may at first not appear to be of much relevance to the engineer concerned with calculating the ice forces on an offshore structure; but an appreciation of the larger picture is important if one is to be able to assess the significance and representativeness of scattered data on, say, wind fields or means and extremes of ice thickness, or to make intelligent use of a short-term experiment that leads to a drag coefficient or momentum term that may or may not be representative of what will happen in the conditions or area where one wants to apply the results. Also, it should be remembered that, in the major projects being considered for the arctic offshore, engineers are designing structures or transport systems for a thirty-year operational lifetime. During that period the atmospheric or oceanic circulation pattern will have changed, perhaps more than once, in important ways that will affect the economy, safety, and feasibility of the operation; the only way to assess current information and to identify and allow for areas of likely environmental variation is to be able to relate local data to the best possible understanding of regional and global environmental dynamics.

An appreciation of the dynamic and interacting nature of arctic environmental conditions is essential for sound engineering for arctic projects. Some of the conspicuous mistakes of the past have been the result of dealing with the element of arctic conditions as separate entities. It helps, in trying to work with sparse data from the arctic, to keep in mind that arctic weather and oceanic circulation and ice conditions are basically energy transfer or energy replacement systems and part of global scheme. Weather does not start in the arctic, as some journalists like to
say, although the conditions of rapid heat loss due to a number of reinforcing local factors may lead to storm systems or masses of cold air which move southward at times and give the rest of us a feeling that the Arctic is exporting its weather. Another general factor to keep in mind is that, because of the greater heat capacity of water compared to air, by far the greatest amount—perhaps nine-tenths—of the heat carried from the tropics to high latitudes is carried by the oceans. But the atmosphere moves its energy from place to place perhaps 100 times as fast as the oceans. The oceans can store heat, or maintain enormous masses of comparative coldness, for very long times—thousands of years, apparently, for the cold nearly stagnant bottom waters—, while the atmosphere can hold it a few days at most. It is the unstable energy marriage between the comparatively stolid but thermally dominant ocean and the flibbertigibbet atmosphere which is a thermal lightweight but very nimble that produces the vagaries, both short and longer term, in the arctic environment. The engineer needs to have a general understanding of these variations if he is to assess the usefulness of available data. Most time-series data in the arctic are very short. But 7 year's data on winds may have different usefulness than, say 7 year's data on ice movement or ocean density profiles.

A third general characteristic of the atmosphere-ocean component of the arctic energy transfer system which it is essential for the engineer to appreciate is that a whole new thermal and biological regime is entered, above and below the ocean, as soon as the air-sea interface cools below 0°C and a layer of ice is formed. This may seem so obvious to everyone at this conference as to be pointless to mention; but for a moment let us forget about the ice itself and its properties and consider what happens to the atmosphere and to the ocean and the things in it. These effects are very pertinent to the success or failure of engineering structures and vessels in meeting performance, economic, environmental, and policy goals. Some of the points to be considered are:

When ice forms on a previously open ocean—

i) the exchange of energy between the ocean and the atmosphere is reduced two to four orders of magnitude as compared to an open ocean, and the gain or loss of heat on both upper or lower surfaces of the ice is dominated by the latent heat of melting or freezing, not directly by the temperature gradient;

ii) gaseous exchange between the ocean and the atmosphere, which in the open ocean takes place over the whole surface of the ocean, is drastically reduced, and occurs mainly through cracks or leads;

iii) the reflectivity (albedo) of the ocean surface is increased enormously; most of the solar radiation is reflected back into the atmosphere, decreasing its thermal stability; what solar energy does enter the ice is further attenuated, and spectrally filtered, so that under typical Arctic Ocean conditions only about 1/100 of the solar energy that falls on the ice actually penetrates into the upper layers of ocean water, and that mostly in the shorter wave lengths;

iv) the chemistry of the ocean water is characterized by seasonal extraction of salt which is rejected to lower levels, and by periodic flooding of the surface layer, under or around the ice, with fresh water;

v) much less kinetic energy is exchanged between the atmosphere and the ocean, but momentum and compressive forces from winds or ocean movements may be transferred to the ice and transmitted over tens or hundreds of kilometers by the ice itself [23; 29].

The state of knowledge of these processes, and the assumptions used to determine or describe the typical characteristics or range of characteristics of sea ice, atmosphere, and oceans in arctic area, enter nearly every engineering calculation. It behooves us to think very carefully how much we know about these processes, where we have obtained our numbers from, and whether there are some important factors that we have quite neglected [6].
4.3 The Living Arctic

Marine engineers traditionally don't have much to do with the biology of the ocean - unless they are designing trawlers fish-finding gear. But in the arctic, where at first glance there is less evidence of life than elsewhere, every major project has to keep biological factors in mind at every stage. The low primary productivity and general sparseness of life forms in the northern oceans does not mean that biological matters are unimportant, or that they can be left to look after themselves; on the contrary, the comparatively simple arctic marine ecosystems are enormously important and essential parts of the environment, and because they are easily disturbed by human actions, require special attention. The concern with marine life is not just altruistic; - the sub-arctic waters contain some of the world's richest fisheries, despite the slow unit growth of their stocks, and that richness depends at least in part on the biological characteristics of the less productive waters farther north that contribute to the productive conditions; some favoured places are the breeding or feeding sites for migratory birds from half a hemisphere or marine mammals from a large part of the northern oceans; and for the native cultures and settlements, the continued presence of accessible marine renewable resources is an essential part of their way of life, their economy, and increasingly, of their politics.

The degree of scientific understanding or ignorance of arctic biological systems can affect engineering aspects of arctic development in several ways. The most obvious is that most environmental concerns and policies about renewable resources in all northern countries have a dominantly biological component; and in designing equipment or operations to meet environmental specifications, the engineer must deliberately design to control the effect of his equipment or activities on the biosphere. But if the understanding of the biological systems is faulty, the design targets may be quite wrong; and careful engineering undertaken with insufficient biological knowledge may result in a design that meets the specifications but fails to achieve its environmental or resource protection purpose. For example, if it is necessary to develop a shipping system that will not disturb the whales in a particular area - and that should be entirely possible -, one has to know enough about the ecosystem, and the habits and reactions of the whale populations in the area as well as those of the main elements of the whales' food chain before one would know whether the principal threat to whales in that area would come from, say, underwater noise, interfering with the whales' communications system, or from disturbance and break-up of the ice which prevented accumulation of copepods and other small organisms that made up the whales' diet or the diet of those on which the whales fed -- or from any of a number of other possibilities, each of which might cause the whales to leave or to stay, to live or to die, depending on the characteristics of the shipping system developed. The engineer cannot help but be a biological agent, for he contributes to the decision of whether the best way to avoid disturbing the whales, in hypothetical a case such as this, would be by (a) designing propellor and cooling water exhaust systems that don't put a large amount of energy into the water in the whales' sensitive acoustic frequency range; or (b) scheduling a routing and navigation system which diverts at critical times of the year so as not to disrupt the breeding and growth of certain lower animals upon which the whales feed; or (c) being extremely meticulous about on-route discharges of trace amounts of oil in certain areas because nursing whale mothers are more sensitive than grown males to aromatic hydrocarbons, etc. To help make such decisions sensibly, to estimate their costs compared to the likelihood of achieving the desired biological result while still achieving an efficient shipping system, the engineer must have adequate biological information.

Arctic biological activities themselves may directly impose engineering problems. Engineers of arctic submarine defense systems tell anecdotes about how vociferous groups of beluga and seals play innocent havoc with under-ice listening and sonic navigation networks. One cannot ask the animals to change their communication frequency, so it is up to the engineer to adapt to the biological environment.
The solution to all these kinds of problems, of course, is to know enough about the biology and the ecosystem to be able to take appropriate details of it into account in equipment selection and design and in operational procedures. This requires a sensitivity to things biological that marine engineers are seldom called upon to possess in their ordinary work in other parts of the world, where biological disturbance is much more easily accommodated by the ecosystem and damage is more quickly repaired. There is no single or general-purpose solution to biological problems; - broad-brush treatment for protection of conspicuous species, which is what most regulations tend to impose, easily becomes a waste of money for little real benefit because it cannot allow for the variations and vagaries of living things. And tests or pilot runs of the effects of one practice or another on slowly adjusting arctic marine populations are just not feasible in advance of a major development decision. The only effective course, as in most things, is to know enough about the subject and the problem to assess the available evidence and data and use it in a practical and sensitive way. To do this, arctic marine engineers must have an awareness of the state of understanding of arctic marine biology.

Although a lot is known, there is a great deal that is not known about marine life in the arctic and the functioning of arctic ecosystems [10, 11]. Some of the areas where information is sparse and understanding is admittedly not very good, in part because the research is operationally difficult and the period of observation very short compared with the rate of biological response and variation, are in the very areas where marine design and construction engineers need to have firm facts, and numbers to design or build to, if they are going to take biological factors into account in an economical and effective way. Such fundamental questions as the relative roles of snowfall and sunlight in slowing, prolonging or hastening algal bloom in ice-covered waters; the effects of internal waves in distributing nutrients; the breeding conditions of arctic cod and the behavioural variations of different populations or groups of the same species in the same area; the relative toxicities of degrading spilled oil under arctic marine conditions and the sensitivities of different elements of the marine ecosystems to specific concentrations of degraded oil products at different times of the year and in different ambient radiation and temperature conditions ... an almost endless list of questions, each reasonable in itself and necessary to be answered if engineering works are to take biological factors into account, has already arisen in the first decade or so of intensive field investigation and design planning for industrial development of arctic marine areas. Very few of these questions can be answered today with sufficient precision to lead to design or operational parameters in an engineering sense.

Quantitative studies on ecosystems in the arctic ocean are few, and those that have been carried out show intermittent zonal distribution with depth and position from shore or persistent leads, and marked differences in biomass and species diversity from place to place [15]. We know enough not to generalize on what we have observed.

In the absence of adequate observational data or firm answers to such specific questions, both engineers and industrial planners, as well as those setting regulations and specifications, are vulnerable to pressures and premature conclusions from those who feel that the arctic environment and its natural inhabitants are: (a) fragile and easily destroyed; (b) very slowly reacting and ecologically stable; (c) of little relative importance on the global or national scale and thus easily written off; (d) the last remnant of pristine Nature and thus especially precious and deserving of extreme care and protection, etc. All of these points of view, and more, can be found in existing regulations policies, and public opinions concerning northern development, in every northern country. In this situation it is even more important in the arctic than in temperate latitudes for those responsible for the technical planning and operations - that is the engineers and regulatory authorities - to have the best possible understanding of the state of knowledge of biological
and environmental processes in the arctic, to be aware of where our knowledge is on firm ground and where are the areas of speculation and ignorance. And this means that, in the arctic marine area, there is need for a close link between arctic environmental research and arctic development engineering.

When understanding of biological processes is poor, there is little basis for developing a common explanation of the reasons for controlling disturbances of the marine environment, or for judging the effectiveness of measures taken, and perhaps even less basis for agreed acceptance of the environmental goal toward which all claim to be striving. Such a situation favours confrontation between the regulators and the regulated, and between both of these and those who feel that regulation is inadequate. It leads to compliance with environmental regulations because it is the law, rather than because it is to the recognized long-term advantage of everyone. Compliance under these conditions often becomes very expensive, arbitrary and may not achieve the desired environmental effect. The best safeguard, of course, is adequate scientific knowledge of the biological and environmental situation.

The following are some examples and the wide range of biologically-related problems and difficulties with which the marine engineer must be prepared to deal if his operations are to be in harmony with the northern marine environment:

a) Certain parts of the sub-arctic or arctic waters are anomalously rich biologically, compared to the surrounding regions. Lancaster Sound is the well known and obvious example in Canada [27; 7] there are other "biological oases" off the coasts of Alaska and Greenland, and on the Barents shelf east of Svalbard [30]. Each of these areas is a breeding or feeding ground for migratory species that come from a long distance away or congregate from a large area; in general, they are places where the primary productivity per hectare is relatively high, resulting in a concentrated food supply which then gets dispersed as biological energy to other parts of the ocean. If the special conditions which result in the richness of any one of these places were to be removed, there is no alternative place to which the populations could go, and no other area would provide the biological energy to maintain the regional ecosystem at its present vigour. Thus, destruction of the biological functioning of these specially rich areas would mean that an ecosystem covering large parts of the northern marine world would be destroyed or irreparably damaged. These distinctive oases owe their richness to a combination of factors, unique in each case and not fully understood, which allow ocean currents, winds, or other organisms to bring nutrients from large regions and in fortunate circumstances concentrate them in a restricted area where physical and chemical conditions for breeding or feeding are suitable. We do not know how stable such combinations are, or which of the contributing factors are most critical to the maintenance of the system. In the absence of this knowledge, there is always a risk that a comparatively slight disturbance of environmental conditions - the same kind of disturbance that would have no significant effect in the surrounding biologically poor regions -, could have severe biological consequences that would be felt over a large area.

b) In seas that are usually covered by ice most or all of the year, the most critical place from a biological point of view is the shore lead. It is here that light enters the water most freely, gases are exchanged between the water and the atmosphere, and tidal pumping action, wind-induced surges, and disturbances due to ice movement are most marked to produce water mixing, nutrient distribution, photosynthesis and growth. So it is in the shore lead that marine and coastal life, large and small, is most varied and vigorous. But shore leads are, for many of the same reasons, the places where arctic marine pollution becomes concentrated - in the very place where a given amount of toxic substances will have the greatest effect on marine life. The prevention
of pollution from shore-based or near-shore installations through good
design, and the design and selection of pollution clean-up equipment and
procedures, is to a great extent in the hands of the engineer; but to do
a good job in this he has to have an adequate understanding of both the
physical dynamics and the biological characteristics of the shore leads he
is designing to protect. One shore lead is often biologically, and perhaps
chemically not at all like another. And only a rudimentary start has been
made at describing shore lead ecosystems in a systematic way and relating
them to the factors that give them their distinctive characteristics.

c) One of the areas of knowledge most important to environmental protection of
arctic marine ecosystems is that of the relative or absolute sensitivity to
pollution of species communities which make links in the marine food chain.
Because of the highly stressed nature of arctic life systems and low net
energy of the entire system, physiological adaptation of arctic forms of
life appears to be comparatively slow, but behavioural variations within a
species are surprisingly wide. The behavioural differences have apparently
developed to enable some members of the population to survive unexpected or
unusual conditions. For these reasons the susceptibility to pollution of
any individuals tested in the laboratory or in a small field experiment may
be quite different than the susceptibility of the population as a whole of
the same species. Information from southern latitudes on pollution sensi-
tivity often is not applicable to northern marine situations even if the same
species is found over the whole range of latitude, because of those behav-
ioural differences [9, 11] and because the toxicity and rate of chemical
breakdown of the pollutants themselves, especially the hydrocarbons, is
different owing to the low temperatures, reduced vapour pressures, and weaker
or absent sunlight [5], [19]. This is a very difficult area of research and
engineering, and present experience and understanding is fragmentary. And
yet the successful design and implementation of environmental protection
measures has great need of such knowledge.

d) A distinctive feature of the arctic marine ecosystem, obviously not present
in the oceans at lower latitudes, is the biological community that grows on
the under side of floating ice. In some parts of the arctic the under-ice
biota is quite rich and abundant, and serves as an important food source
for larger invertebrates and fishes. There are many gaps in our understanding
of the typical life history or behaviour of these communities, - they have
naturally been studied mostly where they are richest and most accessible -, but in some areas they have been observed to be very sensitive to seasonal
variations in ice conditions, with consequences that appear to be important
for larger animals in the ecosystem [16]. The distribution and abundance of
sub-ice communities may be a good indicator of the presence or pattern of
temperature anomalies, currents, or nutrient flows in a channel. Knowledge
of these communities, their composition, distribution, permanence, and
sensitivity to disturbance would be very useful in connection with the
design and siting of coastal and near-shore installations, and, offshore,
may be a factor in determining the choice and scheduling or shipping routes
at different times of the year.

These examples show that in the arctic, biological factors are of more
importance to the marine engineer than it might first appear, and that the variety
is such that general rules "to protect the ecosystem" have little meaning. To take
these factors profitably into account, biological information has to be used with
the best possible understanding of the biological systems to which it relates. There
should be an appreciation of the limitations of present knowledge of, particularly,
the interpopulation relationships within the ecosystem. It is important, also, not
to look at arctic marine biology in terms of the biggest and most conspicuous
elements, like whales and seals and polar bears. It was pointed out earlier that the
high-energy type of arctic humans, to which most engineers and resource developers and their activities belong, play their main roles, for good or ill, well down in the lower trophic levels of the arctic marine ecosystem, for it is there that they have the most effect on the environment and it is at these levels particularly that we need to apply our best efforts to maintain marine environmental quality and productivity.

5. THE APPLICATION OF BASIC SCIENCE TO ARCTIC MARINE ENGINEERING

5.1 Different purposes need different kinds of knowledge

Scientific knowledge about the characteristics and natural processes of arctic marine areas, as sketched above, is fundamental to engineering and applied work in three distinct fields. These fields, or purposes for which the knowledge is applied, place different kinds of demands on scientific knowledge, and are handicapped in different ways if our knowledge is incomplete or inaccurate:

i) environmental knowledge and engineering design. Knowledge of the ice, ocean, weather, etc. is essential to sound design of constructed works, for safe and economical operations. This kind of knowledge is the daily stuff of POAC, and is what most of the scientific papers at this conference are about. Work of this kind requires well-focused analysis and measurement of the components of the arctic environment, to produce quantitative data that then can be turned into physical criteria or design specifications. The practical work suffers if the basic data are not accurate or representative, or if the understanding of arctic environmental processes is faulty so that measurements don't represent what they are intended or expected to represent.

ii) environmental knowledge applied to regulations and specifications. Scientific knowledge is essential as a basis for protecting the environment and social values. Increasingly, the arctic marine engineer is required to ensure that offshore, coastal, or sea-bottom installations, or ships and their operations, will not be the cause of more than some acceptable degree of disruption of the environment either through their routine operations or as a consequence of any accident or unplanned event. A good example is the detailed list of questions, directed at engineers and planners in the Guidelines for the Environmental Impact Statement of the Beaufort Sea Hydrocarbon Production Proposal [12]. It should be noted that the "acceptable" disruption of the environment is rarely determined scientifically but is more likely to be a political or social judgement. But in order to meet these requirements, and to justify the expenses and special measures needed, it is necessary not only to have the best possible information on physical and biological characteristics of the separate components of the arctic marine environment, but to have adequate information on the inter-relationships between the physical environment and living systems, and on the relationship of both of these to the human use of renewable resources. This is a different kind of information and knowledge from that needed for most technical design. Such knowledge must be composite and synthetic rather than analytical, and its usefulness is critically dependent on the understanding of rates of physical change and the time factors involved in arctic biological effects and responses. The design engineer or operational planner can hardly expect to be expert in all these areas, so he is dependent of the adequacy of basic scientific studies and very vulnerable to inadequacies of scientific interpretation.

iii) environmental knowledge, policy, and administration. Scientific knowledge plays a role in the administration and control of northern development. To an ever increasing extent, engineering works and operations in the arctic marine area are subject to regulations and approvals which are technical in nature but which are themselves not based on experience or proven
practice but are, at best, experimental or cautious judgements on what ought to be safe or publicly defensible. Sometimes, the absence of adequate knowledge, or ignoring what knowledge and understanding there is, has been a cause of our collective difficulties in achieving workable northern development policy or regulations.

The standards and criteria in these regulatory and administrative procedures should, like the engineering design specifications, be based on the best available data; but because in most cases the observations are sparse and the time over which they have been taken is short at best, the translation of such data as exist into guidelines and regulations is dependent on the accuracy and completeness of scientific understanding of arctic environmental processes. At the present state of knowledge, successful and practical administrative and control procedures require both a knowledge of the extent and accuracy of quantitative information about arctic phenomena, and the broadest possible understanding of regional variations, ranges in natural behaviour, and the evolution of the arctic environment. The problem is especially difficult for policy-makers because knowledge is changing rapidly, yet regulations, to be practical, have to be consistent and if not inflexible, at least firm enough to be enforceable over a workable period. It requires an especially good grasp of the state of scientific understanding and the directions of progress of science to develop, in the absence of good data, regulations and control procedures which can serve their purpose while information and understanding increase and change.

The various studies and tests reported at this POAC conference, and its predecessors, are evidence of the steady and progressive increase in data, observation, and results of short-term experimentation on arctic marine conditions and processes. But many of the studies will also draw attention to how little is really understood of the basic processes of the arctic environment and what causes their variations. This situation demands a great deal of personal judgement from the engineer, as well as the application of proven engineering methods and principles to know data and facts.

The following are some areas where, it seems to me, arctic marine engineers have problems rather more difficult and of a different nature than those encountered in similar work in other parts of the world, in fitting their engineering work and results into the larger picture of knowledge and decision. The problem areas I will choose are the way that mathematical models and quantitative data analyses aid understanding of the arctic environment; the use of traditional and social knowledge in engineering; and the need for subjective or qualitative interpretation of technical or engineering results. Each of these requires the engineer, whether he wishes to or not, to step out of his traditional role of being as objective and impersonal as possible; and each requires an appreciation of the state of basic science in fields that are not directly related to traditional engineering.

5.2 The Modelling of Arctic Marine Conditions

An essential part of engineering and scientific work dealing with arctic marine problems is the use of mathematical models to organize data, test known or possible relationships, and clarify the logical connections between phenomena that are only partly understood. In many ways, the advance in our practical understanding of arctic conditions, and our ability to turn that understanding into useful structures of steel and concrete, is directly related to our ability to design realistic mathematical simulations of arctic environmental processes, and to reduce observed arctic phenomena to mathematical parameters. And yet there is a danger in this situation against which we have to be on guard, to avoid making very costly mistakes. Where data are scarce and it is not known how representative they are in area or time, and where physical or physical-biological processes are poorly understood, there is a great temptation to build a model by finding the simplest mathe-
matical relationship between the few available measurements, and to carry on as if that contrived mathematical relationship was somehow a representation of the real relationship in Nature. When this kind of a model is used as a basis for design concepts or cost estimates, it becomes a first-class tool for compounding and magnifying our areas of ignorance. This temptation to design a simplified model based on as few data as possible and to base decisions on the apparent relationship as shown by such a contrived model is an important cause of all-too-common cost over-runs in arctic development schemes. The terrestrial pipeline business in the arctic, in several countries, provides some sobering lessons which marine engineers and modellers could heed.

If one looks critically at the reports of several of the modelling efforts being presented at this conference, I think one will have to conclude that we are in danger of trying to use models to simplify nature, rather than to use them to help us understand her complexities. We must beware of betting our money, and ultimately human lives and the safety of the environment, on simplified constructions that may bear little resemblance to the real world.

It is useful to keep in mind that at this stage of our basic understanding of arctic conditions and their variations, the most useful modelling efforts, undertaken as a way of organizing our own thinking and knowledge, fall quite neatly into two distinct categories.

1) There are the models that have a recognized end in view — say prediction of sea ice pressure, or the amplitude and frequency of internal waves at the interface of two water masses —, and with whatever real-life observations are available we attempt to make some empirical sense out of the numbers and get a working relationship which then can be run forward, on the assumption that overall conditions won't change very much, to see what will happen next. The aim in developing such models is usually to reduce to a minimum the number of observations, or the length of time of observation necessary, to come up with a reasonably predictive result. Several examples of such models are being discussed at this conference.

These kinds of models can be very useful, in a given setting or narrow range of conditions, for determining the scales and spacing of future observations, for illustrating the expected sequence of physical and chemical changes, or for setting design parameters for apparently normal conditions. A good example is provided by several useful models of oil spill decay processes [18]. But such models may be quite misleading if the original data were not causally connected, or if conditions change markedly because of influences not covered by the model. And such models may not help us to increase our understanding of what is really happening; the user of such a model is in danger of becoming like the driver of a car who learns that to make the car go forward he puts the gearshift lever in one place, and to make it go backward he puts it in another. No matter how much he practices with the gearshift lever, he is not likely to learn much more about how the car works or how to fix it when the lever comes off in his hands.

Clearly, the more that is known about the real causative relationships in environmental processes — that is, the better is our scientific understanding —, the more realistic such empirical models will be; but the use of such models does not, in general, contribute directly to scientific knowledge, except to document averages or by their failure, to help us to note the occurrence of extremes or examples when simple relationships do not hold.
ii) The other kind of models belong to the group that are often called causal models, in distinction to empirical or operational models. These are models designed to explore and test relationships and interactions between different parts of environmental processes as an aid to understanding—in the examples above, causal models would not attempt to find an approximate prediction of ice pressures from as few wind observations as possible, but would try to take into account all the measurable factors of surface roughness, wind drag, turbulence, momentum, internal friction, etc. to find some coefficients of the coupling between a moving air mass and sea ice; or they would explore the spatial relationships between thermal and density gradients that lead to propagation of internal waves in a moving water mass. Such models are, of course, basic science, and are an essential part of attempts to understand the way that Nature operates and the causes of those phenomena we know as Arctic conditions; but they are not often of much immediate use to the engineer or regulator who wants to obtain workable predictions or design numbers from the available data. Attempts to use causal models as a basis for prediction without realizing their essential character are a cause for some of the frustration felt by engineers and managers when they attempt to use the results of growing scientific knowledge, and lie behind some of the disrespect sometimes felt in both directions between the pure research scientist and the practical engineer.

Both empirical and causal models can be developed, and be useful, at different levels of simplification and sophistication, and apply to a wide range of scales in space and time. Their purposes are different, but they are related. The World Climate Program has put the relationship between them in a succinct way:

"There is, or should be, no conflict between empirical and causal approaches in the sense that a correct empirical relation cannot be contradicted by the result of a correct causal model; there is, however, a definitive hierarchical relationship between these: the empirical model mostly states the observed facts, while a causal model attempts to explain why they take on the observed states and forms."[33]

Comprehensive causal models are the only defensible technical way we have of organizing a large amount of data on related but not clearly understood phenomena and interpreting them in a logical manner. In the Arctic, where data are fragmentary and the period of recorded numerical observations very short, such models are essential to the development of comprehensive understanding of environmental conditions. One of their great values is that they can show convincingly that we just don't know enough to make predictions. The general movements of both the atmosphere and the oceans and their thermal characteristics, for example, in the recent past are fairly well known from observations taken over the last century or so. And thanks to the international collaboration work of the Global Atmospheric Research Program (GARP) and the Scientific Committee on Oceanographic Research (SCOR) and related bodies, impressive progress is being made in developing mathematical models of atmospheric circulation and of ocean movements and heat transfer that take into account nearly all of the things we can measure about these movements [13; 1].

Such models are intended to test, by mathematical simulation, what would happen in the future if a given set of conditions were to exist and if our understanding of the relationships between those conditions, as expressed in the model, were to be the real ones. They are not predictions of the future. By letting the model "run" and noting when and how it begins to show an unreasonable or physically impossible result, the model helps to identify in which set of relationships the approximation or simulation was in error, and to correct it to something a little closer to the way Nature works. The development of such complex models has only been made possible by the availability of advanced high-capacity computers; yet for all
its sophistication, mathematical modelling is underneath really just a careful cut-and-try process.

This is an area where exciting progress is being made; each year the collective skill in comprehensive modelling improves, and we are getting closer to realistic simulation of atmospheric movement and of ocean dynamics, limited mainly by the capacity of computers and the money to operate them. But no group of researchers has yet developed a workable model that integrates or couples the atmospheric and oceanic circulation in a realistic way for arctic regions; - the complexity of this is beyond the capacity of present research computers to handle, and perhaps beyond our knowledge of the physics and thermodynamics involved to reduce the relationships to mathematical logic [3]. So, in the very area where designers and planners would benefit most from quantitative relationships and reasonable predictions that will set boundaries to expected variations in weather, sea ice, and ocean behaviour, the recent advances in research and modelling are not much help.

Judicious use of mathematical modelling of environmental processes is essential to good arctic marine engineering, and will increasingly be so in the future. But the successful use of model results requires skillful subjective judgement and a knowledge on the part of the engineer of the science of modelling and its limitations. We should keep in mind that although we may dress up the estimates with numbers to make them look like data or conclusions, our general understanding of the overall relationships between arctic weather and arctic ocean behaviour is not demonstrably better than that of the Inuit who have a "feel" for it, based on long and intimate observation. This is not an admission of failure; rather, it is a reminder that others already know a great deal.

5.3 Arctic engineering and the use of historical and social sciences

It is a truism to say that one of the most under-utilized sources of information in many vital areas of arctic development is the accumulated practical knowledge that the Inuit posses about the arctic environment. Every engineer or scientist who has spent a long time in the arctic together with native peoples is impressed by the depth of the apparently innate understanding possessed by many native people about things concerning the natural environment which people from other cultures have to learn through formal study. It is significant that this is not a superficial impression gained by visitors who come to a strange land for the first time; southerners who have worked or studied in the arctic for many years and know it well are those most conscious of the soundness and depth of traditional knowledge. Yet it is extraordinarily difficult to tap this knowledge and use it in a modern context. As the Inuit themselves explain, the essence of accumulated understanding of the environment is very difficult to put into words or numbers; and direct questioning, which is the habitual way for southerners to seek information, often evokes only tales of folklore or trivia. Thus it is easy to conclude that traditional native knowledge is not relevant to modern industrial or engineering problems. But the knowledge is there, practical, hard-won, and well tested.

Those who could benefit best from such knowledge should in their own interest find better ways of obtaining it and using it. If there exists sophisticated practical information that could be useful to us in our own sophisticated work, we should be able to develop better ways of getting it than having to live with the experts for twenty-five years, or by hiring somebody as a snowplow driver and expecting him somehow magically to pass on to the officers of the company all his engineering knowledge about snow.
The people from our Western technical culture who have made it their specialty to investigate the knowledge of northern people are the social and historical anthropologists and ethnologists. But - and I mean no disrespect when I say this - most of the social scientists who study native customs, language and traditions seem to have very little interest in the fundamental base of understanding of Nature upon which practical day-to-day knowledge is built, and even less interest in the practical need or our own culture for useful knowledge about the same region. There is very little systematic effort to transfer useful information between the cultures at the level of technical sophistication at which both possess it.

Most of those who study northern cultures seem to know or care little about modern engineering and its knowledge requirements, and have not apparently seen themselves as a link between the knowledge of their natural world already possessed by the indigenous cultures and the knowledge of the same world which is being obtained at great effort and expense by modern developers, engineers, and natural scientists. Although there have been some reviews of the growth in knowledge base that must have accompanied the historical adaptation and evolution of successive arctic cultures, based on archaeological evidence, [22], students of modern northern indigenous cultures seem in general to be more interested in historical analyses, recording of folk tales, and identification of societal structures and habits which are in danger of dying out, than they are in identifying and translating the distinctive sophisticated knowledge of some aspects of environmental phenomena possessed by these cultures which could be useful and practical today in the modern context. Yet this kind of knowledge, if it is applicable to today's problems, could serve not as a reflection of the past but as an example of the present vigour and modern relevance of the old culture. Some of the folk tales do provide the engineer with insights that are interesting in a technical sense, but they are not much help in solving a modern design problem. I recall for example the traditional story of how the seal got his short legs because of the rapid growth of frazil ice in a lead when the turbulence was increased by his swimming [25].

But we need more discriminating information and help from the social scientists than we can get from anecdotes and legends. Students of Inuktitut and Greenlandic seem to be fascinated, for example, by the fact that people who live in the north use a dozen different words for snow - snow that falls and won't blow away, snow that will support a running caribou, snow that is best for making igloos, etc. Well, modern glaciologists have lots of terms too - firn, rime, depth hoar, etc. - and if we add $S_1$, $S_2$, etc. I suspect that we could give interviewers from another culture an equally imposing list. But none of that is very useful in itself. What seems to me to be significant is that whereas our own terminology, in the European languages, has tended to classify snow and ice types in an analytical way, according to physical structure or place of occurrence, the long evolved Inuit approach has been synthetic rather than analytic. The Inuit classification of snow recognizes the end product as a result of a number of dynamic environmental processes not perhaps apparent from the snow itself but inherent in the term used. This is a much more dynamic, holistic way of looking at arctic conditions than we are used to as engineers, and it may have a lot to teach us. However, we are not likely to be able to learn this way of looking at the environment without help from the experts, and without much more knowledgeable and technically sophisticated translators. So perhaps, if we want more useful technical information from the social scientists, we need to give them a little technical help too.

It is strange indeed, in a part of the world where the environment presents such distinctive engineering challenges, and where great efforts are being made and large sums are being spent to learn how to meet those challenges, and where at the same time social scientists and historical anthropologists find such a fruitful field for research in studying the ways that a people have learned to live and prosper under those same environmental conditions, that there has been so little
contact or collaboration between arctic engineers and arctic social or historical scientists. It would seem to be a wise and prudent move for any engineer or firm dealing with sea ice and arctic ocean-weather relationships to develop the best possible communication with social scientists and ensure that they, and the native people, understand in a candid way our need for information and have a good appreciation of what would be useful to our own technical work. It could very well be a good investment to obtain, in a deliberate and sensitive fashion, as much knowledge as possible of the practical understanding of the Inuit, Greenlander, and Alaskan Eskimo people in these areas. Such information may well give us new insights into the information we are presently obtaining from field measurements, remote sensing, and mathematical models.

5.4 Value judgements

Engineers and physical scientists are, by training and inclination, concerned with facts, objective measurements, predictions of inanimate or definable conditions and consequences. It is a contradiction of traditional good engineering or careful science to mix emotion, intangible values, or a sense of "what ought to be" into our work. Others can take our designs and our measurements and place a social value on them; but then the value judgement is theirs, not that of the engineer. In this attitude and practice lies the integrity of the engineering profession and the credibility of the scientific method.

In the arctic today, as a large number of the engineers and scientists at this conference can attest, things are not quite working in that objective way. To a degree that is distressing to many, engineers and scientists are required to step out of their objective and impersonal role and provide subjective and qualitative interpretations of the results and consequences of their own work. Just as, two generations ago in many parts of Canada, the small-town country doctor was looked upon and respected as a source of wisdom and opinion on all manner of things that went far beyond his professional medical training, simply because there was no one else available with the same degree of knowledge and breadth of view, so today in the arctic the engineer or scientist who has studied some arctic phenomena finds himself expected to evaluate the social significance of his findings and give advice or judgement on matters extending much beyond his own technical work. The environmental impact hearings on any arctic projects, or the range of questions posed when company representatives hold community meetings to explain their projects, give abundant examples of this. The need to evaluate, and assess the implications according to societal criteria, rather than to measure, report or predict in economic or physical terms has in the field of arctic development moved right down into the level of technical operations.

No longer can a company explain its presence or defends its resource development operations in the arctic by pointing out that what it is doing is perfectly legal; it is under increasing pressure to examine not only whether its activities will be legal and profitable, but whether they will work to the net social and environmental benefit of the region and further the policy goals of the country or territory. Less and less can a scientific or engineering employee of either a company or a government agency plead that he is just doing his job, and that his responsibility is limited to technical or scientific work. He is expected by both his employer and by those outside to place a value judgement on his own work and on the project to which he is contributing. However much he may be inclined to plead "please don't shoot the messenger", in northern development today it is often the messenger who first gets shot at.

The need to make value judgements on the implications or consequences of project development or engineering works places special demands on the ability of engineer to use the knowledge of basic science in interpreting the expected or
actual results, or variations in results, of engineering designs and operations. An appreciation of areas of ignorance as well as an awareness of what is understood about arctic processes has much to do with the credibility and reliability of evaluations made by technical people of the social implications, as well as the technical soundness and representativeness of the technical work itself.

Have you noticed how often the weakest part of our technical work in the arctic is the extrapolation from controlled data or simplified tests to the real world, or in the accounting for natural variations in time and space?

- How often we spend a tremendous amount of time and effort on careful measurement or experiment to get an explainable result that appears to be quite precise; then throw away that precision and care by having to make very subjective assumptions in order to apply our conclusions to the actual situation?

- How common it is for us to add enormous safety factors, which are often nothing more than guesses, to account for the fact that Nature is not tidy, that the environmental conditions in one season are often quite different from those in the same season of another year, that ice never breaks the same way twice except in the laboratory, or that the original data were taken from a place that turned out not to be representative of the area? Such safety factors are hardly good engineering, for good engineering should, at the least, reduce the guesswork in technical undertakings.

When this problem becomes acute, - and it is acute when we become so accustomed to it that we tend to look on this situation as a normal way of doing things in the arctic -, it is likely not to be our engineering methods or our data which are at fault, but our science.

It seems to me that we have the problem of extrapolating from controlled data to the real world, and of accounting for natural variations, very acutely in arctic development and arctic marine engineering today. We are developing a habit of calculating our details of design or operations to a couple of places of decimals, then adding a safety of factor of five for good luck to cover local variations or unexplained effects. And we stubbornly resist building accommodation for change into our work. For example, even though from both observational data and personal experience we all know that the arctic short-term climate is changing in important ways, because the scientists cannot agree on exactly which parameters are changing by how much, engineers and project planners seem to persist in planning as if it were not going to change at all.

These habits into which we are falling are just not good enough, in view of the magnitude of our arctic projects and the enormous responsibility on engineers and technical people to ensure that development of resource and transportation systems improves and does not destroy or adversely change the arctic natural, social and economic environment for years to come. There is no single solution to these bad habits. But the problem will not be removed merely by the acquisition of more data or more careful and thorough engineering in the traditional sense (though that would always help). The solution lies very importantly, I think, in the development of better basic science, better real understanding of what is going on in Nature, physically, chemically, and biologically, and in developing the ability to evaluate - that is, make value judgements on - the relationship between that understanding and the social and environmental consequences of the project or operation.

When good engineering becomes handicapped by lack of good science, the engineers have a responsibility to see that science is improved, so that they can get with their job.
6. KEEPING SCIENCE AND ENGINEERING IN BALANCE IN THE NORTHERN MARINE FIELD

What has happened, to allow the progress of engineering and technological development to get out of balance with scientific knowledge in the arctic?

There are many factors, and of course they are not confined to the arctic alone; but let me mention a few that seem to be especially important in the arctic today:

6.1 Urgency

Resource development opportunities, northern development policies in most polar countries, energy problems and their geopolitical ramifications, the rapidly rising cost of money — all put tremendous pressure on solving northern resource development problems now. This puts the emphasis on applied activities, on finding practical solutions with the knowledge on hand, and less importance is thereby given to finding out what lies behind those problems. Thus, "practical engineering" thinking, as well as policy and financial support for engineering activities, gets a boost and stimulus from the sense of urgency and politics in the arctic, while pure science does not.

Good scientists can work under pressure just as well as good engineers — even ivory towers may be most productive if they are on fire, as our wartime science shows. But significant scientific progress cannot be produced to a deadline or a cost-benefit schedule; if scientific research is programmed in this manner, it usually results in pretty trivial science. Science is mostly a deliberate and pains-taking activity, in which careful following of clues in many directions results in occasional surges of knowledge that push ahead of all old ideas and put everyone on a new level of understanding; but neither the surge nor the pay-off can be planned in advance or put on a PERT chart. So people in a hurry are not likely to support a continuing science program.

6.2 Problem-by-problem approach

Our arctic activities and problems have almost all come about as responses to opportunities in single areas of interest where some established group is already equipped to take action. Think of some of the recent drives that have shaped arctic engineering:

- Petroleum potential is recognized: - the oil companies go after it;
- There is a perceived need for arctic transport: - the shipping companies and Ministry of Transport look into it;
- The military needs communications in the arctic: - and arctic electronics and magnetospherics becomes popular.

Each of these groups is bound to approach its work in the arctic with the tools and knowledge it already has, and to look on arctic research and development as a process of adapting what it already knows how to do, to the peculiarities of the arctic environment. This process — and we see examples of it in the papers submitted to this conference — tends to make our technical activities more and more narrow and specialized. Only the universities, today, try to be free to take a broad or detached view of arctic questions; and they are progressively becoming excluded from the arctic scientific scene.

But the arctic environment is a great integrator and synthesizer, even more than the environment in more temperate regions, where the greater diversity and complexity allows relatively more independence of action. A characteristic feature of northern development, already noted, is the degree to which projects tend to have ramifications and influences outside the areas or interests that are their
prime concern. One cannot break ice in a shipping lane without affecting the biology of the area, or develop an arctic harbour without affecting sea ice behaviour and perhaps the local weather and primary marine productivity, etc.--the list of important but often unwanted side effects is as long as any list of projects. As has been aptly said [14], all of us with activities in the north, no matter how narrow our respective terms of reference or how limited our intentions, are, whether we wish to be or not, in the business of regional development.

Engineering, as a whole, thrives on specialization, because specialization helps us to define problems we can get on with and to set the conditions for specific tangible results on which action can be taken. But science, by and large, does not lead to specific results. It rarely leads to really sound science if the expected answers are focussed in advance on a pre-defined problem. Scientific study is better suited to trying to understand what makes the problem, than it is to solving a problem with knowledge we already have. Thus, once again, the current emphasis on "problem-solving" in the north does not favour the pursuit of good basic science.

6.3 The amount of money at stake

Northern development decisions in this decade are big-money decisions, and investments in the arctic are what the trade likes to call "world scale" investments. I read somewhere that one-third of the countries belonging to the United Nations have national domestic budgets smaller than the peak budgets of each of the five largest arctic industrial proposals in North America today.

Arctic development is often portrayed, by its proponents and by the press, as a risky business, a gamble, and adventure. In imagination, this is true; in money terms, it is not. With the large amount of money at stake, no set of managers, political or industrial, is going to take one more chance with the investment than absolutely necessary. There may be bad judgements, which sometimes get passed off to the public as the consequences of taking a gamble and losing; but the past record shows that the financial management of major developments in the arctic is even more conservative and cautious than most of that in the south--just because the sums are so large and there are so many unknowns. I am pretty sure that the future record will show this too. High rollers just don't survive in high latitudes.

Given this situation, there is bound to be an emphasis on the tried-and-true methods for doing things, even if such methods have never been tried in the arctic; and a tendency toward making decisions based on the knowledge we already have. The natural inclination is for research and development efforts to be applied to adapting known techniques to new situations, not to developing new techniques; to getting more and better measurements of the kind we are familiar with, toward developing adaptive gadgetry rather than seeking understanding. This often seems the safest way to use available money, in the short term. In an accounting sense it appears to be the best way to protect the investment when faced with many unknowns, for although it may lead to very large research expenditures, the general kind of results can be predicted to some extent and thus justified when the work is completed. Science, on the other hand, is always a risky investment, even if it does not involve much money, by northern development standards. Much of the effort put into science does not seem to pay off at all, and when there is a direct usable result it may be in an unpredictable direction, perhaps benefitting the competitor as much as the sponsor, or bringing to light unintended side effects whose net costs may be greater than the expected net benefits from development. This is hardly the kind of result that endears scientists to careful managers.

Is it any wonder that our major thrusts in northern development have not stimulated basic science or supported the search for better understanding of arctic processes and phenomena? Is it any wonder that the kind of people who make billion-dollar decisions or who set major regional or resource policies ignore the support
of science or are impatient with it because scientists cannot give unequivocal answers to such apparently (to managers) simple and logical questions as, "What is the climate going to be like in 20 years?" or "How much spilled oil would it take to spoil the seal hunting?"

How can one expect sympathetic support from a cautious business manager for a study of freezing processes and thermal migration in soils when the experts hedge their reply to a straightforward question like "Will permafrost build up under an artificial island?" by saying ... "It all depends ..."? It is understandable that the manager will go instead to the engineer, who will run some tests, take some measurements, extrapolate the data obtained according to some general published relationships which have been determined elsewhere, and give at least an understandable and useable answer.

The trouble is, with no discredit or disrespect to any of us, that because our sources of new basic knowledge are drying up, and because through the success of our own engineering work so far we are asking more and more sophisticated questions of the scientists in areas where fundamental knowledge is not good, the engineering answer, honest and best available though it may be, is in increasing danger of being quite wrong or irrelevant.

6.4 Nuturing basic science for practical ends

The tree of knowledge can be likened to an apple tree. What the public wants are the apples - the useable product. That's why we value and keep the tree. The engineers and applied scientists, together with a few economists and others, can be looked on as being responsible for the tree itself, for the shape of the trunk and the branches, for the leaves and the photosynthesis reactions that provide the energy to keep the organism functioning, etc. All of these are essential, are visible, and it is possible most of the time to see what role they play. It is possible for the policy makers to work with these applied scientists to direct and help arrange the results, as a farmer does by pruning branches here and there, or controlling insects to get a better and more dependable product. But the health and continued productivity of the tree depends on its roots, which can be compared with the basic science, working somewhere beneath it all in ways that may not look as if they could lead to apples at all - interactions between bacteria and earthworms, osmotic transfers between organic fluids and dissolving mineral salts, the fundamental processes of life. Which of these behind-the-scenes processes makes which part of which apple is a pointless question, but it cannot be denied that one cannot have a good apple crop for very long unless the roots of the tree are healthy.

I think that we have plenty of evidence that with regard to arctic development and technology, our scientific roots are not healthy. Our basic science roots are in danger of withering, because our attention, and the decision-making and investment apparatus that controls the whole enterprise, is focussed on the visible part, like the tree above ground. A wise farmer, of course, looks after the roots along with the rest of the tree, sees that they get water and gives them a shot of fertilizer now and then if they need it, not because he cares much about the roots but because he wants, in the long run, to keep on picking apples off the tree. But who plays the role of wise farmer in arctic development?

7. THE SCIENTIFIC CHALLENGE - ARE WE GOINT TO BE ABLE TO MEET IT?

I have painted a rather gloomy picture of the state of basic science in the north, especially in marine fields, right at the time when we are busy telling ourselves what real advances are being made and what exciting challenges and opportunities lie ahead. There is no doubt about the technical advances being made. There is no doubt about the reality of the scientific challenges, or about the genuine thrill and opportunity of being part of a fast-moving development, encom-
passing a wide range of scientific and engineering disciplines, that is tremendously relevant to this country's - and other countries' - policies and well-being. And, what is rare these days, there is a lot of money being spent on arctic technical research work. Indeed, judged by the kinds of problems that have preoccupied scientists and science managers throughout the world for the past couple of decades, the situation with regard to arctic science must seem at first glance to be rosy.

But I think that there are good reasons for us to take a hard look at whether we are indeed capable of meeting the challenges ahead. I am not going to discuss priorities for research, or funding, or who should be doing the work. Let me leave with you a few worries that seem to me to be more fundamental:

a) To an increasing degree, even the largest projects, whether government or industry-sponsored, are forced to plan their science on a step-by-step basis, tied to approval processes and timetables and short-term arbitrary funding decisions. Integrated or balanced scientific programs, long-term or designed and approved for the life of the project, are not found in the arctic today;

b) government finds themselves unable or unwilling to state their long-term priorities and plans, even though the decisions they make today open some options and foreclose others for many years ahead. There is an embarrasing and growing lack of connection between the decisions or policies for northern development or resource management and the support of the scientific work and acquisition of new knowledge made more necessary and urgent as a result of those decisions. Even in the most directly practical areas such as resource assessment surveys or the acquisition of base-line environmental or social data where the need for better knowledge is widely recognized as essential to the carrying out of announced policies or the review and approval of the resulting projects, funds for long-term research or study have not accompanied development decisions and policies. In-hose scientific activities whose worth has been proven for many years have now, regardless of their record, to justify their existance from one year to the next;

c) the principal source of new concepts in science (as distinct from new or additional information) and of new scientific blood has to be the universities; yet the universities have benefitted hardly at all from the recent surge of activity and investment in arctic marine resource exploration. Some university departments have admittedly become increasingly busy with contract work and professional consulting; but many of these have suffered intellectually as they have become scientific services or advisers on problems, with professors and faculties drawing on their accumulated expertise from past researches to carry out short-term investigations on immediate problems. Such a situation leaves the university with little time, incentive, or resources for leading arctic science into new fields;

d) the supply of top-quality scientific and engineering persons in the fields most needed in the arctic, and particularly persons motivated to undertake arctic work for a long enough period to be really useful there, is in danger of becoming woefully inadequate. Most of us at this meeting, will I should think, agree that the peaking of scientific and technical activity in arctic and northern offshore areas will come maybe ten years from now. If the industry or the country wishes to have really bright and qualified productive arctic scientists or engineers, with say five years' on-the-job experience so that they can individually assume important professional responsibility based on their expertise in the arctic ten years from now, those persons should at least be entering university this year. Where are those persons? What are we doing, from government, industry, or professions,
to show the kids in high school that this is the life for them? What are we doing to make it possible for the universities to give future scientists and engineers the best possible start, in the way of up-to-date stimulation and facilities to learn, if bright students do show such an interest? Have we given attention to the problem of keeping the best of these students keen, and participating, through the long practical as well as academic apprenticeship process that arctic scientists still have to go through? Do the universities have the capacity and resources, and most importantly the freshness and verve to make sure that the best students are encouraged to look scientifically forward and create their own roles, and not only be programmed toward capitalizing on the immediate employment market which responds to the demand for application of yesterday's knowledge?

Surely, as engineers, we know better than to try to fix the water supply by just fiddling with the outlet end of the pipe. Surely our profession knows better than to expect to increase the supply by applying more suction at the delivery end, when there is hardly anything in the pipe. Yet that, it seems to me, is what we are starting to do as the needs increase for trained scientific manpower in the arctic.

8. WHAT CAN ENGINEERS DO TO IMPROVE THE STATE OF SCIENCE IN THE NORTH?

If, as I have tried to point out, continued success and progress of arctic engineering and industrial projects is vitally dependent on the continued progress of basic science in the north, and if that science is not in a healthy state today, what can the engineers, individually and as a group, do about it? I think engineers can do quite a lot, because engineers as a group are in a position of power in northern development and are likely to remain so for some time. Here are some obvious suggestions:

First: Awareness of science. Arctic engineers in all fields can become more consciously knowledgeable about scientific study and the state of knowledge about the north, can become more aware of its connection to engineering work, and learn to use more effectively and critically what data and knowledge there are. It is proper for engineers to put the scientists on the spot: we can challenge them with engineering dilemmas and areas of ignorance that we recognize, and make it clear that we expect a vigorous scientific activity to be a natural complement to increasing technical industrial and government activity. Such scientific activity would not be to feed the engineers with pre-digested technical information, but to ensure continued engineering progress built on the ideas and concepts that can only come from basic research.

The engineering profession can strive to be as informed as possible in its criticisms of the shortcomings of scientific knowledge and show that it understands the different purposes and methods of approach between pure research and the applied sciences and technology. Engineers can try to make sure that not only scientists, but funding agencies, are aware of how much the sound practice of engineering is built upon on a continuously improving basis of advancing scientific knowledge.

Second: Demand support for research. Engineers, as the main direct customers and users of new scientific knowledge about northern resources or environment, can press for increased investment and support of continuing research in areas critical to arctic development. This should be demanded by engineers as an integral part of good technical planning. The engineers can have an important influence in this area, for they are the expert on what they need to do their job, and it is their job, not the results of scientists, that both governments and corporations want to see accomplished. As engineers, we should be able to make a strong case that we need an adequate underlying science program just as we can demand adequate meteorological data or insist on the necessity for improved computing facilities, ... with the
differences that in the arctic context, science generally costs less per year than most of the other "important services", but it must be carried on and supported for a long time if it is to be really worthwhile. Science is also different in that it usually benefits many others besides those who pay for it.

Third: Campaign for recruitment and education of new scientists and engineers. Engineers can make loud noises in the right places, about the supply of future scientists and engineers, and about the place of universities in arctic development through their dual roles of (i) advancing and integrating arctic knowledge, and (ii) the training and stimulation of future scientists and engineers. The engineering profession can take the lead in directing attention to the inlet end of the scientist supply pipe, instead of, as now, expecting the market to apply suction to the outlet.

The time lag between becoming individually interested in subjects that can lead to a career or profession in arctic science and engineering, and becoming effective arctic scientists and engineers is so long - 10 to 15 years and there seems no practical way of shortening it -, that the problem of developing a first-class cadre of arctic professionals cannot be expected to hold much interest for governments or even companies who are making long-term investments. But engineers, as a group, are accustomed to working with much longer times between decision and final results. They are in a particularly good position to make a strong case for far-sighted decisions and to influence long-term policies in this vital area.

It has been said that war is much too important and activity to be left entirely to the generals. Similarly, basic science is much too important to northern development to be left to the efforts of the scientists alone. It is in the shelf-interest of engineers, as well as in the interests of society, the welfare and justice of our respective nations and the various cultures within them, and in the interests of the quality of the environment of the future, for all to ensure that science will be able to play its vital and proper role in the arctic development of tomorrow and the future.

9. REFERENCES


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ABSTRACT

Arctic offshore structures must be designed to withstand moving ice ridges, either unconsolidated or consolidated. The ridges are most likely to fail in crushing, shearing, or in-plane bending against cylindrical and vertical flat-sided structures. The ridge failure pattern may include out-of-plane bending against conical and inclined flat-sided structures.

In this paper, several ridge failure patterns are reviewed and plasticity theory is used to calculate upper bounds of pressures resulting from ridge crushing and shearing. The Mohr-Coulomb failure criterion, considered applicable for unconsolidated ridges, and a quadratic failure criterion, considered applicable for consolidated ridges, are used in the analysis. The upper bounds are presented in a dimensionless form for typical ridge cross-section shapes, as functions of normalized ice strengths and ridge and structure dimensions.

INTRODUCTION

Sheet ice, rubble fields, and pressure ridges are typical conditions of moving sea ice interacting with structures. Geometric description, mechanical properties, and failure patterns, needed for analytical estimates of ice forces, are known better for sheet ice than for pressure ridges.

The largest ice forces measured on cylindrical structures in Cook Inlet were generated by pressure ridges. The reported peak loads were several times larger than sheet ice loads (Blenkarn [1]).

Pressure ridges are significant elongated accumulations of broken ice caused by buffeting.
action between adjacent ice floes. A newly formed ridge consists mainly of unconsolidated ice rubble; a first-year ridge often contains a solid ice zone; and a multiyear ridge is generally consolidated throughout most of its thickness. The consolidation significantly increases ridge strength. Major ridge dimensions are: keel depth, sail height, width, and length. Sail height to keel depth ratios occur in a wide range. In addition to ridge dimensions and strength, ridge orientation, spacing, and clustering are important for prediction of ice forces on structures.

It is easier analytically to bound, rather than predict, ice forces; substantially less information is needed to establish a bound than to describe the actual process. A number of analytical bounds for ice forces have been presented to date: several for sheet ice forces (for example, Ralston [2], Reinicke and Remer [3], and Prodanovic [4]) and rubble forces (Prodanovic [5]), but none for ridge forces.

In this paper, plasticity upper bounds for ridge forces on vertical (cylindrical or flat-sided) structures are presented. The bounds relate to both unconsolidated and consolidated pressure ridges embedded in a uniformly thick ice sheet. The analyses are based on idealized ridge shapes, strength properties, and failure modes. The considered ridge geometries are idealized from limited field observations reported to date (for example, Weeks et al [6], Kovacs [7], Kovacs et al [8], and Wright et al [9]). For the presented examples, the strength of consolidated ridges is based on laboratory-grown freshwater ice strength measurements; the strength of unconsolidated ridges is based on model-scale rubble strength property measurements (Prodanovic [5]). Nevertheless, the analyses could easily be adapted for other ridge geometries and strength data.

It is assumed that sheet ice, multiyear ice, and rubble behave as elastic-perfectly-plastic materials, described by the corresponding yield functions, and that the "associated" flow rule relates current plastic strain rates to current stresses. Conservative estimates of the maximum ice loads are obtained by constructing admissible velocity fields and applying the upper bound theorem of plasticity theory.

CONSOLIDATED RIDGES

Ridge Profile

Geometry of a consolidated ridge, that is, a pressure ridge that has survived at least one melting season, is idealized by a simplified ridge profile shown in Fig. 1, proposed by Kovacs [8]. The sail height, S, is the distance between waterline and sail crest; the keel depth, K, is the distance between the waterline and the flat-bottomed keel; and the keel width, $B_K$. 
Fig. 1 Consolidated Ridge Shearing Mechanism
is the distance between the intercepts of the keel slope projections with the sea level. The ridge profile proportions are defined by $K = 3.3S$, $B_K = 5.5K$, and the sail and keel slopes of $20^\circ$ and $30^\circ$, respectively.

The keel depth is selected as the independent parameter of the ridge profile since the keel depth of a non-grounded ridge at a particular offshore location is limited by the water depth at that location. The thickness of the ice sheet, $T$, is another independent parameter in the analysis.

**Ice Failure Criteria**

The following anisotropic pressure-sensitive yield function is used to describe the strength of the sheet ice in which a ridge is embedded:

$$f(\sigma) = a_1 \left[ (\sigma_y - \sigma_z)^2 + (\sigma_z - \sigma_x)^2 \right] + a_3 \left( \sigma_x - \sigma_y \right)^2$$

$$+ a_4 \left( \tau_{yz}^2 + \tau_{zx}^2 \right) + a_6 \tau_{xy}^2 + a_7 \left( \sigma_x + \sigma_y + \sigma_z \right)^{-1},$$  

(1)

with $a_6 = 2(a_1 + 2a_3)$. The independent coefficients $a_{1}', a_{3}', a_{4}'$, and $a_{7}'$ can be determined from measured ice strength properties.

For a description of consolidated ridge ice, the use of an identical yield function (1) seems inappropriate. Ice pieces incorporated in a ridge are randomly oriented and the global strength anisotropy observed in sheet ice is not expected in ridge ice; therefore, for an approximate description of the ridge ice global strength, the following isotropic version of the failure criterion is used:

$$f(\sigma) = a_{1}' \left[ (\sigma_x - \sigma_z)^2 + (\sigma_z - \sigma_y)^2 + (\sigma_x - \sigma_y)^2 + 6(\tau_{xy}^2 + \tau_{yz}^2 + \tau_{zx}^2) \right]$$

$$+ a_{7}' \left( \sigma_x + \sigma_y + \sigma_z \right)^{-1},$$

(2)

where the coefficients $a_{1}'$ and $a_{7}'$ can be determined from two known strength values. This yield function describes a material with differing tensile and compressive strengths and with the property that a confining hydrostatic pressure produces a parabolic increase in strength.

**Failure Mechanisms**

Conceivable failure patterns of consolidated ridges interacting with cylindrical and flat-sided vertical structures include crushing, shearing, and in-plane bending. The failure patterns may
include out-of-plane bending against conical or flat-sided inclined structures.

The crushing and shearing failure patterns are considered in this paper. A detailed consideration is given to a simplified three-dimensional failure mode, in which a vertical structure shears a broadside advancing ridge and the adjacent ice sheet fails simultaneously in a crushing mode. Limited consideration is given to a plane strain crushing failure mode.

The ridge shearing is envisioned along two vertical surfaces of velocity discontinuity, which are at an angle, \( \psi_0 \), to the indentation direction. The sheared ridge "plug" is assumed to act as a flat indenter on the adjoining ice sheet. The failure mechanism for the ice sheet is specified by an inclination angle, \( \phi \), of the forward discontinuity surface, an angle, \( \theta \), between velocity vector and this surface, and an angle, \( \psi_1 \), between the sideward discontinuity surfaces and the indentation direction, as shown in Fig. 1. A no-slip (no relative movement) condition is assumed between the structure and the ridge, while a free-slip (no shear stress) condition is assumed between the ridge and the ice sheet; i.e., it is assumed that a crack forms between the ridge and the ice sheet in the failure zone. The latter assumption does not affect the results significantly; for instance, for a flat-sided wide indenter and uniform ice sheet interaction, the free-slip and no-slip conditions give practically the same force upper bounds.

For a given penetration distance, the considered failure mechanism is specified completely by the angles \( \psi_0, \phi, \theta, \) and \( \psi_1 \). The force results in the maximum value when the structure reaches such a state of penetration that the total structure width, \( D \), and the effective ridge width, \( B_0 \) (Fig. 1), are simultaneously involved. For a flat-sided structure the maximum force is thus achieved when the structure starts to penetrate the ridge; for a circular structure when the structure already penetrated the ridge to a distance equal to \( D/2 \).

The ridge crushing can be represented by a plane strain failure mechanism consisting of a discontinuous velocity field shown in Fig. 2. The structure penetration into the ridge causes the ice to deform in two regions, to the right and left of the structure. The regions are separated from the rest of the ice by discontinuity surfaces defined by logarithmic spirals. The failure mechanism was used earlier for sheet ice force upper bound calculations and described in detail by Ralston [2] and Prodanovic [4].

**Consolidated Ridge Force Upper Bounds**

According to the upper-bound theorem of the plasticity theory limit analysis, the rate of work done by the external force \( F \) is equal to the total rate of energy dissipation for the assumed velocity field. That is, \( FV = D_C + D_S \), where \( V \) is the velocity of ice advance,
Fig. 2 First-Year Ridge Crushing Mechanism
and DC and DS are rates of energy dissipation in the ridge and sheet ice, respectively. The
DC and DS expressions are shown in Appendices A and B.

The ridge shearing total force is obtained as:

\[ F = 2A_o \left( C_1 \tan \psi_o + C_2 \cot \psi_o \right) \\
+ TD_1 \left[ \frac{g_1}{\sin \phi} + \frac{T}{2D_1} \left( \frac{g_1}{\sin \phi \tan \phi} + \frac{g_2}{\tan \phi \cos \psi} \right) \right], \quad (3) \]

where the constants C1 and C2 depend on the coefficients of the ridge ice yield function (2); functions g1 and g2 depend on the coefficients of the sheet ice yield function (1) and failure mechanism parameters (Fig. 1); Ao is the effective ridge cross-section area; and D1 is the ridge shear "plug" width, all defined in the appendices. The desired upper bound for the maximum force that can be exerted in the collision between the structure and the ridge is the minimum value of F with respect to the four variables \( \psi_o, \phi, \theta, \) and \( \psi_1 \), subject to the following constraints: \( \theta > 0, \phi + \theta < \pi/2, \) and \( \sin \psi_1 \cos (\phi + \theta) > 0. \)

The ridge crushing force is obtained in the following integral form:

\[ F = \frac{D}{\sin 2\psi_o} \left( C_1 \tan \phi + C_2 \cot \phi \right) \int_{\psi_o}^{\pi} \int_{z_0}^{z_1} e^{2(\psi - \psi_o)} \tan \phi dz d\psi, \quad (4) \]

where \( \psi_o \) and \( \phi \) are the failure mechanism parameters (Fig. 2) and \( z_0 \) and \( z_1 \) are the integration limits over variable thickness of the ridge (i.e., the lower and upper surfaces of the ridge). The force depends on the current penetration distance of the structure into the ridge. For a given penetration distance, the minimum value of F is obtained by optimizing (4) with respect to \( \psi_o \) and \( \phi \) (where \( \phi > 0 \)). The desired upper bound is then selected as the largest F value, that corresponds to a critical penetration distance.

Bound Comparison

The ridge shearing and crushing upper bounds are compared in a dimensionless form in Fig. 3. For an assumed perfect contact between the ridge ice and the structure (but without adfreeze), a normalized ridge force is computed as \( F/(\sigma_u DT) \), where \( \sigma_u \) is the in-plane unconfined compressive strength of the sheet ice, D is the structure diameter, and T is the thickness of the uniform ice cover. In the computation, the coefficients of sheet ice yield function (1) were based on results of triaxial compressive tests on laboratory grown, columnar grained freshwater ice. The coefficients of ridge ice yield function (2) were based
Fig. 3 Consolidated Ridge Force Upper Bounds

Fig. 4 Rubble Force Upper Bounds
on ice strength values derived by averaging (with respect to orientation) the triaxial compressive strength data for the columnar grained freshwater ice. The figure shows the resulting ridge forces for the aspect ratios D/T = 1 to 100, the keel depth to ice sheet thickness ratios K/T = 0.9, 2, 5, 10, 20, and 30, and σ_u' = 1.5 σ_u, where σ_u' is the unconfined compressive strength of the ridge ice. The dimensionless ridge forces generally decrease with increasing aspect ratio.

Although the plane strain (crushing) failure mechanism provides rigorous upper bounds for all structure dimensions, these bounds are most accurate for small structure dimensions compared to the total ice thickness, i.e., at low aspect ratio, D/T→0. The ridge shearing mechanism gives overestimated ridge forces for low aspect ratios, but it provides better bounds for ridge forces at high aspect ratios.

The ridge shearing analysis yields unrealistically high sheet ice forces for D/T→∞. For instance, the corresponding size-effect parameter for a uniform ice sheet, F/(σ_u DT) for K/T = 0.9 in Fig. 3, resulted in a value of about 9 at D/T→∞, whereas a value of 3 has been earlier confirmed for uniform ice sheets by a plane stress analysis, Prodanovic [5].

UNCONSOLIDATED RIDGES

First-Year Ridge Profile

Ice pressure ridges which have not yet endured a melt season are referred to as first-year ridges. In contrast to multiyear ridges which are generally consolidated, a major part of a first-year ridge is unconsolidated. These ridges generally consist of ice rubble contained in a visible sail and a submerged keel, with a solid ice zone in between. The ridge keel is primarily composed of relatively soft, unconsolidated rubble and voids filled with slush and water. The ridge sail contains relatively well drained ice rubble and voids filled with snow and air.

Shortly after it has been formed, a pressure ridge is strictly an unconsolidated accumulation of broken ice pieces and acts as a discontinuity in the ice cover. By aging, it acquires a refrozen or consolidated zone, the thickness of which depends on the thermal history and is often equal to, or larger than, the surrounding ice sheet thickness. Such a first-year ridge acts as a reinforcement to the ice cover in which it is embedded.

A typical structural configuration of a first-year ridge, assumed in the present analysis, is shown in Fig. 2. A solid zone is assumed of uniform thickness, t; the ratio of the ridge keel depth, K, and sail height, S, is assumed as 5:1. The keel slope (related to the repose angle
of the submerged rubble) is taken as \(32^\circ\) to the horizontal. This is an average value observed in floating first-year ridges. The sail slope is taken as \(24^\circ\) to the horizontal.

**Failure Mechanisms**

The solidified zone and unconsolidated rubble of a first-year ridge are assumed to deform and fail independently of each other in this analysis. The maximum horizontal ridge load \((F)\) is thus perceived as the sum \((F = F_S + F_R)\) of a force imposed by the solid zone \((F_S)\) and a force imposed by the rubble \((F_R)\).

The solid zone is assumed to crush against a vertical structure in the same manner as sheet ice. The sheet ice failure modes and associated upper bounds have been investigated earlier (Ralston [2] and Prodanovic [4]); and are not investigated further in the present analysis.

The failure of rubble contained in the ridge keel and sail is described by "plug-type" shearing and "gate-type" crushing modes. Both failure modes have been observed in first-year ridge model tests; the plug shearing failure pattern apparently occurs more often. Jagged sail crest and keel bottom lines could lead to mixed-type failure modes.

The plug-type failure mechanism is the same as that considered in the analysis of consolidated ridges. The mechanism is defined by two vertical surfaces of velocity discontinuity, which are at an angle, \(\psi_o\), to the indentation direction (similar to that shown in Fig. 1). The shearing failure mode is likely to occur when the structure diameter is large in comparison to the ridge thickness.

The gate-type failure mechanism resembles the classical Prandtl velocity field; ice rubble pieces flow and clear on both sides around the structure. A discontinuous velocity field defined by logarithmic spirals, shown in Fig. 2, as used for the plane strain analysis of sheet ice and consolidated ridges, is also used to investigate rubble crushing. This failure mode is likely to occur when structure diameter is small in comparison with the ridge thickness.

**Rubble Failure Criteria**

Little information is available on strength properties of ice rubble contained in a first-year keel and sail. The rubble is a multi-phase, highly complex material, properties of which are expected to be highly variable as a result of variable conditions under which it forms and ages.

Limited model test data on rubble strength reported to date (Prodanovic [5] and Weiss et al
indicate that ice rubble, on a sufficiently large scale, can be considered homogeneous and isotropic, and that its strength increases approximately linearly with confinement.

The rubble is assumed to behave as a linear, cohesive Mohr–Coulomb material, with the following two-dimensional yield criterion:

\[ \tau = c + \sigma \tan \phi, \]  

where \( \tau \) and \( \sigma \) are the shear and normal stresses on the shear plane, and \( c \) and \( \phi \) are independent parameters, referred to as the effective cohesion and angle of internal friction. A three-dimensional extrapolation of this yield function (Drucker and Prager [11]) is used for construction of rubble force upper bounds.

**Rubble Force Upper Bounds**

Using the plasticity upper bound theorem and the expression for rate of energy dissipation during rubble plug shearing given in Appendix C, the rubble shearing force is obtained as:

\[ F_R = 2A_o c, \]  

where \( A_o \) is the cross-sectional area of the rubble contained in the ridge keel and sail, and \( c \) is the effective cohesion of the rubble.

The rubble crushing force is obtained as:

\[ F_R = \frac{cD}{\sin 2 \psi_o} \int \int \int \int \int \int e^2 \left( \psi - \psi_o \right) \tan \phi \, dz \, d\psi, \]  

where \( \psi_o \) is the failure mechanism parameter (\( \phi \) is the angle of internal friction), and \( z_o \) and \( z_1 \) are the integration limits over the variable rubble thickness (that excludes solid ice thickness). The force also depends on the current ridge penetration distance.

**Bound Comparisons**

The rubble shearing and (simplified) plane strain upper bounds for rubble forces on a cylindrical structure are plotted in a log-log scale and dimensionless form in Fig. 4. The ordinate represents the normalized rubble force \( F_R/(cDK) \), where \( c \) is the rubble effective cohesion, \( K \) is the ridge keel depth, and \( D \) is the structure diameter. The abscissa represents the aspect ratio \( D/T \), where \( T \) is the consolidated zone thickness. Different curves shown
in the figure relate to different K/T ratios.

The governing failure mode at high aspect ratios is the rubble shearing. At lower aspect ratios, the rubble crushing provides cut-offs, similar to those obtained for the consolidated ridges. Other ridge profiles would have upper bound curves shifted.

A bound for the solid ice force ($F_S$) has to be added to the bound for rubble force ($F_R$) to obtain an upper bound for the first-year ridge total force ($F$). The applicability of this approximation is demonstrated by a favorable comparison of the computed upper bounds and ridge loads measured in model and field tests.

Load records obtained during two seasons on a 14-ft diameter test pier in Cook Inlet indicated that first-year ridge loads are typically 2 to 3 (maximum 3.6) times the sheet ice loads, Blenkarn [1]. The variations in ridge loads are believed to stem from variations in ridge geometry and integrity. The seasonal ice floes in the inlet are up to 2 ft thick; they are accompanied by first-year ridges with keel depths of up to 20 ft. The primary failure mechanisms observed were direct crushing and shearing. Uniform ice floes exerted a steady force of 10 to 20 kip/ft, whereas the maximum ridge loads were 60 to 70 kip/ft of the pier diameter. For $K/T = 20/2 = 10$ and $D/T = 14/2 = 7$, Fig. 4 gives the dimensionless ridge force upper bound $F_R/(cDK) = 4$. With an estimated $c = 5$ psi [5], the upper bound for rubble force is $F_R = 4 (5) 144 (14) 20/1000 = 806$ kip. The measured rubble force was approximately $F_R = F - F_S = (70 - 20) 14 = 700$ kip.

REFERENCES


APPENDICES

A. Energy Dissipation in Consolidated Ridges

Assuming the isotropic quadratic yield function represented by (2), the rate of energy dissipation per unit area of a discontinuity surface is given by

\[ D_A = C_1 \delta_v + C_2 \delta_u^2 / \delta_v, \]

where \( \delta_v \) and \( \delta_u \) are the normal and tangential velocity jump components across the surface of discontinuity. The constants \( C_1 \) and \( C_2 \) depend on the coefficients of the yield function and are defined by

\[ C_1 = 1/3a_1' + a_r'/(6a_1') \]
\[ C_2 = a_r'/(8a_1'). \]

For the consolidated ridge shearing failure mechanism shown in Fig. 1, the velocity jump components are given by \( \delta_v = V \sin \psi_o \) and \( \delta_u = V \cos \psi_o \). The rate of energy dissipation is:

\[ D_C = 2 \iiint_A D_A \, dA, \]

where \( A \) is the shearing surface. If \( A_o \) designates the cross-sectional area of the ridge, then \( A_o = A \cos \psi_o \). The rate of energy dissipation in the ridge results as

\[ D_C = 2A_o V (C_1 \tan \psi_o + C_2 \cot \psi_o). \]

The dissipation rate in the ridge does not depend on the shape of ridge profile, but on the total area of the ridge cross-section only. Hence, for comparison of force bounds for ridges
with different profiles, only the total cross-sectional area has to be specified. For the consolidated ridge profile under consideration, the area is given by \( A_0 = 4.02 K^2 + 0.38 KT - 1.69 T^2 \).

B. Energy Dissipation in Sheet Ice

For the anisotropic pressure-sensitive yield criterion given by (1), the rate of energy dissipation per unit area of velocity discontinuity results as

\[
D_A = \frac{\delta_v}{3a_7} + \frac{3a_7}{\delta_v} \left\{ \frac{1}{18a_1} \left[ v_z n_z - \frac{1}{2} (v x n_x + v y n_y) \right]^2 + \frac{1}{4a_6} (v x^2 + v y^2) (n x^2 + n y^2) + \frac{1}{4a_4} \left[ (n x v_z + n z v x)^2 + (n y v_z + n z v y)^2 \right] \right\},
\]

where \( \delta_v \) is the normal velocity jump component across the surface of discontinuity; \( n x, n y, \) and \( n z \) are components of the unit vector normal to the discontinuity; and \( v x, v y, \) and \( v z \) are components of the velocity jump vector relative to the coordinate system shown in Fig. 1. For the considered velocity field, the rate of energy dissipation in the ice sheet may be expressed as

\[
D_S = D TV \left[ g_1 \frac{\tan \psi}{\sin \phi} + \frac{T}{2D_1} \left( g_1 \frac{\tan \psi_1}{\sin \phi \tan \phi} + \frac{g_2}{\tan \phi \cos \psi_1} \right) \right],
\]

where \( D_1 = D + 2B_0 \tan \psi_0 \) (Fig. 1) is the effective ridge "plug" width, and

\[
g_1 = \frac{\sin \theta}{3a_7 \cos (\phi + \theta)} + \frac{3a_7 \cos (\phi + \theta)}{\sin \theta} \left\{ \frac{1}{18a_1} [\cos \phi \tan (\phi + \theta) \right\}
\]

\[
+ \frac{1}{2} \sin \phi^2 \right\} + \frac{\sin^2 \phi}{4a_6} + \frac{1}{4a_4} [\cos \phi - \sin \phi \tan (\phi + \theta)]^2 \right\},
\]

\[
g_2 = \frac{\sin \psi_1}{3a_7} + \frac{3a_7}{\sin \psi_1} \left[ \frac{\sin^2 \psi_1}{72a_1} + \frac{1}{4a_6} + \frac{\tan^2 (\phi + \theta)}{4a_4} \right]
\]

For the assumed consolidated ridge profile, the effective ridge width is given by: \( B_0 = 5.50 K - 3.12 T \).
C. Energy Dissipation in Ice Rubble

Assuming the linear Mohr-Coulomb yield function (5), it can be shown that the rate of plastic energy dissipation per discontinuity surface unit area is \( D_A = c \delta_v / \tan \phi \) where \( \delta_v \) is the normal velocity jump component across the surface. For the plug shearing velocity field, the total rate of energy dissipation is \( D_R = 2A_o V_c \), where \( A_o \) is the cross-sectional area of the first-year ridge rubble. For the idealized first-year ridge profile shown in Fig. 2, \( A_o = 1.69K^2 - 2.97KT + 1.32T^2 \).
MARINE FOUNDATIONS

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ABSTRACT

Present-day experience indicates that jacket foundations are most economical for open ocean conditions. There has been a definite shift to jacket structures in the North Sea and offshore structures elsewhere are continuing to be jacket structures. These trends apply in Canadian offshore areas at Sable Island and Queen Charlotte Islands.

Piled foundations have been widely used for drilling and production platforms, resisting open water shear and overturning forces. Forces acting on these structures are an order of magnitude less than arctic ice forces. Therefore, in all Canadian areas, except Sable Island and the Queen Charlotte Islands, extensive massive piling is required. Gravity-type structures made of concrete or steel are suitable for high thrust situations.

In arctic waters, earth mounds, confined earth islands and confined gravel islands have been used or are being installed. Essentially, these structures transmit ice forces to foundations by gravity.

For deeper water, structures with gravity foundations have been proposed for Canadian waters. The result is a heavier structure designed for different conditions than the ocean structures described above.

Wherever proper soil conditions are available, gravity structures can be placed directly on the bed, or, in greater water depth, on earth or rock-fill mounds. Subbases have been shown to be able to withstand the erosion and ice forces. Oceanbed protection has been achieved by rock fill, mattresses or baffling elements.

Floating mooring facilities and floating platforms are also indicated in ice-covered areas. In ice-affected areas, this type of structure would need a special anchor system to be developed since forces on the anchor system could be expected to be larger than anchor forces in open water conditions. New developments in anchor design are also necessary if more rigid floating structures such as tension-leg platforms and guyed tower structures are to be used in Canadian offshore waters.
This paper addresses foundation problems in Canadian offshore areas where production can be visualized within 20 years. The offshore areas for consideration in this report have included the following:

- Hibernia field offshore Newfoundland
- Beaufort Sea
- Sable Island
- Arctic Archipelago
- Labrador Sea near Hopedale
- Baffin offshore waters
- West Coast, Queen Charlotte Islands

The production of oil and gas in Canadian offshore areas will demand new types of support structures to withstand the extreme conditions imposed by the environment. The main technical problems involved in the selection and design of platforms, including foundations, are understood. However, there are serious inadequacies in the information available for the detailed design of critical components such as anchor systems and for the mathematical analysis of foundations subjected to large dynamic loadings.

All these areas, except Sable Island and the West Coast, have ice problems of one kind or another. The areas open to the Atlantic Ocean exhibit special oceanographic conditions with strong waves and bottom currents. Some oil finds in the Labrador Sea, Baffin Bay and the Arctic Archipelago are located in very deep water with severe exposure to ice.

Requirements in engineering are summarized in the following chart which identifies offshore production systems and their application in the particular areas of interest. These requirements are presented in Table 1.

**FOUNDATION PROBLEMS**

As shown in the table and as evidenced from the above discussions, there are a series of foundation problems which arise from the location of marine structures in the Canadian areas of interest for new development. It seems to be that the following engineering highlights are of particular interest:

- foundation and rock stability under static loads
- foundation safety under large cyclic loads
- ice, iceberg and impact resistance
- foundations on subsea permafrost
- anchor capacity
- piping and scour problems.

**Foundation Soil and Rock Stability under Static Loads**

The use of concrete gravity structures in the North Sea resulted in the solution of many geotechnical problems and showed foundation hazards which are yet to be investigated. The reaching of new geotechnical engineering frontiers has been described by Gibson and Dowse at the First Canadian Conference on Marine Geotechnical Engineering (1979) as:

1) foundation design of footings up to 140 m diameter with loads of up to 600,000 tonnes;
2) static penetration of 4 to 5 m of skirts below the structures and a total length below a typical structure in the order of 2 km;
3) cyclic loading of the foundation soils under storm conditions;
4) dynamic response of a large concrete, liquid and soil structure; and
5) setting down of an immense load on the seabed in a period of less than 24 hours.
### TABLE 1

#### ENGINEERING REQUIREMENTS

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<tr>
<th>OFFSHORE FACILITIES</th>
<th>Hibernia</th>
<th>Beaufort Sea</th>
<th>Sable Island</th>
<th>Arctic Archipelago</th>
<th>Labrador</th>
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<td>3. Tension Leg Platforms</td>
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<td>4. Subsea Production</td>
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<td>5. Overice Schemes</td>
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<td>7. Pipelines</td>
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<td>AS</td>
<td>GACS</td>
<td>GAS</td>
<td>GAS</td>
<td>GS</td>
</tr>
</tbody>
</table>

**Engineering Highlights**

- **G**: Geotechnical and rock studies for static loads
- **A**: Analysis for large cyclic loads
- **R**: Ice, iceberg and impact resistance
- **C**: Foundations on Subsea Permafrost
- **D**: Anchor systems
- **S**: Piping and scour problems
In Canadian offshore areas, subsoil conditions may include permafrost, highly sensitive clays and boulder fields. Generally speaking, a better knowledge of the static and dynamic geotechnical properties of Canadian sea bottom soils is required together with their interaction with bottom-supported structures.

A large body of literature is available on the geology of the offshore areas of Canada. Much of the information has been published by universities and government departments. However, a good deal of information has been gathered by the private sector which is outside the public domain. Systematic surficial geology studies have been published on the southern Beaufort Sea. Marine geological studies in the Arctic Archipelago are rare. Site specific studies have been performed in numerous offshore areas but their applicability to broader areas is limited.

Present knowledge of subsea bedrock conditions is even less well-defined than the properties of the surficial sediments. Definite data are confined to the very limited oil exploration activities to date.

As in the case of seismic events, firsthand observations of subsea mass movements in Canada are nonexistent. Studies in Canada have been performed, for example, at Kitimat, B.C., which have led to a greater understanding of the mechanism involved. There is a general lack of data available on predicting the effects of subsea mass movements on offshore structures.

Foundation Safety under Large Cyclic Loads

Cyclic loads which have been found to be dangerous to structures in the North Sea could be important for structures subjected to large waves as in the Hibernia location, Sable Island and Queen Charlotte Island locations. In areas of heavy ice, wave effect is reduced and existing range of experience can be directly applied.

The basis for much of our present knowledge of seismic effects on onshore structures is based on firsthand observations. These are not possible in the offshore areas. Of particular importance are slope instability and dynamic loadings on foundation soils. Increased data on seismic events is necessary for a predictive capability. Detailed seismic records for northern regions are available for the past 20 years. Statistical analyses are therefore limited to a rather small time frame.

Ice, Iceberg and Impact Resistance

Ice and iceberg effects will be the major factor why large structures in the mentioned seven Canadian offshore areas will differ from offshore structures elsewhere.

Sea Ice

Structures may be exposed to ice forces as static loading or as dynamic horizontal loads or as vertical loading from ice movement or gravity effects. Depending on the ice region and the structure, either static or dynamic effects could result in the governing design force.

There are numerous modes of ice sheet failure against a structure. The mode usually depends on the shape of the structure and the time to failure (i.e., speed of ice sheet). The most common failure types are compression (crushing), buckling, bending and flaking.
### Arctic Coastal Ice Features - Design Considerations

<table>
<thead>
<tr>
<th>Water Depth</th>
<th>Typical Design Considerations</th>
</tr>
</thead>
<tbody>
<tr>
<td>0 to about 3 m</td>
<td>- Design for slow moving sea ice up to 3 m thick</td>
</tr>
<tr>
<td></td>
<td>- Sea Ice could be fresh near river mouths</td>
</tr>
<tr>
<td></td>
<td>- Can defend</td>
</tr>
<tr>
<td>3 m to about 20 m</td>
<td>- Design for fast moving sheet ice up to 20 m thick</td>
</tr>
<tr>
<td>(Fast Ice Zone)</td>
<td>- Design for fast moving multi-year ridges as thick as water depth plus</td>
</tr>
<tr>
<td></td>
<td>- Consider addressing</td>
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<tr>
<td></td>
<td>- Consider Ice Pile-Up</td>
</tr>
<tr>
<td>20 m plus</td>
<td>- Design for multi-year ridges up to 20 m thick (Risk Analysis)</td>
</tr>
<tr>
<td></td>
<td>- Sheet ice between rubble fields up to 5 m thick</td>
</tr>
<tr>
<td></td>
<td>- Consider addressing</td>
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<tr>
<td></td>
<td>- Consider Ice islands (Risk Analysis or Defence)</td>
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<tr>
<td></td>
<td>- Consider Ice Pile-Up</td>
</tr>
</tbody>
</table>

#### Probability Distribution

![Probability Distribution](image)

#### Probability Density for One Channel

![Probability Density for One Channel](image)
EXCEEDANCE PROBABILITIES
OF INSTANTANEOUS PRESSURE
FOR COLD SEA ICE

PROBABILITY DENSITY OF INSTANTANEOUS PRESSURE
Considerable research effort has been centred on determining ice forces on structures. Ultimate forces on structures locating in ice-infested waters are almost always due to the activities of ice. The amount of easily applicable experimental data is still small and purely analytical approaches are complex and not fully developed. The need for economically sound offshore drilling methods has promoted industry to evolve many ice failure models. To develop better integrated models than those in existence would require a better understanding of the ice itself. For these reasons much design work uses empirical or historical estimates of forces. The mechanical properties of ice are so complex and variable that any analytical estimates must be well supported by experiments. Full-scale experiments are expensive and the number of variables needing investigation makes it impractical to design on that basis alone. Thus, the preferable design method must be based on analytical theory (mathematical algorithm) supported by field tests on a full-scale structure and time sequence measurements of the ice conditions meteorological and oceanographic parameters in the study area.

When determining the magnitude and direction of ice loads, considerations are given to the nature of ice, mechanical properties of the ice, ice structure contact area, shape of structure, direction of ice movements, etc. The oscillating nature of the ice loads is considered.

Ice loads other than those caused by laterally moving ice, such as loads due to masses of ice frozen to the structure and possible impact loads during thaw of the ice are taken into account.

Icebergs

Icebergs are a potential major problem on the east coast. The icebergs are usually calved from glaciers on the Greenland ice cap. Once in Davis Strait, they drift slowly and erratically southwards, often breaking from internal cracks as they melt. Both the number of the size of icebergs decrease rapidly once the bergs move south of 60°. In an average year, about 1200 bergs are monitored moving past 60° latitude with only about 250 passing it to 50° latitude.

These concentrations show the magnitude of the problem in Labrador offshore waters.

Iceberg impact absorption/deflection techniques have been suggested using various moored arrays or fixed bottom structures on rubble piles. None have been taken beyond the concept stage. Further work could be profitably done in this area.

Foundations on Subsea Permafrost

Permafrost occurs extensively throughout the Beaufort Sea area and a Map of Surficial Permafrost in this area is shortly to be released by the Canadian Geotechnical Survey. It is generally found in a continuous zone from 150-700 m in the sediments. In waters less than 30 m, it is believed to be degrading but in deeper waters some aggregation has been noted. Presence of true permafrost can be detected fairly readily by seismic reformation, but work needs to be done on detecting the gradual transition from normal sediments through ice blending to permafrost. This transition zone is believed to be significantly greater than the permafrost zone flatbed.

The implication of permafrost on marine structures is similar to that on land. The effect of structure-induced thermal flux on the load bearing capacity of soil is a major concern.

In this connection, the presence and confirmation of hydrates in the zone of influence of structural thermal/flux is of major concern--roughly 1 m³ of hydrate producing 170 m³ of gas. In offshore oil and gas production, the need for an understanding of hydrate degradation is essential.
One manifestation of permafrost is the occurrence of pingos, hill-like structures rising several metres above ground and occurring both on land and under the sea. Over 200 have been detected by the Canadian Hydrographic Department in the Western Arctic. It is suspected that possibly twice this number exist, probably all in the

**Anchor Capacity**

Anchor systems presently in use offshore involve conventional marine anchors, drilled-in and cemented piling, driven piles, jetted-in piles or deadmen, and explosive anchors. Unique sea bottom conditions are prevalent in many Canadian offshore and inshore areas. Such bottom conditions include very sensitive marine clays, glacial drift, boulder fields, exposed hard bedrock, permafrost, areas of sand wave activity, and underconsolidated sediments.

**Conventional Marine Anchors**

Some items which require consideration are: very soft/loose sea bottom conditions in the Arctic Islands, strong currents and deep water off the east coast, rapid disengagement/recovery in ice-infested waters, extra strong capability to withstand moderate ice floes, etc. Research is needed to enable more reliable prediction of anchor performance in such frontier conditions.

**Pile-Type Anchors**

Research is required regarding the performance and design of pile-type anchors in the Arctic offshore and ice-infested areas. This includes such aspects as driving the piles in permafrost, the long-term thermal regime around such piles and how this will affect their capability, and the installation of piles in deep soft/very loose sediments or difficult bouldery soils. The capability of installing such anchors deep enough to develop required capacity needs review, since very large capacity may be required to withstand ice forces.

**Piping and Scour Problems**

In shallow water, waves can cause scour through the effect of orbital velocity. Ocean currents will affect mainly structures in shallower water but the deep ocean currents should also be considered for deeper foundations.

The most significant scour phenomenon in recent years has been shown to be caused by cyclic pressure variation in the soil and pore water under a structure. This effect increases with increasing loading and with increasing size of the footing. Thus it is particularly important for deep water gravity platforms.

Large gravity structures as used in the North Sea have shown that the cyclic loads caused by waves can generate excess pore pressures in foundation materials. It has been found that pressure gradients can reach critical gradients for piping.

Pressure in pore water can be imposed in two different ways on soil beneath the sea. A pressure wave can move on the seabed in phase with a displacement wave on the water surface, causing pressure gradients in the soil pore water.

In the second phase, excess pressure is placed on the soil beneath by the overturning moment of the platform due to the wave load. Considerable pore pressure variations have been generated along the edge of the bottom plate by the rocking of the platform. Skirts have been placed around footings to prevent high sea seepage forces which otherwise would develop along short drainage paths.
Ice Scour

In much of offshore Canada, ice scour represents a definite hazard to offshore structures, particularly pipelines and seabottom installations.

Echo sounding traces of the sea floor of the Beaufort Sea have revealed a complicated micro relief pattern believed to be the result of sea ice scour. Apparently large pressure ridges abound on the sea floor but are kept moving by winds and pack ice. The results are long scours gouged out by pressure ridge keels on the sea floor. These scours are parallel to winter wind directions and have been recorded as deep as 10 m.

A number of projects have been initiated to predict ice forces on seabed material, frequency of ice scour compared with scour depth, water depth and location, and minimum scours caused by given forces. Frequency has proved quite difficult to predict, a major unknown being the age of the scours. Some of the scours investigated are believed to be over 14,000 years old.

Iceberg scours are somewhat deeper than sea ice scours because of the greater mass. Other than depth, iceberg scouring is very much the same as sea ice scouring and research requirements and concerns for the two are similar.

TYPES OF MARINE STRUCTURES

As shown, new foundation developments are called for structures built in ice-affected waters. Platforms and marine terminals have to be designed to withstand forces due to the action of ice. Depending on the area, ice forces could exceed other overturning environmental and operational forces by an order of magnitude.

Presently, there are two broad types of offshore structures, namely bottom-founded structures and floating platforms. Each type has its advantages and disadvantages, depending on the ice conditions, water depth and other factors.

A number of offshore structures placed in ice-affected waters have performed well over a period of several years. Steel-built structures such as jacket, jack-up and monopods were used in Cook Inlet, Alaska, with its moderate ice conditions as early as 1963. Initially, ice action caused considerable operational and structural problems. These, however, were overcome with an increased knowledge of ice and its interaction with the structure. One decade later Esso started to build its first gravel island in the southern Beaufort Sea in shallow water. Those islands also performed well even though they had to withstand some of the most severe ice conditions. A program of implementing caisson-retained islands has started.

Structures in Shallow Water Depths

Unconfined islands, confined islands, and cells and caissons have been used for navigation structures and platforms in shallow ice-affected waters.

Artificial islands require protection and continued maintenance for use as production platforms. Wind, waves and ice affect the islands constantly over the long period of use. Caisson-retained islands are likely to survive better but the foundations will require frequent inspection and maintenance.

Piled Foundations

Jacket-type pile-supported structures and jack-up production platforms would normally only be used in southern waters where ice forces are not severe. More massive pile foundations have been prepared for arctic applications. Concrete monocones have also been proposed for production.
Types of Offshore Platforms for Beaufort Sea

<table>
<thead>
<tr>
<th>ADVANTAGES</th>
<th>DISADVANTAGES</th>
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<tbody>
<tr>
<td><strong>GRAVITY ISLANDS</strong></td>
<td><strong>RETIRED ISLANDS</strong></td>
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<tr>
<td>- Year Round Platform</td>
<td>- Requires Kangaroo</td>
</tr>
<tr>
<td>- Relatively Low Cost</td>
<td>- All Year Protection</td>
</tr>
<tr>
<td>- Proven Technology</td>
<td>- Not Versatile &amp; Fewer Locations</td>
</tr>
<tr>
<td>- Short Lead Time</td>
<td>- Non-Usable During Winter</td>
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<tr>
<td>- Can Be Used for Production</td>
<td>- Less Permanent</td>
</tr>
<tr>
<td>- Good for Ice Forces</td>
<td>- Less Construction Time</td>
</tr>
<tr>
<td>- Requires Kangaroo</td>
<td>- More Expensive</td>
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<tr>
<td></td>
<td>- Longer Lead Time</td>
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<tr>
<td></td>
<td>- Transportation to Site</td>
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<tr>
<td></td>
<td>- Non-Proven</td>
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<tr>
<td></td>
<td>- Ice Forces</td>
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<td></td>
<td>- Ice Island Risk</td>
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</table>
Gravity Platforms

Structures with gravity foundations have been considered for almost all areas but are unlikely to be used in very deep waters. Limited use only can be foreseen in areas where icebergs or severe pack ice are a problem.

The principle of the gravity platform is that it will rest on the seabed and is heavy enough to resist movement caused by lateral forces. The gravity platform does require a sound seabed foundation to limit settlement and tilting of the structure.

In arctic areas, the effects of ice on closed structures such as steel sheet pile cell wharves is still to be defined. Research is also required into possible implications of degradation of permafrost under fixed structures.

Structural and geotechnical factors are still to be defined to design structures to absorb iceberg impact.

Floating Structures for Deeper Waters

Floating production platforms and marine terminals are being considered for use in areas where ice conditions would allow their use during the summer months or a full year depending on conditions. These would most likely be dynamically positioned or quick-release ships operating for a part of the year. Estimates of the operating season for such platforms in the Beaufort Sea range from 100 days/year to 200-300 days/year with heavy icebreaker support. Such platforms would require the ability to move off the site quickly if severe ice or icebergs interfered and to reposition and reconnect easily.

Tension-Leg Platform

Tension-leg platforms are suitable for deep water where they require anchor capacities of perhaps 10,000 to 20,000 tons. In ice-affected waters, larger capacities than that should be considered to provide minimum resistance to frequently occurring mild ice pack conditions.

Guyed Tower Structure

Exxon has proposed a guyed tower which allows the floating structure to resist larger horizontal loads than the vertical anchored structure. Guy cables in their design have a dead weight anchor and a conventional anchor for ice loading. Other anchor designs should be considered to make the structure stay on location for a longer time.

Comparison of Gravity and Structural Foundations

The weight effect of gravity platforms plays a significant role in the reduction of ice impact forces on the foundation. Jacket structures for 200 m depths could be estimated to weigh about 30,000 tonnes in open water and perhaps 60,000 tonnes in ice-covered waters. This is less than 10% of the weight of a gravity structure in the same depth in ice-covered waters.

The impact force of an iceberg weighing 100,000,000 tonnes could be substantially reduced absorbing excess energy in tilting or shear. Maximum forces will not be directly resisted by soils. On a jacket foundation all the impact will be directly transferred to foundation soils and thus the total contact force will be applied to piles.
Similar reduction effect will apply to ice pack collisions. Ice pack forces create much smaller time average forces than forces during short impulses. Experience has shown that the light and brittle structures have failed even if designed to resist reasonably high pressures. Massive and resilient structures have withstood intense ice loading. Support elements with a large energy to failure would make best use of the time attenuation effect of the ice impact.

Various studies have shown that structural platforms made of concrete or steel can be designed to resist local large forces. Structural foundations run into problems on the other hand. It appears that gravity foundations are the only feasible type of foundation to withstand ice forces amounting to several thousand tonnes, and gravity foundations would have a reappearance on the scene.

In order to handle ice impact, cone structures or other special shapes appear most suitable in severe ice or iceberg locations. This means that while gravity foundations would be reintroduced, they should be shaped differently and thus built differently from open water schemes.
DISCUSSION

By:

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Montreal, Canada

Could you describe what kind of geotechnical investigations would be the most appropriate in connection with the marine foundations designed?
Geotechnical investigations are site and structure specific. A preliminary geotechnical study should be done to aid in the determination of the most suitable type of structure. Such a study is easily justified in regard of the large capital investment required for offshore structures. Some probes of the preliminary study should be extended to great depth, perhaps twice the diameter of the largest foundation alternative. In situ strength, grain sizes, water contact, and density are important. Once a decision has been made with respect to the type of structure, a specific soil investigation program can be elaborated. In Canadian offshore areas, soft soils are likely to be encountered. In these areas, in situ testing is vital to avoid disturbance. Various methods such as vane tests and pressure meter tests could be used to define both long-term and short-term foundation properties. The extent of the program has to be determined by experience in the area.
ON MODELING MESOSCALE ICE DYNAMICS USING A
VISCOUS PLASTIC CONSTITUTIVE LAW

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ABSTRACT

The behavior of an ice dynamics model employing a viscous plastic rheology is investigated. Time and space scales of the order of 3 hours and 20 km are emphasized. However, whenever possible the results are presented in a nondimensional form. Numerical parameter variations examined include the effect of the "rigid" creep rate on numerical convergence rate, the effects of ice strength on the numerical adjustment time needed to fully attain ideal plastic flow, and the effect of grid size on the behavior of simulated ice dynamics. Based on the results of these studies a viable numerical procedure for simulating mesoscale plastic flow is proposed.

I. INTRODUCTION

An important component of ice forecasting models is the ice interaction. This interaction is especially pronounced in regions with narrow land constrictions such as the Bay of Bothnia or the Great Lakes of the United States. Observations of the ice motion there [8, 5] have shown that the ice often remains relatively stationary, even with significant wind forcing. To model this phenomenon Udin and Ullerstig [9] have proposed that the wind field (and hence the ice stress gradient) be modified in regions of ice convergence. While this procedure does produce more realistic forecasts of stationary motion than linear viscous models of the Baltic [6] it does not produce a well defined ice stress field.

A more consistent way of modeling such rigid behavior is to make use of a highly nonlinear ice rheology. In a recent paper, Hibler [2] developed a nonlinear model for the simulation of sea ice circulation and thickness over a seasonal
cycle. Using this model it was possible to reproduce many of the observed features of the circulation and thickness of the Arctic ice cover. In addition, for sufficient- ciently high strengths, this model causes the Arctic ice cover to stop moving [3], even though the wind forcing is significant. A key feature of the model is a viscous plastic rheology. This rheology approximates rigid plastic flow by allowing the ice to flow in a plastic manner for normal strain rates and to creep in a linear viscous manner for very small strain rates.

Investigations of the model to date [2, 3] have mostly utilized realistic Arctic basin geometries and have emphasized the seasonal velocity and ice thickness variations on scales of the order of 100 km. While such seasonal simulations are valuable there are also many features of the model that may well be useful in simulating smaller scale ice dynamics of particular relevance to ice forecasting. However, there are a number of subtleties that arise when this type of rheology is used on a smaller scale. The purpose of this paper is to carry out a methodical investigation of the numerical response characteristics of the momentum balance portion of the model using idealized forcing and geometry. While this study emphasizes the mesoscale response on time and space scales of 3 hours and 20 km, whenever possible the scaling of the results to other dimensions is discussed. In all these studies the numerical code documented by Hibler [4] is employed.

II. DESCRIPTION OF THE MODEL

The model consists of a momentum balance equation coupled to ice thickness evolution equations. For this study, only the momentum balance equation will be considered. The momentum balance includes inertial terms, Coriolis force, wind and water stresses and, most important for this paper, ice interaction. In Cartesian coordinates the momentum balance is

\[
\frac{Du}{Dt} = -\frac{mk}{a} \mathbf{k} \times \mathbf{u} + \mathbf{r}_a + \mathbf{r}_w + mg \mathbf{H} + F
\]

where \(\frac{D}{Dt} = \frac{a}{dt} + \mathbf{u} \cdot \nabla\) is the substantial time derivative, \(\mathbf{k}\) a unit vector normal to the surface, \(\mathbf{u}\) the ice velocity, \(f\) the Coriolis parameter, \(m\) the ice mass per unit area, \(\mathbf{r}_a\) and \(\mathbf{r}_w\) the forces due to air and water stresses, \(\mathbf{H}\) the sea surface dynamic height, \(g\) the acceleration due to gravity, and \(F\) the force due to variation in internal ice stress. The air and water stresses are determined from idealized boundary layers, assuming constant turning angles [7]:

\[
\mathbf{r}_a = \rho_a C_a [U_g |U_g| (U_g \cos \phi + k \times U_g \sin \phi)]
\]

\[
\mathbf{r}_w = \rho_w C_w [U_w - U] [(U_w - U) \cos \theta + k \times (U_w - U) \sin \theta]
\]

where \(U_g\) is the geostrophic wind, \(U_w\) the geostrophic ocean current, \(C_a\) and
\( C \) air and water drag coefficients, \( \rho_a \) and \( \rho_w \) air and water densities, and \( \phi \) and \( \theta \) air and water turning angles. The geostrophic ocean currents are computed by \( \mathbf{U}_w = g \mathbf{\varepsilon}^{-1} \mathbf{\kappa} \times \mathbf{\eta} \).

To simplify analysis of the studies described here, \( \mathbf{U}_w \) has been effectively set to zero, and the momentum advection term set equal to zero. As a consequence the momentum balance employed here is

\[
\frac{\partial \mathbf{u}}{\partial t} = - \nabla P + \mathbf{\tau}_a + \mathbf{\tau}_w + \mathbf{F}
\]  

For modeling the ice interaction the ice is considered to have a nonlinear viscous constitutive law given by

\[
\sigma_{ij} = 2\eta(\dot{\varepsilon}_{ij}, \mathbf{\varepsilon}) \dot{\varepsilon}_{ij} + \left[ \zeta(\dot{\varepsilon}_{ij}, \mathbf{\varepsilon}) - \eta(\dot{\varepsilon}_{ij}, \mathbf{\varepsilon}) \right] \epsilon_{kk} \delta_{ij} - \frac{\mathbf{P}}{2} \delta_{ij} / 2,
\]

where \( \sigma_{ij} \) is the two-dimensional stress tensor, \( \dot{\varepsilon}_{ij} \) the strain rate tensor, \( \mathbf{\varepsilon} \) a pressure term, and \( \zeta \) and \( \eta \) are nonlinear bulk and shear viscosities. Using this constitutive law the force components due to internal stress are calculated from \( \mathbf{F} = 3 \sigma_{ij} / \partial x_j \). For calculations performed here the dependence of \( \zeta \) and \( \eta \) on \( \dot{\varepsilon}_{ij} \) and \( \mathbf{\varepsilon} \) is normally taken so that the stress state lies on an elliptical yield curve passing through the origin with a no-stress condition applying for pure divergence:

\[
\zeta = \frac{\mathbf{\varepsilon} / 2}{\Delta}, \\
\eta = \zeta / e^2,
\]

\[
\Delta = \left( (\dot{\varepsilon}_{11}^2 + \dot{\varepsilon}_{22}^2) (1 + 1/e^2) + 4e^{-2} \dot{\varepsilon}_{11}^2 + 2\dot{\varepsilon}_{11} \dot{\varepsilon}_{22} (1 - 1/e^2) \right)^{1/2},
\]

where \( e \) is the ratio of principal axes on the ellipse. (A derivation of this equation assuming rigid plastic flow, together with a normal flow rule, is given in \([1]\)). For very small strain rates the viscosities in eqs. 3 and 4 become arbitrarily large. To avoid this they are chosen to be the minimum of the plastic values and some large limiting values dependent on the ice strength \( \mathbf{\varepsilon} \). For the calculations performed here the standard limiting values were taken to be

\[
\zeta_{\text{max}} = (5.0 \times 10^7 s)\mathbf{\varepsilon}, \\
\eta_{\text{max}} = \zeta_{\text{max}} / e^2.
\]

For the standard \( \mathbf{\varepsilon} \) and \( m \) values we take \( \mathbf{\varepsilon} = 5.0 \times 10^3 \) \( \text{N m}^{-1} \) and \( m = 0.91 \times 10^3 \) \( \text{kg m}^{-2} \).

A particularly important dimensionless scaling parameter in all the subsequent analysis is \( \beta = \mathbf{\varepsilon} / \tau_a L \) where \( L \) is the length scale of the ice area being simulated.
III. NUMERICAL CHARACTERISTICS

To numerically solve equations 2, 3 and 4 a time stepping procedure using finite difference techniques is employed. The computer code for this procedure is documented by Hibler [4]. There are a number of subtleties that arise when the numerical scheme is used on the mesoscale. Briefly the numerical procedure works as follows. At each time step a linearized momentum balance is solved by relaxation. The viscosities used in the momentum balance are based on the deformation field from the previous time step. Using these viscosities a new velocity field is obtained, a new set of viscosities are estimated and another linearized equation solved. We will refer to each of these relaxation solutions as an "iterative time step." By carrying out several iterative time steps at each "physical time step," ideal plastic flow may be approached. In the standard case two iterative time steps are carried out at each physical time step.

The main numerical features that change as smaller scales are simulated are the number of relaxation loops needed to solve the linearized momentum balance and the number of iterative time steps needed to fully attain plastic flow. To investigate the dependence of these features a series of simulations over a region 148 by 148 km with fixed land boundaries and fixed geostrophic wind in the x-direction.
Figure 2. Equilibrium velocity field for the standard case simulation.

Table 1. Numerical parameters used in standard simulations

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>$C_a$</td>
<td>0.0012</td>
</tr>
<tr>
<td>$\rho_a$</td>
<td>1.3 kg m$^{-3}$</td>
</tr>
<tr>
<td>$\Delta t$</td>
<td>3 hours</td>
</tr>
<tr>
<td>$U_g$</td>
<td>8 m s$^{-1}$</td>
</tr>
<tr>
<td>$C_w$</td>
<td>0.0055</td>
</tr>
<tr>
<td>$P^* = 5000$ N m$^{-1}$</td>
<td></td>
</tr>
<tr>
<td>$\xi_{\text{max}} = (5 \times 10^7 s) P^*$</td>
<td></td>
</tr>
<tr>
<td>$U_g$</td>
<td>0.0</td>
</tr>
<tr>
<td>$\epsilon$</td>
<td>2</td>
</tr>
<tr>
<td>$m = 0.91 \times 10^3$ kg m$^{-2}$</td>
<td></td>
</tr>
<tr>
<td>$\eta_{\text{max}} = \xi_{\text{max}} / \epsilon^2$</td>
<td></td>
</tr>
<tr>
<td>$U_w$</td>
<td>0.0</td>
</tr>
<tr>
<td>$f = 1.46 \times 10^{-4}$ s$^{-1}$</td>
<td></td>
</tr>
<tr>
<td>$\Delta x = \Delta y = 18.5$ km</td>
<td></td>
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<tr>
<td>$\phi = \theta = 25^\circ$</td>
<td></td>
</tr>
<tr>
<td>$V_w$</td>
<td>0.0</td>
</tr>
</tbody>
</table>

were carried out (see Figure 1). For later reference the standard simulation utilized the parameters shown in Table 1. The equilibrium velocity field for the standard simulation is shown in Figure 2.

a) Relaxation Convergence Rates

With respect to the convergence rate of the relaxation scheme, a series of numerical investigations verified the intuitive concept that the key parameter is $\xi_{\text{max}} / \Delta x^2$ where $\xi_{\text{max}}$ is proportional to $P^*$. To examine the rate of convergence, tests were performed with $\xi_{\text{max}}$ and $\Delta x$ varying separately. However, the physical size of the grid and external forcing were kept fixed. The momentum balance equation was integrated for up to 35 physical time steps with 2 iterative time steps per physical time step. Figure 3 shows the total number of iterations versus $\xi_{\text{max}} / \Delta x^2$ for the system to converge to a steady state. The reason for this
dependence is related to normal stability criteria for modeling viscous flow. Even though spatially varying viscosities together with overrelaxation techniques are employed, the maximum viscosity and grid size still dictate the degree of convergence during each relaxation loop. Basically, as the strength increases and the grid size decreases more relaxation loops are needed to solve the linearized momentum balance.

There are two main reasons that the scaling is not linear in Figure 3. One is that 100 overrelaxation loops are used in the relaxation scheme before converting back to straight relaxation (this is done for stability reasons since the overrelaxation can diverge). A second reason is that changing $\zeta_{\text{max}}$ and/or $\Delta x$ will alter somewhat the solution for the same physical region and forcing. The effect on the solution is illustrated in Figures 4 and 5 which show the equilibrium velocity at the center position of the grid for different values of $\zeta_{\text{max}}$ and $\Delta x$, respectively. The variations for different grid sizes are strictly numerical artifacts, whereas $\zeta_{\text{max}}$ variations are physical. Specifically, decreasing $\zeta_{\text{max}}$ reduces the degree to which rigid flow is being modelled. The basic conclusions Figures 4 and 5 yield are a) $\zeta_{\text{max}}$ could safely be reduced by an order of magnitude without drastically changing the results, and b) as long as at least six grid cells over a grid are used the results are not critically dependent on resolution. It should also be noted that while a fixed physical region and forcing are used these results can be scaled to other sizes using a dimensionless form of the spin-up results described next.
Figure 4. Equilibrium x-velocity component of center point versus $\zeta_{\text{max}}$ for fixed strength, fixed $\Delta x$ (= 18.5 km) and fixed external forcing.

Figure 5. Equilibrium x-velocity component of center point versus resolution for fixed strength, fixed $\zeta_{\text{max}}$ and fixed external forcing.

b) Plastic Adjustment Time

Analysis of a variety of numerical tests shows that the plastic spin-up times depend on the physical size of the basin, external forcing and ice strength. From dimensional analysis it can be deduced that the key dimensionless parameter is $P^* \tau_a \cdot L = 8$.

To examine the dependence on this parameter a series of tests were done with fixed $\zeta_{\text{max}}$ and $\Delta x$. In each test a different value of $P^*$ (fixed in time and
Figure 6. Total number of iterations to equilibrium as a function of ice strength.

space) was used. To verify the scaling, tests were also done with L and \(a\) varying, with similar results as long as \(\beta\) was fixed. (To obtain identical results the water drag and ice mass would also have to be modified.) Figure 6 shows the spin-up results in terms of the number of iterative time steps needed to attain plastic equilibrium flow for different values of \(P^*\). Figure 7, on the other hand, shows the actual evolution of the velocity field as a function of physical time steps. These figures also show the strength for which the system becomes rigid and does not flow at all. In these cases the external forcing is simply not large enough to exceed the plastic yield. The basic character of the results is that for very small \(\beta\) the adjustment is very rapid and essentially caused by the adjustment of the nonlinear water drag. As \(\beta\) increases (strength increases) the spin-up time to plastic flow increases. The main reason for this (as supported by Figure 3) is the increase in the maximum viscosity as \(P^*\) increases. Based on Figure 7 it is clear that in most cases 15 iterative time steps are quite adequate to fully attain plastic flow. If the numerical procedure is modified in this way plastic flow is essentially attained at each physical time step. It is notable that such a procedure will not necessarily greatly increase the computational time. This is because as equilibrium is approached only a few iterations are required to solve the momentum balance. As a consequence the longer iteration time will be reduced very rapidly if the system is close to plastic flow.
IV. APPLICATION EXAMPLE

To show the utility of this procedure for modeling rapid changes of ice velocity in forecast applications, a simulation with time-varying wind fields was carried out using the standard grid and strength parameters. Two simulations were made, one with 15 iterative time steps per physical time step and one with 2. Because of the similarity of the grid to the Bay of Bothnia, observed drift rates were compared with simulated drift of the center point of the grid. As can be seen from the results in Figure 8, the rapid adjustment time in the case with 15 iterative time steps per physical time step gives a sharper discontinuity in the ice velocity field, which is in quite good agreement with observation.

V. DISCUSSION

The purpose of this paper was to examine the numerical response characteristics of the momentum balance of a viscous plastic ice dynamics model. Emphasis has been placed on determining the parameters affecting the computational speed and the nature of the adjustment to fully plastic flow.

The results show that a particularly important parameter computationally is the maximum viscosity value. Very large values of this parameter simulate ideal rigid plastic flow quite well, but are computationally slow and require several iterative time steps to fully attain plastic flow. Very small values, on the other
Figure 8. Simulated and observed drift speeds (a) and direction (b) for a time-varying wind field. Simulated results for different numbers of iterative time steps (2 and 15) per physical time step are shown. In part (b) of the figure the wind direction is the direction towards which the wind is blowing with $0^\circ$ being north and $90^\circ$ being east.
hand, are computationally fast but effectively yield linear viscous flow. The results described here indicate, however, that it is possible to choose inter-
mediate values of this viscosity parameter that adequately simulate rigid behavior while still being computationally efficient.

With respect to plastic adjustment time, 15 iterative time steps are found to be adequate to model rapid changes in the ice velocity field. When used with a relaxation solution, this procedure does not necessarily greatly increase computational time since as plastic equilibrium is approached, only a few iterations are required to solve the momentum balance.

This study has emphasized the mesoscale response on time and space scales of 3 hours and 20 km. However, using the dimensional scaling noted in the text, the plots given in the paper should also be useful for choosing strength and viscosity parameters for different applications.

VI. ACKNOWLEDGEMENTS

We would like to thank Steve Bowen for valuable editorial aid on this manu-
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REFERENCES

DISCUSSION BY:
R.S. Pritchard, Flow Research Co., U.S.A.

Please comment on your reasons for selecting a viscous-plastic rather than elastic-plastic constitutive law. Also why did you use an implicit relaxation scheme rather than an explicit scheme. On this letter point, the fact that convergence is the relaxation scheme is like number of iterations appears proportional to \( \frac{\zeta_{\text{max}}}{\Delta x^2} \) which is just like the explicit scheme time steps limitation.
RESPONSE TO QUESTION

The viscous-plastic law is more physically realistic since it does not include elastic waves in the spin up process. This is especially important when one is trying to interpret the meaning of the simulation results. Also, the viscous-plastic constitutive law is a more general rheology. Specifically while an elliptical yield curve was used here you can easily consider a wide range of highly nonlinear rheologies without the necessity of an associated flow rule used in elastic plastic rheologies. Thirdly, one of us (Hibler) developed this approach to modeling sea ice dynamics and the purpose of this paper was to examine some of the detailed behavior of this rheology.

The main advantage of the implicit scheme is that it is computationally faster. On your latter point, you could, of course, solve the implicit equation at each time step by an explicit time integration procedure. We have experimented with this approach. This should not be confused with a totally explicit procedure which is much slower. However, the relaxation solution is particularly efficient when over relaxation techniques are employed. Also, for many purposes it is not necessary to totally model rigid plastic flow at each time step, and in such cases only a few iterative time steps at each physical time step are required.
NUMERICAL MODELING OF
LABRADOR PACK ICE DYNAMICS

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T.E. Keliher, Associate Professor (Research), Newfoundland Institute for Cold Ocean Science.

Abstract:

Sea ice conditions and dynamics will strongly affect oil and gas development, fisheries, and transportation in Newfoundland and Labrador waters [1]. At least one requirement will be for timely and accurate ice information to support the various marine operations. As in other areas, this will probably be provided by a combination of near real-time observations and forecast models. However, the ice dynamics in the Labrador offshore are very intense and the ocean plays a much more important role than in many other regions. Models developed for other regions are not able to accurately model the complex dynamics that occur in this region.

The pack consists of a high percentage of young and first year ice forms, with an admixture of old ice forms and icebergs advected into the Labrador flow from the northern regions. The dynamics are governed by intense atmospheric disturbance, a strong and unstable current regime, shoreline interaction, and processes at the ice edge. Some satellite imagery is shown to illustrate the complexity of the ice circulation features. Clearly shown are numerous mesoscale features including long waves, meanders, eddy structure and streamers. An analysis of wave-like features from LeBlond [2] is used to discuss this aspect of the modeling problem.

While none of the available ice models is considered entirely suited to the Labrador offshore, the model used by the Swedish Hydrological and Meteorological Institute [3] to forecast ice conditions in the Baltic has been adapted to run simulations. The model seems to
produce reasonable large scale results using atmospheric winds derived from surface pressure fields and available information on currents. Unfortunately, the information available to test any model is extremely limited. Experimental programs to improve our knowledge of Labrador ice dynamics are needed.

Introduction

The development of the enormous potential of Canada's eastern Arctic and subarctic regions will be strongly affected by sea-ice conditions along the coast of Labrador [1, 4]. This is true for all sectors of development—oil and gas, hydroelectric power, mining, fisheries, and transportation. These activities will have some common and some unique requirements for ice information. Recent reviews [5, 6, 7] document the operational requirements for ice information for various activities. Present knowledge of ice conditions, observational and modeling capability do not meet these requirements in this region.

The Labrador seasonal sea ice zone is unique in Canadian waters, and poses some particularly difficult modeling and forecasting problems. There are a number of reasons for this fact, but a few of the principal ones are:

i. The Labrador pack is very heterogeneous, a large percentage of weak young and first year ice, but with some older sea-ice and icebergs being advected into the region from the north.

ii. There is a strong ocean forced component to the ice motion which is poorly documented at this time. There are very few current measurements available during the ice season and these are well below the surface layer forcing the ice. Mesoscale features dominate the motion.

iii. Wind forcing is also strong, but synoptic meteorological data is scant, particularly over the ocean.

iv. The Labrador Sea provides an open boundary along the entire length of the ice zone. Waves from the North Atlantic penetrate the marginal ice zone breaking up the floes, and along with the winds and mesoscale circulation features, cause significant redistribution of the pack in this zone.
v. Thermodynamic forcing is strong, but poorly understood. Comprehensive reviews of the scientific and technical problems of the seasonal sea-ice zone have recently been made which provide useful background [8, 9].

The Labrador Seasonal Sea-Ice Zone

Ice conditions along the Labrador coast are discussed by Wright and Berenger [10] and Markham [11]. Labrador waters are inhabited by local young ice in the early winter, but an increasing proportion of the pack contains older forms advected from northern waters as the season advances. Terrington Basin and Lake Melville are the first areas along the central Labrador to freeze (late October to mid November), and are consistently ice covered by mid December when approximately 330 freezing degree days have accumulated at Goose Bay. Shortly after ice appears in the coastal waters and spreads south into the Straits of Belle Isle by early January, reaching White Bay and western Notre Dame Bay by the end of the month. The width of the ice zone continues to expand, reaching up to 500 km offshore in southern Labrador at the peak of the season. When the winds blow offshore, the ice edge may be very diffuse and difficult to define, but onshore winds compact the ice toward the coast.

Figure 1 shows the seasonal progression of ice types and concentration for southern Labrador according to Markham [11]. This is a composite picture representing the average conditions for a decade (1963-1973) at four locations. The dashed curve for old ice is an estimate based on very limited data. The amount of old ice is highly variable from season to season as the multi-year floes must drift into the Labrador circulation through Kane Basin, Jones Sound and Lancaster Sound. The seasonal ice pattern is strongly advective with southerly movements of 300 km per month. [10] shows the drift track of RAMS buoy placed on a multi-year floe in the 1977 ice season. Typical speeds averaged over a 4-8 hour time period ranged 0.05 to 0.50 ms⁻¹, but speeds as high as 1.5 ms⁻¹ were observed.

By mid February the ice thickness at Goose Bay and Cartwright has reached 60-75 cm. Offshore thickness data is scarce. However, using the average thickness associated with the ice classes in Figure 1 at the time of maximum concentration (mid February), the average thickness would be 40 cm which is obviously too low. The fact that the Labrador pack is so free to expand to the south and east indicates that
substantial amounts of new ice forms and is subsequently rapidly deformed into thicker categories under compacting conditions. Winsor and LeDrew [12] report ice thickness mostly less than 2 m off Sagleak and Hopedale in February. Wright and Berenger [10] report first year ice thicknesses measurements for April 1975, and February, March and April 1976–1977. The range of thickness for February (1976, 1977) was 0 - 2 m for 32 measurements. March and April (1975, 1976, 1977) values ranged up to 8 m (81 data points) with 47% of the measurements greater than 2 m in thickness. The authors point out that the data is biased toward higher values due to the requirements of the helicopter for safe landing. However, their results show a significant increase in maximum thickness for March and April over February which must be due to rafting and ridging, but also to the appearance of ice advected from the north. Extensive areas of highly deformed ice with thickness up to 11 m have been reported (Wright [13], Allen [14]). A substantial amount of the thick ice is unconsolidated.

Monthly snowfall averages about 60 cm during February, March and April for three stations (Battle Harbor, Cartwright and Hopedale). Much of this must be incorporated into the ice or blow into the ocean since only 20 cm has been reported on the ice [12]. Weeks and Lee [15] report three distinct layers in level ice at Hopedale. The top layer is snow, the middle layer is snow ice (frozen slush) and the bottom layer is sea ice.

Floe size and shape distributions are poorly known and probably quite variable. Winsor and LeDrew [12] report floe sizes between 0 - 75 m with mean values between 7.5 to 30 m determined from aerial photographs of the pack off Sagleak. Certainly, much larger floes are found, but in general, small floes predominate. For comparison, Lapparanta [16] reports a mean floe size of 1.46 km for the characteristic flow diameter near his 1978 field site in Bothnian Bay (Baltic).

Requirements for Modeling the Labrador Pack

Before discussing numerical modeling of the Labrador pack, we should look at the modeling requirements. Of course, there are many points of view from strictly basic scientific studies to strictly operational forecasts of ice conditions. Because of the nature of this POAC conference let us focus on the operational requirements. The nature of the ice information required will vary from one activity
to another. However, in a recent meeting discussing the mission requirements in the RADARSAT program, it became quite clear that certain operational requirements exceed the resolution of present day satellite borne sensors [17].

Ref [2] specifies the operational ice data required in support of marine transportation, resource development and exploitation, and provides some guidance on modeling requirements. Three distinct requirements for ice information are identified.

i. Planning information: required for pre-season planning. This is statistical and time series data derived from a strategic data base. 10-20 km accuracy is required on a 1-14 day frequency over the entire area of interest.

ii. Strategic information: required for short term planning for voyages and ice forecasting. 500 m accuracy is required on a 6-24 hour frequency.

iii. Tactical information: required for site specific support of a vessel or platform. 500 m accuracy is specified on a near continuous real time basis.

Specific information required is extent of ice coverage: type, size and thickness of ice; distribution and height of ridges; snow cover; location, extent and size of leads; location and drift of icebergs, bergy bits, and growlers; location and size of polynyas. Obviously models will never satisfy most of these requirements but when used in conjunction with remote sensing and a good surface observational program, they will provide an important input - particularly to the strategic and tactical decisions. For most operational requirements, the model will be used to extend observed ice conditions in space and time. It will only be one of the tools available to the forecaster; aircraft, satellite and surface based observations will normally also be available. However, off Labrador during the ice season a persistent cloud cover will severely restrict aircraft and satellite observations with the exception of microwave systems. Wadhams [17] suggests that a model of the seasonal ice zone will probably have one of four aims.

i. to develop a regional model of a whole drift system (i.e. the Labrador current);

ii. to predict small scale motions of the ice edge for the immediate use of clients such as oil drilling ships, cargo or fishing vessels;

iii. to predict mesoscale motions of the ice margin, e.g. sea-
sonal or annual changes of the ice limits;
iv. to predict the large scale seasonal advance and retreat of ice cover for coupling with atmospheric models.
He indicates that aim (ii) can probably be best met with on site "rule of thumb" models, and that aim (iv) is the province of large scale models which will likely not be affected by the small and mesoscale properties of this ice zone. The ice properties and processes will be most important in meeting aims (i) and (iii) of the modeling activity.

Modeling the Labrador Seasonal Sea Ice Zone

A number of numerical and empirical models have been developed to study and forecast ice conditions and dynamics. Hibler [19,20] has recently published a complete review of numerical modeling of ice dynamics. In general, modern numerical models consist of several elements:

i. a statement of momentum balance including the forces acting on the ice (air and water stress, Coriolis force and internal ice forces). Off Labrador it may be necessary to add the wave induced forces at the ice edge;

ii. a relationship between the ice stress and the ice strength and deformation (the reology);

iii. an ice thickness distribution equation which describes the ice thickness changes due to thermodynamic and dynamic effects and along with concentration determine the ice strength in (ii). Growth and decay of the ice is normally computed by separate code and used to modify the thickness distribution.

The selection of space and time scales on which the model is to operate is very important. Solving the momentum equation, it is assumed that the ice behaves as a continuum, at least in a statistical sense at sufficiently large space and times scales. Hibler [19] indicates that this is satisfied for scales larger than 20 km. Udin and Ullerstig [3] suggest that a value of 10 km is appropriate for Bothnian Bay.

Figure 2 is a NOAA 5 image of the Labrador pack for 27 March 1978, which shows the nature of the ice regime to be modeled in this region. This image clearly shows the extent of the pack, confined by the strong advective regime along the coast. Numerous mesoscale features can be seen in the ice zone and the the long waves, eddy and streamer features can be seen. LeBlond [2] has analyzed the wave-like
features in the ice margin using four sequential daily NOAA 5 satellite visual images 8-11 April 1977 and finds horizontal oscillations traveling downstream, with the Labrador Current, at speeds of approximately 0.2 m⁻¹. These features have a wavelength of 75 km and an amplitude of 15 km.

Wadhams [17] discussed the characteristics of the marginal sea ice zone (MIZ) and points out several properties of this zone which a modeler may have to take into account. These include:

1. wave breakup and floe size distribution;
2. lateral melting and wave-induced melting;
3. the effect of rapid ice production or melting on the mixed layer;
4. frontal processes;
5. internal stress different than in the central pack.

Undoubtedly, it will be some time before we have sufficient information on these edge processes to effectively model them. However, several recent field research programs have been specifically focused on the MIZ. A review of recent and planned research was discussed in a MIZEX Workshop [9].

The initial goal of our work is to provide operationally useful results, so we have selected the Udin and Ullestig [3] model for initial testing off Labrador since it is used routinely in operational forecasts in the Baltic. Also the ice conditions in the Baltic are similar in many important ways to the Labrador pack; thickness, age, area of coverage, concentration, continuity scale, and possibly strength, but probably not roughness. Because of limited space, only a brief description of the model is provided. Interested readers are referred to the complete description provided by the developers [3].

Briefly, the model consists of a momentum balance between the wind stress, water stress, Coriolis force and internal ice stress. The acceleration term is neglected as it is small in comparison to these terms for time scales longer than a few hours. The wind and water stress are both parameterized on the basis of a logarithmic surface layer (exchange coefficient a linear function of vertical coordinate) followed by an Ekman layer (constant exchange coefficient). Ice mass is handled by the average concentration (0-1) and the average thickness within a unit area. Thickness changes are computed from the convergence of the concentration field for concentrations approaching unity. Thermodynamic effects are neglected over the short forecast
period of a few days. Internal friction is parameterized as a combination of viscous and plastic terms. The plastic term is taken as proportional to the driving force as a function of the concentration and thickness. Fast ice areas are variable and depends on the ice thickness, ice concentration and wind. Numerical computations are carried out over a grid (10' of latitude by 20' of longitude), using a staggered grid to treat the boundary condition. The model is driven by the geostrophic wind obtained at 23 grid points.

The grid used off Labrador is shown in Figure 3. Two aspects of the region require major modifications in the model. The first is the existence of a strong current flowing southeast along the Labrador coast. The water no longer acts simply as a purely passive retarding medium to the ice. Unfortunately, as indicated previously, there is almost no direct information available on the near surface currents of Labrador during the ice season [22]. For lack of better information, we have elected to use climatological surface current estimates derived for oil spill modeling [23]. A second aspect requiring modifications is the fact that ice extends beyond the boundaries of the grid. Therefore advection of ice in and out of these boundaries is allowed. Ice is advected in at the north boundary and advected out at the southern boundary.

So far, the model has only been run for a three-day period in February 1980. During this period the winds are predominantly from the northwest and the ice motion is along the coast to the southeast. The initial ice conditions are derived from the weekly ice charts made available from the Canadian Ice Forecasting Central, using ice thicknesses associated with the various ice classifications. The fast ice limits are input in a rather subjective fashion at the present time. Geostrophic winds are derived by hand from the 6-hourly surface pressure analysis fields provided by the Atmosphere Environment Service. However, the wind is not allowed to vary spatially over the grid, a uniform wind estimated at the middle of the grid is used in the model results presented in this paper.

Figure 4 shows the initial ice concentration field derived from ice charts for 21 February 1980 subjectively extended north of 55°N using satellite photos, the model results after 72 hours of simulation and the ice chart concentration reported for 24 February 1980.

The model results indicate that the concentration in the southern portion of the grid decreases and the ice margin has moved east to
52°W from its initial position at 53°20'W. The high nearshore concentration initially reported has decreased and a shorelead has developed off northern Labrador. Offshore in that region an irregular pattern of concentration has developed from the initial nearly uniform conditions, probably due to the current pattern assumed in that region. The ice chart reported conditions for 24 February 1980 show a more uniform concentration which consists of thicker ice. Air temperatures reported from coastal stations and ships during this period were between -10 and -20°C which may account for the increased thickness reported in the ice chart. Since the model has no thermodynamic code at the present time, this growth would not be found in the model results. The general comparison of the ice margin between the model and the ice chart shows favorable agreement. The differences in the shape of the ice margin could easily be due to the lack of spatially varying wind, specifications of currents, or mesoscale processes which are not modelled.

Figure 5 shows the initial ice thickness distribution derived from ice charts for 21 February 1980; and the model results after a 72-hour period and the corresponding ice chart thickness for 24 February 1980. Again, the thickness data for the northern part of the area is derived from satellite data. The model results show thicker ice developing in the southern region which, due to the lack of inclusion of thermodynamic effects in the model, would have been induced by the generally northwesterly winds during this period. The winds were southwesterly for the first 12 hours which created some loose ice conditions which then compacted in the area around 56°N while keeping loose ice in the very southern portion of the area. The analysis of 24 February 1980 indicates that this is roughly the ice situation on that date. More testing is required before we can feel comfortable with our "tuning" and input parameters. However, we are confident that the model will perform as well as any presently available model in this region. Furthermore, given the lack of quantitative information on the Labrador pack, there would be little basis for selecting another model or significantly changing this one.

The above are only preliminary results but the model simulations are in general agreement with reported ice data. The concentrations, thickness and implied drift are within reasonable limits considering the nature of the forcing inputs.
Summary and Conclusion

Forecasting ice conditions for marine operations off Labrador is a very difficult problem. A model developed for the Baltic has been adapted to the Labrador offshore and seems to give reasonable initial results. However, the lack of observational data off Labrador makes it impossible to validate this or any other model. Two major deficiencies are identified in input to the model. These are information on the ocean currents and thermodynamic processes.

It is essential that studies of pack ice dynamics be undertaken as soon as possible to answer the fundamental questions regarding processes in the Labrador offshore. A comprehensive integrated program such as the Labrador Ice Dynamics Experiment (LIDEX) is considered necessary to meet this need.

Future Research

Our research is proceeding along several lines.

i. We are currently preparing to run the Udin and Ullestig model over the four-day period used by LeBlond [2] in his analysis of edge wave phenomena (8-11 April 1977). This period is one of the few we have found with sequential satellite imagery having acceptable cloud cover, suitable atmospheric surface pressure fields, and good ice deformation.

ii. A thermodynamic model [24] is being prepared to incorporate ice production and decay into our computations.

iii. Planning for a comprehensive study of ice dynamics and related processes (LIDEX) is continuing. The program of research will take place over several years and have to involve a number of investigators with various interests (ice dynamics, meteorology, and oceanography). A pilot program is planned for March 1982 in conjunction with RADARSAT sensor development overflights. The LIDEX program is described in detail [25].

References


FIGURE 1  Average sea ice type composition and concentration off Southern Labrador.
(From Markham [11])

FIGURE 2  NOAA 5 Satellite image of the Labrador pack for 27 March 1978

FIGURE 3  Grid used in sea ice dynamics model off Labrador

FIGURE 4  Ice concentrations in percentages
a) Initial concentrations based upon the ice map for 21 February 1981, b) Model results for 72 hours later, c) Ice concentration from the ice map for 24 February 1981.

FIGURE 5  Ice thicknesses in centimeters
a) Initial thicknesses based upon the ice map for 21 February 1981, b) Model results for 72 hours later, c) Ice thicknesses from the ice map for 24 February 1981.
FIGURE I

Southern Labrador

- White or First Year Ice
- Grey-White
- Grey
- New
- Mean Concentration
- Estimate
- Old Ice

CONCENTRATION (IN TERTHS)

CONSTITUENT ASES (%)
SEA ICE RUBBLE FORMATIONS OFF THE NORTHEAST BERING SEA AND NORTON SOUND COASTS OF ALASKA

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Introduction

The presence of large ice features in the Bering Sea off Alaska was first recorded in the 1800s by seamen navigating these waters. The size of these formations led the sailors to believe that they were icebergs (W.F. Dehn, personal communication). A photograph of such a formation, taken shortly after the turn of the century, shows how impressive they can be (Figure 1). We now know that the features were large sea ice pressure ridge fields or fragments. These ice rubble formations may form at sea, or may develop on shoals or near beaches during ice movement andpressuring events.

Kovacs [6] reported on very large floating ice rubble fields in the waters north of the Bering Strait, at the latitude of the Arctic Circle, and islands or ramparts of pressure ice 14 m high grounded on the west side of Prince of Wales Shoal. Large

Figure 1. Photograph of Bering Sea pressure ridge taken in early 1900's. The formation was said to be over 12 m high. Note the absence of voids and the dirty appearance of the ice. Photo courtesy of G.K. Sherrod, Nome Museum.
grounded pressure ridge formations have been observed in Landsat imagery off the Yukon Delta [4, 15, 18]. Hunter et al. [5] and Thor et al. [19, 20, 21] noted that the sea floor in Norton Sound and along the northeastern Bering Sea Coast is highly scarred by ice keels pushed about during winter ice movement or during breakup, when the grounded ice formations or fragments of them are at first set adrift and then recontact the seabed.

The existence of large sea ice formations in the northern Bering Sea and Norton Sound area is of concern today, as these features may represent the severest ice conditions that an offshore structure placed in these waters would have to resist. In addition, the deep keels of the ice formations would represent a threat to bottom-founded structures should contact occur.

A reconnaissance was made along the northeast Alaska Bering Sea Coast and the coast of Norton Sound in April 1980 to establish the locations where large sea ice pressure ridge formations occur and to determine their general characteristics. The locations of shore ice pile-up and ride-up events were also documented, as these phenomena also pose a threat to shoreline facilities.

**Field Observations**

On 10 April a reconnaissance flight was made over the Alaska Bering Sea - Norton Sound area (see Figure 2). From Nome to the Yukon River Delta we flew over loose pack ice. About 5 km out from the fast ice surrounding the Yukon Delta there was an open water area, in the center of which (position A in Fig. 2) were several large grounded ice formations (Figure 3). Bathymetric maps show shoals in this area which rise to within 5 m of the surface.

Along the fast ice edge, large grounded ice rubble formations were observed. These formations were typically 200 to 1000 m long and 50 to 300 m wide. They tended to be isolated features, separated from one another by many kilometers of level or rubble ice of low relief. Further inland from the fast ice edge more ice pile-ups existed which were smaller in area. These were also of considerable height but were often found to be surrounded by undeformed sea ice. Virtually all the ice formations observed were formed by ice piling up under compressing forces and not as a result of shear deformation, for example along the fast ice edge. Ridge orientation within the ice rubble fields and ice pile-ups on shore indicated that these formations occurred during ice movement from virtually every compass quadrant.

The largest concentration of grounded pressure ridges in Norton Sound was observed off the Yukon River Delta between positions B and C in Figure 2. A number of these ice formations were visited to determine their relative height, the thickness of their ice blocks, and local water depth.

The first ice rubble pile visited is shown in Figure 4. The fast ice was in contact with this formation only at the southeastern end. Drift ice along the north
Figure 2. Area map.

Figure 3. Ice ridge formations believed grounded on ribbon-like shoals shown to exist off the Yukon River Delta by Dupre [4]. Fragments of broken ice exist along fast ice edge at bottom of photograph. Top of photograph is north.
Figure 4. Grounded ice formation one.

(a) Aerial view.
(b) Ground view (arrow points to man).
(c) Close-up view showing ice block thickness variation and dirt incorporated in the ice rubble.
side was estimated to be moving westward at 3 to 4 knots. The formation was estimated to be 75 m wide and over 250 m long. It consisted of ice blocks typically 35 cm thick, but many ice blocks over 1 m thick were noted (Figure 4c). The thicker ice consisted of previously ridged and rafted ice fragments. The ice formation was grounded in 4.1-m-deep water and reached a height of over 14 m.

Ice formation two (Figure 5) was grounded in 6.0-m-deep water and was 12 m high.

Ice formation three (Figure 6) was situated in water 4.2 m deep and was 14 m high.

Ice formation four (Figure 7) comprised ice blocks 25-30 cm thick; it had a ridge height of 13 m and was grounded in 7.5-m-deep water.

Ice formation five (Figure 8) consisted of ice blocks 30-35 cm thick. It was estimated to be 75 m wide and 200 m long. The vertical ice sheet behind the
observer extended 3.8 m above the 13-m-high ice rubble at its base. The water depth beside the ridge was 3.1 m.

Ice formation six (Figure 9) was also estimated to be 75 m by 200 m. It was 14 m high and was grounded in 6.2 m of water.

A wide ice formation was observed near site A in Figure 2. Its size and location helped to fix the northwesternmost position of the fast ice at the time of our
Figure 9. Grounded ice formation six.

Figure 10. Northwestern edge of fast ice off the Yukon River Delta. The ice formation shown closest to the fast ice edge in Figure 3 is located to the center right.

reconnaissance (Figure 10). Two soundings taken 200 m apart alongside the ice formation gave water depths of 9.7 and 10.0 m. The ice blocks in the formation varied from 30 to 40 cm thick and were pushed into ridges up to 12 m high.

Ice formation seven is shown in Figure 11. The average ridge height in this formation was estimated to be between 5 and 6 m. Most of the ice blocks were 30 to 45 cm thick. Some, from pressure ridges or previously rafted ice, were over 1 m thick. The highest ridge was 17 m high. A sounding taken beside the ridge gave a water depth of 11.2 m.

Ice formation eight (Figure 12) was grounded in 3.0-m-deep water. It was approximately 150 m by 200 m in size and had a peak ridge elevation of 11.5 m.

Ice formation nine (Figure 13) consisted of ice blocks up to 2 m thick and was grounded in water 8.9 m deep. The two highest ridges in the formations were over 11 m high.

Ice formation ten (Figure 14) was grounded in 4.5-m-deep water. Ice blocks in this formation varied from 40 cm to 2 m in thickness and formed ridges up to 9 m high.

Ice formation eleven (Figure 15) was grounded in 8.5-m-deep water near position C in Figure 2. This feature was 9 m high. Collapse of a portion of the ridge revealed a core of fragmented ice blocks refrozen into a solid mass (Figure 15b). On the left side of the ice formation the 30-cm-thick ice sheet from which the pressure ridge formed could be seen (Figure 15c).
We also observed grounded ice formations in fairly sheltered locations, such as one over 3.5 m high in 3.9 m of water near the village of Stebbins (Figure 16). This ridge was composed of ice blocks 10 to 15 cm thick. In another somewhat sheltered area off the southeast coast of Isaacs Pt. (Figure 2) four grounded ice formations were observed, each 4 m or more in height. Three of these formations are shown in Figure 17. The nearest one was grounded in 2.6 m of water.

Large grounded ice formations were also observed paralleling the 10-m depth contour from Cape Denbigh to Pt. Dexter, off the coast of Cape Darby, Rocky Point, Cape Nome, and the south side of Sledge Island, off Port Clarence spit (ice piled 5 m high in 6.3 m of water) and along the coast between Cape York and Cape Prince of Wales.
Figure 12. Grounded ice formation eight. Note ice rubble and dirt.

Figure 13. Grounded ice formation nine.

Figure 14. Grounded ice formation ten (at extreme right).
a. Aerial view.

b. Refrozen fragmented ice in ridge core.

c. Portion of original ice sheet.

Figure 15. Grounded ice formation eleven.
Figure 16. Grounded ice formation near Stebbins. The village is along the coastline at upper left.

Figure 17. One of four grounded ice formations near Isaacs Pt. Isaacs Pt. is on the extreme left behind the near ground ridge formations. Two other formations are shown in the distance at the right in the photo.

Discussion

Ample evidence has been presented that shows that many massive grounded ice formations can be found off the Norton Sound and northeastern Bering Sea coasts of Alaska. Many of these features have been observed to have relatively solid cores. This is a result of the freezing of water within the ridge voids. The source of this water is an occasional rainfall and surface snow or ice that melts during midwinter thaws. Both occurred in the Norton Sound region during the winter of 1979-80. The solid cores are also the result of pressure consolidation during ridging.

Most of the grounded ice formations were found to be quite dirty. Some of this material was incorporated into the ice during growth, either by the freezing of
silt-laden water or by the incorporation of dirt-laden frazil ice into the growing ice sheet. Drake et al. [3] have shown that during the winter significant suspended sediment exists in the water beneath the ice in the Yukon River Delta region. However, most of the dirt observed in the ice rubble was the direct result of ice interaction with the sea floor. During ridge-building, portions or fragments of the ice sheet were forced downward against the seabed and then pushed back upward, bringing sediment with them to all parts of the rubble pile. Through this action sediment is gouged up and redistributed by ice or current transport.

The sequence of events associated with the formation of the isolated sea ice rubble formations surrounded by undeformed ice is as follows. A major ice movement event occurs, perhaps in conjunction with a rise in sea level. Fragments of the moving ice sheet come in contact with the seabed, initiating further ice-bed and ice-ice interactions. A large accumulation of pressured ice forms at this site of resistance. With time, under the driving force of wind or current, the remaining drift ice is advected out of the area, leaving behind the grounded ice formations. In time the surrounding waters refreeze. The resulting ice sheet is more resistant to movement, being held fast by the grounded ice. The grounded ice helps to stabilize the fast ice by providing anchorage to the sea floor and allows the fast ice to extend farther off the coast into deeper water than would otherwise be possible. In this respect, the grounded ice formations along the Yukon Delta do what the large ice formations in the grounded ice zone along the Beaufort Sea coast have been shown to do [7, 8].

The lateral confining support provided by a stationary ice field is of course lost during the time an ice rubble formation is exposed to open water or loose drift ice. When this occurs, the exposed outer perimeter of the ice rubble formation undoubtedly calves off. The remaining keel slope is probably similar to the 50° to 90° keel slopes measured on ice formations that had experienced similar loss [6, 7]. For grounded ice formations, that do not experience calving, the keel slope angles are probably similar to those of first year pressure ridges, which average 33° [8]. The interior ridge slopes of the ice rubble formations were not measured, but these too are believed similar to those of first-year pressure ridges, which average about 24° [8].

Other processes associated with ice rubble formations, pressure ridges and pile-ups are beyond the scope of the report. However, models have been developed to describe the force levels and other formation processes associated with the building of these ice formations [1, 9, 10, 11, 12, 13, 14, 17].

A fundamental question for those concerned with the design of offshore structures is: Do these massive ice formations lift off the seabed during high storm surge events and then drift about?
During this study one ice formation (Figure 18) was observed from the fast ice edge, east of position A in Figure 2, moving west. An ice formation (Figure 19) observed from the air appeared to be sailing westward through thin ice west of Rocky Point. However, this was not verified by observations from the fast ice as a landing could not be made.

The following personal accounts of large ice formations sailing about are of interest. In the spring of 1973, while hunting on the sea ice southwest of Cape Wooley, John I. Pullock, a King Islander, encountered an ice formation which he estimated to be 7 m high, 30 m wide and perhaps 100 m long. It was moving from west to east under the driving force of the current as the winds were from a different direction. As the ice formation moved it fractured both thick and thin ice in its path. He and other hunters on the ice had to move toward shore in order to get out of its way.
In May 1973 Ralph Olanna of Nome, his father and his uncle were returning from the west around Sledge Island (see Figure 2). They were in a 5.5-m-long aluminum boat and were trying to get to Nome, as a storm was building fast. About 2 to 4 km offshore, somewhat west of Nome, strong winds and high waves prevented them from proceeding. They were forced to pull their boat up onto a large ice formation, which Ralph estimated was 9 m high and 40 m in diameter. The ocean swells kept getting bigger, reaching 5 to 6 m from trough to crest. The three men had to pull their boat higher and higher, nearly to the top of the ice formation, to prevent it from being washed away. Parts of the ice rubble broke off from time to time under the beating from the waves. Four days later, after drifting about 130 km eastward, they were picked up by a Japanese research vessel about 50 km south of Rocky Point. This vessel had been forced into Norton Sound by ice movement during the storm.

The observations of early sailors coupled with the above accounts clearly indicate that large sea ice rubble formations can be found drifting in the waters of the Bering Sea and Norton Sound. Since extreme storm surges in Norton Sound are of the order of 5 m, while surges of 2 to 3 m are not uncommon [2, 16], it is reasonable to assume that some grounded sea ice rubble formations can be lifted off the seabed and set adrift during high sea rises.

Acknowledgments

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Ice in the sea is one of the facts of life for mariners in Eastern Canada, and the Federal government has for many years provided support to winter shipping in the form of escort by Coast Guard icebreakers. These are, in turn, supported by an ice reconnaissance and forecasting program operated by the Atmospheric Environment Service. Since the mid-1970s, offshore petroleum exploration has brought added pressures to bear on the provision and analysis of sea ice data as it relates to major development projects such as Arctic pipelines, year-round Arctic shipping and offshore oil production from the area east of Newfoundland. With these major developments pending, a significant change in the existing ice services program will be required during the 1980s. In Labrador and Newfoundland waters, icebergs assume greater importance for they present a threat to fixed platforms even greater than to ships moving through ice-laden waters. In the north, winter shipping is more difficult by several orders of magnitude than the usual summer resupply and survey operations. The projected changes are what I wish to focus attention on, but before going into details of the additions to the service, it is appropriate to first establish the present level of activity.

The program carried out by the Atmospheric Environment Service Ice Branch is fairly well known to some of the agencies and companies represented at this conference, but for the others, I will run over an outline of the program as it affects large deep-draft vessels in confined ice-covered waterways.

To provide a complete picture, I should really include areas such as the St. Lawrence River from the mouth of the Saguenay to Montreal, for it qualifies as a confined ice-covered waterway from January until early April. The fact that it is a river where water currents have a great bearing on the navigation conditions is the reason I have chosen to exclude it. Besides, any ship which can cope with the ice in Arctic waterways would have little difficulty with this river ice.
Our ice program has been in operation in northern waters since the late 1950s, primarily as support to the resupply operation of the DEWline and the various bases in the Arctic Archipelago. Reconnaissance, traditionally, begins with general surveys in May and June and then supports specific marine operations from July through September.

Initially, visual aerial reconnaissance was provided, but since the development of medium-resolution weather satellite in polar orbits, these too have provided very useful data. LANDSAT, in 1973, provided a higher-resolution coverage, and even though it was cyclical in nature, some extremely useful data were provided.

The basic AES program uses two long-range reconnaissance aircraft from July to October, both flying 3-4 days a week for a total of about 1500 hours during the summer. Frobisher, Resolute and Inuvik are the primary bases, and from these, coverage of the entire area can be provided.

It is obvious that there is a plentiful bank of data covering the Beaufort Sea, Baffin Bay, and the Resolute-Eureka area, but gaps in coverage are very apparent in areas such as M'Cluer Strait, western Viscount Melville Sound, and the waters around King Christian and Lougheed Islands. Fortunately, the reconnaissance program of the Polar Continental Shelf Project from 1961-1978 filled some of this gap, but even so, ice behavior in these less-frequented areas is just recently being studied in depth, and an understanding is emerging.

As I said earlier, most of the reconnaissance in the 1960s and a good part of the 1970s was by visual means, and that in itself creates a few problems. Low cloud and fog are common over Arctic waters during the summer and snow flurries become prevalent during the shortened days in the fall. As a result, the data coverage tapers off in October and terminates with the advent of the winter dark period.

In 1978, as a result of negotiations with the Department of National Defence, a side-looking airborne radar (SLAR) was made available for one of the reconnaissance aircraft. This provided a day-night all-weather capability that has also allowed Arctic winter reconnaissance, and provided a documented indication of the distribution of old as opposed to first-year ice.

As it stands now, the AES ice program provides annual winter probes into the Northwest Passage, and good coverage of the area from April to late October. We are limited by the absence of SLAR on the second aircraft - there is no backup capability in the event of unserviceable equipment, and of course the program is expensive and an economic ceiling is always present.

Now turning to the future - what needs to be done, and what changes should be made to the program? In my opinion, even though the needs are varied, and some of them are pressing, they can all be solved by transfusions of person-years and
dollars. I intend to deal with these in an order related to acquisition and use rather than on a priority basis.

The data acquisition phase of the Canadian ice program has always been a strong part of the activity, but even so, there are major requirements. Dependence on a single ten-year-old SLAR is unrealistic; both the present aircraft should be fitted with modern SLARs to provide back-up and flexibility. Purchase price would be in the 2-3 million-dollar range.

In order to address the major data gaps, a third aircraft is needed which could provide Arctic winter reconnaissance along the Northwest Passage after the present program needs have ended, then focus its attention on icebergs from Davis Strait to Newfoundland during the summer season. This would make a year-round task and would fully utilize the third reconnaissance platform. It should clearly be fitted out in the same way as the present aircraft so that interchanges can be made.

Data processing - combining aerial data with satellite imagery and improved ice forecasts - is the next logical step to be considered. In the first regard, the program is running well, for input from Toronto, Edmonton, and Søndre Strømfjord satellite receiving stations is already available, and an automated image interpretation system is under development. Today's satellites are weather-restricted and are color blind, so they represent a secondary input which supports the aerial program.

Improved forecasting methods have been worked on for some time - the AIDJEX program in the early '70s, for example, and various regional and small-scale model developments by AES Met Services Research Branch in more recent years. All these require input of oceanographic parameters such as water drag, residual and tidal currents, etc., so the problem is still far from a complete solution. Progress is being made, however, and except for certain areas of high-current velocity, initial models delivering reasonable solutions should be forthcoming within a year or so.

Data delivery to ships at sea is at present one of the weak links in the AES system. The Naval transmitter in CFH covers the Eastern Seaboard and Baffin Bay with reasonable success, but time is available for only five charts per day. In the Arctic, relay via ANIK to Resolute and Frobisher is required before the regional broadcast. These stations are low in power, and the coverage must be increased. When one considers the data needs of very large bulk carriers in the Northwest Passage, it is clear that a dedicated channel is required. Charts provided should probably be at a scale of 1:1,000,000 - four times as large as at present, but direct relay from Ottawa to a good northern broadcasting site will be required.

Direct relay from the reconnaissance aircraft to ships in the area is a
routine procedure at present, but these are the combined visual and sensor charts in common use. Data linking to relay the SLAR image as it is recorded in the aircraft is another possibility, but there are many problems. Present technology requires one hour to relay one hour of flight data, and this places an extreme burden on the aircraft equipment. There seems to be a need for this type of relay (with some data compression technique added) for the area west of Resolute where old ice, large floes, and little ice motion is the rule. In areas such as Lancaster Sound and Baffin Bay where the ice is more mobile, the SLAR image loses its validity within a few hours, and the conventional ice charts with added iceberg data will probably be very adequate. This matter is far from decided, for "deliverables" must be matched with data needs.

There are a few special systems and devices on the horizon which may be developed in the next five years or so. One of these is TELIDON which may be extremely useful for data delivery from Ottawa to the ships. This is a rapidly developing field, and just how (or when) it can be put to use is rather uncertain at this time. Another is the use of radar transponders on particularly large and thick ice floes. By marking such navigational hazards, penetration of the more difficult ice areas should become significantly less difficult. Besides the benefits for the ships concerned, such devices will provide excellent data on rates of ice motion on the broad scale from Parry Channel down through Baffin Bay to the Labrador area.

Ice thickness sensors would obviously be of great value if they can provide data concerning the ice in the path of a vessel, but there are many technological problems, and in the coming ten years, I suspect they will remain as helicopter-borne - that is, low-level, low-speed sensors.

As a final item, there is a passive microwave sensor. Such equipment is being studied by the US Navy and may become operational in the mid-1980s. This sensor will pick out old ice easily, and although the lateral coverage it provides is not great, the system could have the capability of sweeping the area ahead of a ship to detect isolated old floes and iceberg fragments that could cause structural damage if encountered in otherwise open water. MV Arctic had an experience of this nature in Baffin Bay three years ago and was unaware of the damage for several hours.

Before summing up, I think it is pertinent to point out some of the unresolved problems that are already apparent. The first is the lack of hangars in the Canadian Arctic. It is quite obvious that, if aircraft are to operate in winter in the Parry Channel and/or the Beaufort Sea area in winter, it is impractical to expose them to cold-soaking at -40°C during overnight stops. Ground crews and the aircraft themselves can only take a certain amount of this, and a solution must
be found - at Resolute particularly.

The second item is of equal importance, but is one which can be more easily resolved. I refer to the question of a ship control office belonging to the company operating the vessels and its essential liaison with the government's ice office. Modern communications will easily resolve the problem, but there are many details of data exchange to be worked out.

To sum up - by the middle 1980s, we should have year-round ice reconnaissance in the Northwest Passage, and an iceberg surveillance system from Davis Strait to Newfoundland conducted by SLAR-equipped aircraft. Direct delivery of pertinent imagery will be possible and relay of ice charts combining satellite and aerial data along with forecasts of leads, convergence and ice motion from Ice Central, Ottawa, will be dependable aids to ship captains and company operations offices wherever they are located. In the 1990s, we may have radar-carrying satellites, but the basic support system may not be very much changed. Space-borne radar requires very sophisticated processing before operational use and the concept of specialized support from a central office is very likely to remain.
Environmental Data Requirements for a Real Time
Iceberg Motion Model

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ABSTRACT. Motion data for more than 250 icebergs from two sites in the Labrador Sea has been analysed. A principal method in the analysis has been the use of a computer program which recreates the simultaneous dynamic motions of all the bergs at a given site for a given period of time. This enables us to address questions such as: To what extent are the motions of bergs which are present at the same time and close together highly correlated? As the distance between bergs increases, what is the rate at which the motion correlations fall off? To what extent do shape and/or gross size influence motions? What environmental data is required for the prediction of iceberg motions, and what are the appropriate space scales on which the data should be collected. Partial answers to these and other questions are given.
1. Introduction

Iceberg motions at two well-sites southeast of Makkovik Bank in the Labrador Sea are analysed for coherence. The data sets, consisting of radar ranges and bearings, were collected by Total Eastcan during drilling operations in the summers of 1974 (Pelican) and 1979 (Petrel). (Exact locations and bathymetric data are detailed in [1]).

Linear interpolation of the raw data produced interpolated files of hourly positions, yielding 19,000 hourly berg velocities of which 70% were based on non-interpolated observations. Limitations on available environmental data require this study to focus primarily on the berg motion data. We conclude that on short time scales iceberg motions may be reliably forecast; that the current regime is the dominant influence on the variability of iceberg motions; that the atmosphere's effect on berg motion is inadequately explained by wind driven surface currents; and that forecasting berg motions for longer time periods requires precise information on spatial shear in the current regime.

2. Average Berg Motion

The vector average $V$ of all hourly velocities at the Pelican site is southeasterly with magnitude .1 kts; at the Petrel site, south by southeast with magnitude .04 kts. While this net southerly transport of bergs parallel to the coast is expected, the low value of the magnitude of $V$ is not, and is accounted for by the many instances of northerly motions. In fact, Murray [6] reports average speeds of from 6 to 10 nm/24h along the Labrador coast, and Robe et al. [8] report average daily speeds of .3 kts for instrumented bergs in the area of Baffin Island and the Coast of Labrador.

On the other hand, at the Pelican and Petrel sites respectively, the average hourly speeds were .39 kts and .35 kts (with standard deviations of .30 and .28), and maximum speeds observed were 2.1 kts and 1.7 kts. Moreover, 10% exceedances for these hourly speeds are .77 and .71 kts respectively, whereas 10% exceedance hourly speeds of .72, 1.1 and .84 kts for three sites south of Disco Island are reported in [4].

For many bergs present at the sites for more than 24 hours, average speeds of 6.6 nm/24h and 5.1 nm/24h (standard deviations of 4.6 and 4.0) were computed for 24 hours. These speeds are consistent with the literature cited above. However, vector averages over all possible 24 hour periods (Cartesian coordinates, positive x east, positive y north, units nm,) are (1.53-1.34) and (.268,-1.08) with magnitudes 2.03 and 1.11 for the Pelican and Petrel sites respectively. This highlights the mathematical fact that the magnitude of the average velocity is, in general, less than the average
of the speeds. Consequently the average rate of berg transfer down the coast is not a precise concept and more study of the subject would clearly be useful.

However, it is clear from any of the statistics above that operational lead times of from two to four hours for disconnect are readily available. Icebergs, as a general rule, simply do not move fast enough to become completely unpredictable on these short time scales.

3. Variability of Iceberg Motion

Table 3 of [1] lists means and standard deviations for the speeds of 56 bergs at both sites, with \(0.481 \pm 0.29\) kts being a typical report. We interpret this to mean there is substantial variation in the speeds assumed by an individual berg and we find this consistent with the findings of Section 2 above.

![Diagram](image)

Fig. 1 The motions of G001, 013, 015, 020 and 022 during the period from 1900 on day 198 to 2400 on day 200.
Figure 1 plots the simultaneous tracks of several bergs present at the Pelican site and from such plots we assess the directional variability in the motion of a single berg, and in the comparative motions of neighboring bergs. (The time lapse between successive triangles is six hours; however, some of the bergs may not be present for the full time period involved). For example, G001, 013 and 015 are all present at 1900 on Julian day 198; G019 appears at 1800 on day 199; and G020 and 022, at about 2400 on day 197. Observe the directional changes in the tracks of 13 and 15 as compared to the tracks of 20 and 22.

On the other hand, compare the tracks of 19, 20, and 22 with the tracks of 13 and 15. All bergs, and particularly 19 and 20, are moving in a generally southerly direction at similar rates of speed. Yet the behaviour of 13 and 15 is decidedly different than that of 19, 20, and 22. With grounding probably accounting for the singular behaviour of G001, we thus see from Fig. 1 that the velocity of an individual berg can be highly variable over relatively short time scales (less than 12 hours). Moreover, we see that a region which, to a first approximation, supports coherence in the simultaneous motions of a group of bergs may itself be comprised of distinct sub-regions, each of which supports different coherencies at distances as small as 10 nm.

Having noted the existence of variability in the motions of icebergs, we identify three possible causes. First, we cite non-uniformities in the driving environmental forces arising from atmospheric and oceanic drag. Second, we suppose that differences between bergs could produce different responses to the same environmental driving forces. Third, we may even allow that variability in a given berg's motion in time is a consequence of the changes that the berg itself undergoes as it deteriorates.

Besides identifying the sources of this variability in iceberg motion, it is also important to assess the magnitude of the effect each such source has. Certainly this discrimination influences both the modelling of iceberg motion and the environmental monitoring needed in motion forecasting.

We deemed a precise analytic accounting of the causes of iceberg motion variability to be beyond the bounds of our data set. However, some qualitative observations are realistic. Recall from Fig. 1 that bergs 13 and 15 behave coherently, but differently, than the other bergs pictured, and assess the atmosphere, the ocean, and the bergs themselves as the causes of this observed variability in motions. We rule out the atmosphere as such a source of variability since the scale involved (10 nm) is much too small and data presented in [1, p.9] establishes coherence, over much larger
From evidence that is unfortunately not as complete as it ought to be, we will now infer that the ocean, rather than the bergs themselves, causes the observed variability in iceberg motions. It is a great shortcoming of both data sets that there is no record of ocean currents over the areas involved. For many iceberg tracks, including those in Fig. 1, there is often little additional information on berg parameters. We do know, however, that G015 has a length of 341 m, a mass of 1.5 million tons and a draft of 80 m, while G013 has a length of 238 m. Consequently, the strongest support for our contention comes from selected cases where data on berg parameters is richest.

Fig. 2  The motions of G001, 101, and 108. Observe the rapid reversal in the path of G108. The digits 1, 2, and 3 mark the position at 2400 on days 215, 216 and 217 respectively.
For example, Fig. 2 depicts the motions of G100, 101, and 108, all resulting from the breakup of a single berg. Bergs 100, 101 and 108 have, respectively, estimated masses of 1, 2, and .5 million tons; heights of 55, 54, and 29 meters; and lengths of 135, 157 and 86 metres. The draft of G101 is 85 m. These three bergs are different and yet spatially close enough to make acceptable the hypothesis that, at least initially, they are responding to the same driving forces. All three tracks exhibit the same gross features, and, as well, show similarities in many of the small scale features. We interpret this as evidence that individual berg differences result in only a small measure of the observed variability in the motions.

Further evidence against individual berg differences being the agent of berg motion variability is the failure of berg speed to statistically correlate with the berg parameter of mass. For example, the average speed of all bergs with an estimated mass of at least 1 million tons was \( 0.381 \pm 0.16 \) kts. The average speed for bergs having a mass less than 1 million tons was \( 0.469 \pm 0.19 \) kts. Since some of the large bergs were grounded and therefore had reduced velocities, we do not consider this difference to be significant. Moreover, Fig. 3 shows a scatter diagram for maximum speed vs. mass.
hourly speed vs berg mass. This figure shows large bergs to be capable of rapid motions and does not establish their trend towards slower speeds. While such a trend for larger bergs might appear with more data, based on the evidence in our two data sets we find no correlation between berg speed and the berg parameter of mass.

We are aware that our conclusions from Fig. 3 are not consistent with [4, p. 101-103] which reports that bergs of mass less than 1 million tons tended to drift in southerly directions, while larger bergs tended to drift to the north. The reason given for these observations is that the prevailing winds are from the north, while the deep currents flow toward the north. Moreover, smaller bergs moved faster; for example, the 10% exceedance speed for bergs of less than 1 million tons was .88 kts vs .78 kts for the larger bergs. Evidence of such phenomena was just not present in either of our data sets.

Furthermore, modelling studies with a two layer ocean (see [1], Tables 26-29) would lead us to expect much greater speed differentials between large bergs (with large drafts) and small bergs with small drafts than are reported in [4]; hence, some other mechanism may be involved in generating the observations of [4].

4. The Role of the Atmosphere

We have earlier ruled out the atmosphere as a source of local variability in the fine scale motions of icebergs. We do not, however, rule out wind as a cause of the gross movements of bergs. The literature contains a variety of contentions in this regard. For example Riggs [7], Sodhi and Dempster [9], and Mountain [5] assert that the dominant force driving icebergs is the ocean. Budinger [2], on the other hand, gives primacy to the wind, while Sodhi and El-Tahan [10] conclude that both winds and currents are essential to good simulation results. Finally, Foldvik et al. [3], using drogues to study Antarctic bergs, determined a surface layer (10-30 m in depth) having relatively little influence on berg motion, and observed some drogues moving with the deep current and having almost no relative motion with respect to the berg, but others, having relative motions of as much as .1 m/s.

Our data also evidences a partial correspondence between wind and berg motion. For example, in Fig. 2, bergs G100, 101, and 108 move to the right of the wind for two days. (The numbers 1, 2, 3 respectively denote positions at the end of days 215, 216, and 217. The wind initially and finally was from the west, making a swing to the south and back again between the evening of day 216 and the morning of day 218). During the third day the wind shifts and shifts back again but the bergs no longer
move to the right of the shifting wind. Equally puzzling is G101's high average speed (.55 kts vs .48 and .475 for 100 and 108 respectively) given its measured draft of 85 m (thus putting it well beyond the depth of frictional influence and making it the largest of the 3 bergs). Our experience with models of a two layer ocean (wind driven surface current and a separate deep current) suggests that the larger bergs should move more slowly than the smaller ones. Consequently, while we agree that there is a mechanism relating wind to berg motion, it would seem to be other than the simple effect generated by the inclusion of a wind driven Ekman layer in a model.

Fig. 4 The motions of G182, 194, 196, 197 and 198 for the period from 0100 on day 255 to 2400 on day 256. The arrow indicates 2400 on day 255.
5. The Fine Structure of the Ocean

From computer representations of iceberg tracks information about the fine structure of the ocean can be extracted. For example, in Fig. 1 we see, to the east of the drillship, southerly flow on a large and uniform scale, and to the west, an eddy with radius not much over 8 nm. Again, in Fig. 4 we observe the convergent motions of G182 and 194 while G196 and 197 diverge. At 1800 on day 255, these bergs are only 3.7 miles apart, yet in the next six hours they more than double the distance between them by moving in opposite directions; moreover, 197 moves at more than 1 kt, a rapid speed for a berg during this period. Berg 197 continues south for another six hours, and then moves an equal distance north in the following six hours, apparently caught in some type of eddy motion within the ocean.

The motions of these icebergs, which as we have argued above are driven principally by the ocean on the time scales shown, tell us that the spatial coherence in the current system can be substantially less than 10 nm. With the present state of our knowledge about how berg motions are driven by the ocean and the atmosphere, it would seem that good fine scale information on currents will be required if reliable forecasts on the order of 24 hours are to be achieved. It is also possible that this requirement for high density current information as prerequisite to reliable forecasts may be relaxed as the state of our knowledge increases.

6. Summary

The average daily speeds of bergs in our two data sets are consistent with reports in [6] and [8], but the magnitudes of the average 24 hour velocities for these bergs are significantly lower. This discrepancy is interpreted as a measure of variability in the motion of icebergs since the vanishing of this difference is equivalent to the uni-directionality of all berg motions. Another form of variability observed in berg motions is the juxtaposition of small scale coherencies for groupings of bergs separated by distances as little as 10 nm. Such motions are attributed to the fine structure of the ocean current regime which may, in turn, be deduced from the observed motions of the bergs. Berg parameters such as mass seemed not to correlate with observed motions for our data sets and modelling studies reported in [1] indicate that the atmosphere's effect on berg motions cannot be explained solely by a wind driven Ekman layer in a model. Hence the information about berg motions contained in records of their simultaneous tracks can yet yield significant insight into the requirements of prediction schemes and hazard detection systems.
Acknowledgements

It is a pleasure for us to thank Total Eastcan, and Didier Berenger in particular, for cooperation received in this endeavor. The bulk of the work was carried out while the last two authors were on sabbatical leave at C-CORE. The work of the second author was supported by NSERC grant A-9098.

References


"ENVIRONMENTAL DATA REQUIREMENTS FOR A REAL TIME ICEBERG MOTION MODEL" BY P. BALL, H. S. GASKILL and R. J. LOPEZ

DISCUSSION
BY:
J. BOBBIT, St. John's, Nfld.

Comment on P. LeBlond's question on whether or not wind-driven currents were important: From the 9-day current records, the speed of the current at a depth of 34 m compares with that at a depth of 178 m.

Question: Are the drift tracks from the vicinity of the Bjarni drill site located on the Makkovik Bank?

Further Comment: If the drift tracks are of icebergs in the vicinity of Bjarni, then the bathymetry is an important factor. At this location, the current is directed south to the west of the drill site, directed east to the north, and oscillating on the Bank. The drift tracks correlate very well with the current regime.
DISCUSSION BY J. BOBBIT ON "ENVIRONMENTAL DATA REQUIREMENTS FOR A REAL TIME ICEBERG MOTION MODEL"

AUTHOR"S REPLY
By: H. S. Gaskill and R. J. Lopez, Department of Mathematics and Statistics, Memorial University of Newfoundland

The co-ordinates of our sites are: 1974, 54° 54' N, 55° 52' W; 1979, 55° 31' N, 57° 42' W. Since Bobbit does not supply the coordinates of the Bjarni drill site, we cannot say if either of our two sites is in the area of her site.

In addition to her question, Bobbit makes the following points: first, the current meter results from her site show good correspondence between the current at 34 m with that at 78 m; second, bathymetry is an important factor in the drift tract of bergs at this site. Above we have argued, based on an analysis of the berg motions, that there ought to be a correspondence between the deep and surface currents; otherwise, the observed berg motions should be in better agreement with the results from theoretical studies of berg motions in a two layer ocean reported in [1]. Apparently, this correspondence is exactly what Bobbit's data show! A lack of sufficiently detailed bathymetric charts prevented us from analyzing our data to determine the effect of bathymetry on berg motion; however, in [1] we report that other workers have shown this to be an important factor and suggest that bathymetric information might play a very useful role in forecasting berg motions. Thus, we find Bobbit's comments completely consistent with the conclusions reported both above, and (with greater detail) in [1]. Lastly, we thank her for her useful discussion.
SIMULATION OF ICEBERG SHAPES AND THEIR IMPACT PROBABILITIES

D.V. Reddy, Professor
P.S. Cheema, Instructor

ABSTRACT

This paper presents the simulation of above-water and below-water iceberg profiles, and a formulation for determining the probabilities of iceberg impact. Both procedures are based on the Monte Carlo method. The parameters used for shape simulation are the ratios: draft/height, draft at maximum below-water width/total draft, and below-water volume/above-water volume. For the impact problem, empirical Bayesian probability estimates are obtained using a Fault Tree analysis.

INTRODUCTION

In Offshore Eastern Canada, the iceberg threat poses a serious problem during the drilling season. Many of the physical problems associated with the impact of offshore platforms, such as drift and scour, could be better understood if sufficient statistical information could be compiled for the following variables: seasonal regional distribution, number, size, above-water and below-water shapes, and other environmental factors such as the wind and wave climate, currents, pack ice, and tides. This investigation presents Monte Carlo procedures for simulating above-water and below-water iceberg shapes and estimating their impact probabilities. In the impact problem, a Fault Tree approach is used for determining empirical Bayesian estimates.

REVIEW OF LITERATURE

The occurrence of icebergs and their impact probabilities of a bottom-supported platform were evaluated by Blenkarn and Knapp [1] in the Grand Banks area off Newfoundland. Data on iceberg characteristics, iceberg towing, and iceberg drift
obtained from field measurements was presented by Dempster [2]. Dimensional modelling of iceberg shapes, described by Benedict [3], was based on deterministic model shapes. Mountain [4] developed a model to predict the drift of an iceberg which was tested by the International Ice Patrol. Reddy, Arockiasamy, and Cheema [5] presented formulations for determining the uncertainties in the prediction of offshore structure failure due to iceberg impact and modified impact probabilities.

SIMULATION OF ICEBERG SHAPES

Sizes and shapes are important factors in determining the drift and scour patterns of icebergs. But very little size and shape data is available for any detailed analysis. The International Ice Patrol of the U.S. Coast Guard has categorized them into the basic shapes of domed, tabular, dry docked, blocky and pinnacled, and further described them by their lengths and heights.

The parameters used in the Monte Carlo simulation are the ratios: draft/height, maximum below-water width/total draft, and below-water volume/above-water volume. The probabilities of inward/outward radial deviations, 4m, 8m and 12m from the waterline contour (observed dome-shaped iceberg) of the adjacent contour points are assumed to be 0.20, 0.50, and 0.30 respectively. These points are plotted using a sequence of random numbers which are linked to the cumulative probability distribution values. Each subsequent contour is plotted from the previous contour using the same iteration. All the above-water contours are simulated at 6.5m intervals, inward from the surface until the observed iceberg height is reached. The below-water contours are simulated at 8m intervals outward from the surface until the draft at maximum width (0.26 times the total draft - an average for a set of observed icebergs) is reached, and followed by 8m intervals inward till the draft is about five times the height (observed iceberg). This procedure is iterated until the above-water volume is approximately one-eighth the below-water volume. The iteration is carried out by choosing another sequence of random numbers for Monte Carlo simulation. The generated contour profiles are plotted in Figs. 1 and 2. This approach can be used to simulate other iceberg shapes; more parameters can be incorporated easily.

IMPACT PROBABILITIES

The offshore structural failure resulting from iceberg impact, called the TOP event, is assumed to be associated with the occurrence of simpler basic events, termed the primary inputs of the tree, Fig. 3. The basic events are widths and drift angles of the icebergs. The Blenkarn-Knapp impact probabilities, associated with the variation of these parameters are first computed using fault tree analysis. These
probabilities can be modified, Ref. 5, assuming statistical independence of the parameters.

The uncertainties involved in the estimation of the impact probabilities, associated with the sets of drift angles and iceberg widths, make it necessary to assign confidence limits to the TOP event probability of a fault tree, Barlow and Lamberg [6]. In the simulation, the iceberg impact with the offshore structure is treated as a random variable, and expressed as an algebraic function of the other random variables (impact probabilities associated with the drift angle and iceberg width). If the fault tree has 'k' minimal cut sets (a minimal cut set is defined as a set of minimum number of basic events required to ensure the occurrence of the TOP event), and the probability of existence of the ith minimal cut set is \( Q_i \), then the upper bound of the probability of existence of the TOP event, Ref. 6, is given by

\[
Q_{\text{TOP-upper}} = \sum_{i=1}^{k} Q_i
\]  

Having determined the impact probability distributions associated with the drift angle and width, a random value (sample) is picked for each of the two. The normal random numbers (as well as the lognormal random numbers) are generated by applying the Central Limit Theorem to the uniform random values developed, and used to compute a point value for the TOP event probability using Eq. 1. The procedure is repeated for a large number of trials, and an estimate of the system impact probability distribution obtained Fig. 4. The probability intervals are then the corresponding percentile point values of the estimated distribution. The probabilities of occurrence of all widths and drift angle groups can be computed based on the frequency of occurrence measurements of one simulated iceberg. These values are termed prior probabilities. The judgemental probabilities are based on the average relative frequencies of the width and drift angle groups for the remaining set of simulated icebergs. The Bayesian posterior probability mass functions, PMF, are obtained as

\[
P_{w} = P''_{w} (w=w_1) = \frac{P(e/w=w_1) P'(w=w_1)}{\sum_{i=1}^{n} P(e/w=w_i) P'(w=w_i)}
\]  

\[
P_{\theta} = P''_{\theta} (\theta=\theta_1) = \frac{P(e/\theta=\theta_1) P'(\theta=\theta_1)}{\sum_{i=1}^{n} P(e/w=w_i) P'(w=w_i)}
\]

where

\( P(e/w=w_i) \) = conditional impact probability of a width group when \( w=w_i \)

\( P'(w=w_i) \) = prior impact probability for the selected iceberg
\[ P(\epsilon/\Theta=\Theta_1) \] is conditional impact probability of a drift angle group when \( \Theta=\Theta_1 \) and
\[ P'(\Theta=\Theta_1) \] is prior impact probability for the selected iceberg.

**NUMERICAL ILLUSTRATION**

Dimensions of the region = 90,065m square. Assumed width of the platform = 50m. The drift angles and widths obtained from field data collected in Lancaster Sound are as follows:

**Drift Angles:** 320.93° to 139.40° based on a set of 4 icebergs

**Width:** 185m - mean width of observed set of 35 icebergs varying from 50m to 390m.

Typical prior and posterior probability mass functions for drift angles, Eqn. 3, are illustrated in Fig. 5. These posterior probabilities \( P'(\Theta=\Theta_1) \) and \( P'(\Psi=W_1) \) are used in Eqn. 1. The input data for the special-purpose computer program for Fault Tree analysis are the median and the error factor (either of the ratios, 95% value to the median or the median to the 5% value) of the posterior impact probabilities shown in Fig. 5b. The estimates of the system impact probabilities obtained are shown in Table I. It is easily seen that the design value would be 0.0833 with a confidence of 99.5%.

**CONCLUSION**

The shape simulation is of a preliminary nature with considerable scope for improvement with respect to constraint conditions for iceberg shapes. The assumed probabilities of the radial deviations may be treated as prior probabilities, and improved using a Bayesian approach based on the measured iceberg contour profiles. The judgemental probabilities are determined from the radial distance ratios of the contours for the observed iceberg. Also, the impact probabilistic analysis can be extended by expressing drift angles in terms of parameters such as cut sets of wave, current, wind, and storm data.

**ACKNOWLEDGEMENT**

The authors are grateful to Dr. I.E. Rusted, Vice-President of Professional Schools and Professor C.D. diCenzo, Dean of Engineering and Applied Science, Memorial University of Newfoundland, and Mr. O.S. Toope, Head of Applied Arts, College of Trades and Technology, St. John's, for their keen interest and encouragement. The support of this investigation by the National Sciences and Engineering Research Council of Canada, Operating Grant No. A-8119 and Strategic Grant No. G-0561 is gratefully acknowledged.
REFERENCES


### TABLE 1

**Iceberg impact probability**

Random variables: Drift angle and iceberg width

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<td>8.3381E-02</td>
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Fig. 1 Simulated above-water contours and a typical profile.
Fig. 2 Simulated below-water contours and a typical profile.
ICEBERG IMPACT AND OFFSHORE STRUCTURE FAILURE

LEGEND

OUTPUT EVENT

'OR' GATE 'AND' GATE

INPUT EVENTS (BASIC)

WIND CURRENT WAVE STORM

Fig. 3a General fault tree sample

Fig. 3b Reduced fault tree
ICEBERG DATA, ASSUMED PLATFORM WIDTH

<table>
<thead>
<tr>
<th>DRIFT ANGLE</th>
<th>ICEBERG WIDTH</th>
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<td>BLENKARN-KNAPP IMPACT PROBABILITIES</td>
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<td>TWO SETS: DRIFT ANGLE AND WIDTH</td>
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<td>COMPUTER CODE</td>
<td>(LOGNORMAL DISTRIBUTION)</td>
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<tr>
<td>-FAULT TREE ANALYSIS</td>
<td>AND MONTECARLO SIMULATION</td>
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<tr>
<td>BAYESIAN ESTIMATES OF IMPACT PROBABILITY AND CONFIDENCE BOUNDS OF 'TOP' EVENT (OFFSHORE STRUCTURE FAILURE DUE TO ICEBERG IMPACT)</td>
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</table>

Fig. 4 Formulation Outline
Fig. 5a Prior PMF of drift angles

Fig. 5b Posterior PMF of drift angles
"SIMULATION OF ICEBERG SHAPES AND
THEIR IMPACT PROBABILITIES"

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The authors would like to thank Dr. Ghoneim for his discussion of the paper. We have pointed out in our paper that the impact probabilistic analysis can be extended to include parameters such as cut sets of wave, current wind, and storm. Berg density can also be considered as one of the cut sets in the impact analysis.
The design of a marine natural gas pipeline in Arctic waters must recognize and fully consider the effects of possible ice formation on the submerged pipeline. A thorough investigation of this phenomenon will ensure that the design will result in a stable pipeline system throughout its operating life.

An experimental site was established near the proposed marine crossing in Byam Martin Channel between Lougheed Island and Melville Island in March, 1980, to investigate the possibility and extent of ice buildup on submerged natural gas pipelines. The first two experiments were intended to validate results from similar experiments conducted at the University of Toronto. The third experiment, for which no equivalent data were available, was designated to simulate sediment freezing on a fully buried pipe in which compressive effects were important. Since the heat transfer rates depend on the current normal to the test pipe and external bulk water temperature, these were measured in conjunction with the heat transfer experiment. The three experiments were:

a) Heat transfer measurements from a test pipe resting on the bottom sediments with external bulk temperature and normal current.

b) Observation of possible ice formation and its characteristics on a horizontal pipe section resting on the bottom sediments.

c) Observation of the characteristics of possible ice formation on a fully buried pipe section.
INTRODUCTION

Natural gas flowing in a pipeline with increasing downstream elevations will tend to cool for two reasons:

a) Joule-Thomson Cooling - Non-ideal gases change temperature with an isenthalpic change in pressure. The magnitude of this change is called the Joule Thomson coefficient

\[ \mu = \frac{\partial T}{\partial p_H} = \frac{T(\partial v/\partial T)_p}{c_v} - v \]  

For methane, the coefficient is negative.

b) Expansion Work - A combination of the first law of thermodynamics and the pressure distribution of gases subject to gravity (pressure balance) shows that any gas rising against the acceleration due to gravity \( g \) will tend to cool as it does work against the column of gas above it. A useful form of this relationship is

\[ \frac{\partial T}{\partial x} = -g \frac{\partial z}{\partial x} / (R + c_v) \]

These equations show that the rate of cooling depends directly on the rate of increase in elevation. The effect of this can be very important, as calculations show that the pipeline surface temperature depends on the gradient \( \partial z/\partial x \) as well as the gas flow rate, the pipe diameter, the water currents and temperature and the heat transfer resistance of the pipe insulation.

These effects are of little importance in warm waters where sufficient sensible heat can be transferred to the gas to keep the pipe surface temperature above the freezing point of the surrounding sea water. In Arctic waters, however, where a long crossing is followed by a shallow rise to the shore, the rate with which heat can be transferred to a pipeline may be insufficient to prevent the pipe surface from falling below the freezing point of the surrounding sea water.

The extent of ice formation if any, its equilibrium thickness and net buoyancy, depend on the gas flow rate, sea water temperature, the elevation change, the convective heat transfer rate and, to a lesser extent, the thermal resistance of pipeline construction materials and insulation.
It is possible that frozen soil underneath the pipeline may be a factor in assisting to maintain stability of the pipeline on the bottom. Therefore, additional experiments were designed to determine the characteristics of frozen sediments immediately under the test pipe and for a deeply buried portion of the test pipe. The characteristics of ice-frozen soil mixtures will depend on the freezing rate, soil conditions and whether or not the frozen mixture formed is confined as in the case of a buried pipeline.

OBJECTIVES

The objectives of the project were to design, transport and assemble the following:

1. a set of apparatus to measure current velocity and heat transfer coefficients from a test pipe resting on bottom sediments in Arctic waters

2. a test section on which the effects of ice formation on a pipe resting on the bottom sediments were to be simulated

3. a test section on which the effective ice formation on a buried pipeline can be simulated.

For all the above systems, means of maintaining open large diameter holes in the ice had to be found, electrical power and suitable environment for instruments and workers provided. A video camera and still photographs of the project were also planned.

EXPERIMENTAL EQUIPMENT AND PROGRAM DESIGN

Heat Transfer Measurements

Two duplicate test sections, Mark I and Mark II, were constructed for the Heat Transfer experiments. The details of the test sections are shown in Figure 1. Each consisted of standard steel pipe, 16.83 cm outside diameter and 152 cm long. Each test section was instrumented with 8 thermistors located at 45° intervals on the inside circumference of the pipe wall. Since the resistance to heat transfer of the steel pipe is extremely small compared to the resistance of the outside film
of sea water, the thermistors gave a true indication of the outside pipe surface temperature. The leads from the thermistors were connected to the surface by means of an underwater connector and 16 wire submarine cable.

Two stainless steel spars 10.2 cm long and mounted on the test section at about 10° from the top were installed to measure temperatures within the water boundary layer. Liquid propane was charged and the vapour discharged by means of hydraulic hoses was collected on the surface where it was metered. The propane pressure (and temperature) was controlled by means of a pressure reducing valve. Pressures and temperature of the vapour produced were measured, so that the mass flow rate could be used to calculate the heat flux from the surrounding sea water, whether by convection or freezing. A hinge assembly was welded to the top of the test section to which a series of 3.048 m, 3.81 cm O.D. aluminium poles were connected. These poles were intended to provide means of orienting the test section, and also to give an indication of the depth at which the test section was located. The hinge permitted lowering of the test section through 0.39 m holes in the ice in the closed position.

Once the test section was below the ice layer, it was designed to open due to its own weight so that it could then be lowered horizontally to any position below the ice. A marine cable was attached to the hinge to support most of the weight of the test section. In addition, it was found that the hoses could also be used to raise and lower the test section. About 1.5 m above the hinge, and free of the hose connector, a hot-film two wire probe was attached for measuring currents. This probe was connected to the surface by means of electrically equilibrated cables, and current readings could be made using a two-channel anemometer system and a four-channel multimeter.

It was found that in 50 m of water the weight of hydraulic hoses, 16-conductor cable and test section was too great to be conveniently handled by one man, even with the assistance of a 3/8" stainless steel braided cable and tripod. Therefore, a tripod, pulley and boat winch were fabricated to raise and lower the test section and hoses.

In order to ensure that the ice hole remained open, the holes augered initially with an 0.457 m bit were lined with an 0.406 m PVC pipe 6.4 mm in wall thickness. The pipes were supported by means of three brackets on the surface. Heat tapes were then placed in the holes and a total power of 1,100 W supplied to each pair of heat tapes.
This was more than adequate to keep the hole open once the initial sub-cooling of the surrounding ice was removed.

**Ice Formation on a Horizontal Pipe**

Experiment KII was designed to study the characteristics of ice formation on a horizontal section of 4 inch standard steel pipe. The pipe was 122 cm long and capped at both ends. Two high pressure hydraulic hoses for propane were connected to the centre of the pipe and a stainless steel cable was attached to one end so that the pipe could be lowered vertically through the hole in the ice, and recovered with ice and attached sediment. A schematic diagram of the test section is shown in Figure 2.

**Ice Formation on a Buried Section of Steel Pipe**

The test sections for this experiment were labelled KI and KPI. They are shown in Figure 3 in their designed configuration on the ocean bed. Test section KI was a standard steel pipe 3.8 cm O.D. and 122 cm long with an adjustable lead collar for guidance and control of the probe during its descent and penetration into the bottom sediments. PVC pipe insulation was applied to the top part of the test section to reduce heat losses in this region. The cooling refrigerant (propane) was charged by means of a high pressure hydraulic hose which was connected to a length of stainless steel pipe leading to the bottom of the test section. The propane then boiled off thereby providing the required cooling.

In initial experiments with the test section KI, it was postulated that an ice-soil bulb had been formed underneath the collar, but it had been retained in the bottom sediments when the test section was lifted because of the smooth surface of the probe and the absence of any means of anchoring the ice bulb. This hypothesis is supported by the fact that a considerable force was required to free the test section. Therefore, a new design fabricated on site (KPI) was modified by the attachment of four lugs on the bottom of the probe to ensure that any ice bulb formed would be firmly retained. The new test section was made from a piece of 2 inch standard steel pipe 7.6 cm O.D. and 122 cm long. The lead collar was replaced by a steel plate, which was supplemented with fins welded to the top of the test section to ensure vertical penetration of the probe. The test section was insulated with ABS plastic pipe to reduce heat gain. Both test sections were initially supported by a 1/16" marine stainless steel cable, but it was found that these were not strong enough to pull the test section from the bottom once an ice bulb had formed. As a
result, the hoses were used for raising and lowering the test section.

During the tests it was found that the best procedure was to lower hoses manually until the test section was about 7 to 10 m above the bottom and then to release the hoses. The weight of the test section and the hoses ensured penetration of the probe up to the collar. Samples of soil and ice were obtained during the experiments.

For recovering the test section from the bottom, it was found necessary to utilize the Bombardier vehicle to overcome the extremely high retentive force of the ice bulb that was formed.

RESULTS AND DISCUSSION

The equipment was delivered to the Pat Bay Camp on Lougheed Island by April 4, and the Parcoll, Lister generator, fuel supplies, instrumentation and heating were set up ready for experimentation by April 7. From the beginning, weather conditions and particularly barometric pressure were logged and tabulated on a daily basis.

The video camera proved very helpful not only for recording underwater conditions of importance to the experiments, but also as a means of assisting in the recovery of test sections with attached hot film anemometer probes. A two-way radio or telephone system is highly desirable so that operators of the video screen and the camera control within the Parcoll can direct the raising, lowering and panning of the camera.

Key elements of experiments for the determination of heat transfer and the ice formation studies on submerged surfaces were field tested on Lake Simcoe late in February, 1980. Such a field test is invaluable in establishing difficulties which could arise during the experiment. Many improvements in techniques were made during the field trial and detailed procedures and practices for new equipment usage proved extremely helpful for later usage under Arctic conditions.

The test site was established about 6 km offshore, and due west of the landing strip. Ice thicknesses on this site were between 3 and 4.5 m in the shallower portions near shore where the water depth was about 51 m. Water depths from about 1.5 km offshore were measured through 15 cm bore holes and found to agree reasonably well with those published by the Canadian Hydrographic Service. Throughout the program a tracked vehicle was used for transportation to the test site from Pat Bay.
Temperature and Current Measurements

The instrumented test sections MKI and MKII were utilized to measure vertical temperature profiles under the ice. The recorded temperatures were very close to the equilibrium or freezing point of the water from the underside of the ice surface to the bottom. The mean temperature indicated was between \(-1.8\) and \(-1.9^\circ C\), very near to or at the freezing point.

Current measurements were made with the hot film probe at different depths throughout the experimental program and showed the expected sinusoidal behaviour as shown in Figure 4. Because a hot film probe cannot sense direction, the lowest current reading was taken as zero and a change in sign of current velocity was indicated in the plots. The direction of the currents was not determined. Although the minimum feasible current \(0.5\) cm/sec was stated as the measurement by the manufacturer, reliable observations were actually obtained for currents as low as \(0.3\) cm/sec. The low current readings were obtained about 5 m (15 ft.) from the ocean bottom.

Heat Transfer Measurements

The heat transfer apparatus MKII was lowered on April 7, 1980, and temperature measurements were made from the thermistor string until the test section reached the bottom. Leakage in the electrical connector prevented heat transfer results from being obtained with this test section. It was recovered with some difficulty but successfully with the assistance of the underwater camera. MKI reactivated with replacement parts flown in from Toronto was lowered to the bottom on April 21, but without the aluminium poles which had been lost and damaged during recovery operations for MKII. Heat transfer measurements were taken at current velocities of \(0.8\) to \(1.2\) cm/s\(^{-1}\) on April 21 and 22. The results of the heat transfer measurement are shown in Table I. The significance of these results to pipeline technology is discussed elsewhere (1).

Ice Formation on a Horizontal Test Pipe

During the first run of 8 hours, a temperature of \(-6.8^\circ C\) was maintained. On completion of the run the pipe was lifted and was found to be free of ice and soil. It is possible that an ice-soil layer had formed, but remained on the sea bottom because of the probe's smooth surface and poor adhesion qualities, as previously discussed.
The second run was conducted for 19 hours at a measured temperature of -16.7°C. Ice was observed with the camera to form on the pipe; however, again, when the pipe was brought to the surface, it was virtually free of ice except around the regulator. Partial plugging of the propane relief hydraulic hose probably gave an indication of quite low pressure on the surface, but the pressure, and hence, temperature in the test section was in all probability significantly higher. The third run with this test section was carried out at a temperature of -8.9°C. After 114 hours the pipe was lifted to the surface and it was observed that the test section had been partially buried during the experiment. An ice lens, approximately 0.32-7.0 cm thick, was observed next to the upper part of the pipe surface. As well, a fragile layer of columnar ice partially covered the bottom surface of the pipe. The bulk densities of small amounts of soil on the underside of the ice were quite low, approximately 1150 kg·m⁻³. This is reflected in the apparent loose structure and in the inclusion of many ice crystals. The ease with which the bottom sediments fell away suggests that the mechanical strength of the ice soil interface is extremely low and that the mass of soil frozen below a partially submerged pipeline could not be counted on to retain the pipeline on the bottom under all circumstances.

Ice Formation on a Buried Section of Steel Pipe

In the first experiment test section KI was buried to its collar and the propane bleed rate was controlled to give a temperature of about -14.2°C. Clear ice was observed to have formed on the test section after 30 hours and some frozen soil was present directly beneath the collar.

Experiments with KPI were conducted over a 32 hour period at a temperature of -9.8°C. A large ice-soil bulb had formed on the lower portion of the test section, and the bulb diameter measured 43.2 cm at its widest point, about the diameter of the 16" PVC pipe lining the ice hole. Clear ice was observed near the pipe surface directly beneath the collar, with a thickness equivalent to the width of the collar (3.8 cm). This ice layer decreased to about 2.5 cm further down the length of the pipe, tapering off to about 0.08 cm at the tip. Clear ice was also observed on the upper portion of the pipe. The ice and frozen soil formed were well consolidated, very strong and very adherent to the pipe. Thin lenses of ice were observed to have formed radially from the test pipe. The bulk density of the sample was between 1250 and 1300 kg·m⁻³, just slightly less than that of the free sediment. Since the ice lenses tend to radiate from the test section, they do not reduce the strength of the ice bulb. Removal of this ice bulb from the bottom sediment required substantial force, probably in excess of a tonne. This suggests that the retentive force of
ice-soil layers on deeply buried pipelines will be quite large. The results of these experiments are shown in Table 3.

**SUMMARY**

The results of the experiments may be summarized as follows:

a) Sea water temperatures are at or near the freezing point of the sea water from the bottom surface of the ice to the bottom at the test site located about 6 km offshore from Lougheed Island and west of the Pat Bay airstrip. At this location the water depth is 51 m.

b) Currents at the test site off Lougheed Island are extremely low, even allowing for the fact that measurements were made normal to the test section and were not likely maxima. The currents measured agreed qualitatively with those measured by others in the Arctic. The minimum measurable current of about 0.3 cm/sec by the hot film probe is significantly more sensitive than that obtained by conventional current meters. However, the instrument is not as convenient to use since it requires a power source.

c) Heat transfer measurements were made about 3 m from the bottom, and the results are important in the design of a submerged natural gas pipeline in the extremely low currents and cold seas of the Arctic.

d) A relatively thick ice layer was formed on the horizontal test pipe as shown in some of the photographs but the frozen soil layer underneath fell loose when the test section was raised. This indicated that, in this case, the mechanical strength of the frozen soil interface is quite low. Since the surrounding sediment was easily displaced, the bulk density of the frozen soil interface was probably also low at about 1100 kg/m³ as opposed to a sediment density of greater than 1300 kg/m³.

e) Ice salinities of 2 to 3 ppt were measured in the sea ice and formed quite quickly on the test sections during these experiments.

f) Experiments with the submerged probe showed that a retentive frozen soil plug was formed on the buried pipeline with the resultant compression of the soil sediment. A complex network of striation, thin ice lenses, was formed and
the bulk density of the material was 1250-1300 kg·m$^{-3}$, close to that of the surrounding sediment. The results of this experiment suggest that the burial of more than 50-60% of the pipeline can result in the formation of a retentive soil-ice layer which will remain buried indefinitely, provided that the net density of the pipeline and the layer is equal to or greater than that of the surrounding sediment.

g) The underwater camera was extremely useful for observations during the experiments. Two-way communication between the Parcoll and the ice hole would have assisted in obtaining more efficient operations during the experiments.

h) Experience gained in conducting this series of experiments pointed out the value of careful detailed pre-planning including preparation of detailed operating procedures, rehearsal of procedures, good record keeping and the inclusion of support logistics such as shelter erection, power supply, heating and quality of support personnel.

REFERENCE


ACKNOWLEDGEMENTS

The authors gratefully acknowledge the help of many during the planning and field experimentation stages including Sheila Rawlings, John Lynch, Henry Boesch, Andy Jenkins, Stephen Peck, Helmut Lanziner, John Aslin, Martin Kopp and especially of Ed Ryan.
## TABLE 1: Heat Transfer Results

<table>
<thead>
<tr>
<th>Test Section: 16.83 cm O.D., 152 cm long</th>
<th>RUN NUMBER</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Date, 1980</td>
<td>21/4</td>
</tr>
<tr>
<td>Time</td>
<td>14:30</td>
</tr>
<tr>
<td>Current, cm·s⁻¹</td>
<td>0.8</td>
</tr>
<tr>
<td>Re</td>
<td>72</td>
</tr>
<tr>
<td>$T_s^{\circ C}$ (corrected)</td>
<td>-2.8</td>
</tr>
<tr>
<td>$\Delta T^{\circ C}$</td>
<td>0.88</td>
</tr>
<tr>
<td>Gr $(x\times10^5)$</td>
<td>3.0</td>
</tr>
<tr>
<td>Gr/Re²</td>
<td>58</td>
</tr>
<tr>
<td>$h_o (BTU/h\cdot Ft^2\cdot F^o)$ (at 3 m off bottom)</td>
<td>13.6</td>
</tr>
<tr>
<td>$h_o (W/m \cdot K)$ (at 3 m off bottom)</td>
<td>77.2</td>
</tr>
</tbody>
</table>
### TABLE 2: Results of Horizontal Test Pipe Measurement

<table>
<thead>
<tr>
<th>RUN NUMBER</th>
<th>GAS PRESSURE AND TEMPERATURE</th>
<th>DURATION</th>
<th>OBSERVATIONS</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>387.5 kPa (-6.8°C)</td>
<td>8 h</td>
<td>No ice or frozen soil on pipe; frozen soil may have become detached</td>
</tr>
<tr>
<td>2</td>
<td>273.6 kPa (-16.7°C)</td>
<td>19 h</td>
<td>No ice</td>
</tr>
<tr>
<td>(indicated temperature and pressure may have been in error)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>360.0 kPa (-8.9°C)</td>
<td>114 h</td>
<td>Ice lens up to 7 cm thick on upper surface Bottom partially covered with thin columnar ice Frozen soil layer fallen free, but bulk density ~1150 kg·m⁻³ for residual frozen soil</td>
</tr>
</tbody>
</table>
### TABLE 3: Results of Buried Pipe Measurement

<table>
<thead>
<tr>
<th>TEST SECTION</th>
<th>PROPANE PRESSURE AND TEMPERATURE</th>
<th>DURATION</th>
<th>OBSERVATIONS</th>
</tr>
</thead>
</table>
| KI           | 308 kPa
(-14.2°C) | 30 h | 1-2 cm clear ice down to collar, no frozen soil bulb  
Ice density 920 kg·m⁻³  
Ice salinity 2.29 ppt |
| KI           | 343 kPa
(-10.4°C) | 87 h | Clear ice on collar, upper part of test section, no frozen ice bulb |
| KPI          | 349.6 kPa
(-9.8°C) | 32 h | 1.5 cm clear ice on upper surface, mechanically strong frozen soil bulb 43.2 cm diameter at base, clear ice under soil bulb, 3.8 cm to 0.08 cm at the tip.  
Extremely retentive bulb.  
Bulk density of soil (5 samples),  
ρ = 1285 ± 21.6 kg·m⁻³ |
HEAT TRANSFER TEST SECTION MKI AND MKII

**FIGURE 1**

- **Drill & Tap for Propane Fitting (x2)**
- **Drill & Tap for Thermistor Spars**
- **Welded Blind Flange**
- **Bolted Flange**
- **152.4 mm Std. Steel Sch. 40**
- **Hinge Assembly**
- **Electrical Connector**
TEST SECTION KII
TEST SECTIONS KI AND KPI

Hydraulic Hoses
38mm Pipe Cap
Stainless Steel Cables
Fins
PVC Insulation
ABS Insulation
Lead Collar
Plate Collar
Aluminium Tubing
Sea Bed
Lugs
KI
Ice Bulb
KPI
FIGURE 3
CURRENT DATA NORMAL TO TEST SECTION
HEAT TRANSFER EXPERIMENTS

FIGURE 4
Slide 1 - Offshore field camp

Slide 2 - MKII Heat Transfer probe prior to lowering through ice hole

Slide 3 - Test Section MKI, showing ice formed during Heat Transfer Test April 22, 1980

Slide 4 - Test Section KII, used for testing ice formation on a horizontal pipe

Slide 5 - Ice-Soil Interface on KII, showing dendritic structure

Slide 6 - Ice Lens on KII
Slide 7 - Relatively pure ice covering bottom of KII; frozen soil may have fallen away

Slide 8 - Test Sections KI and KPI after recovery of KI and loss of frozen soil bulb

Slide 9 - Frozen ice-soil bulb; Test Section KPI

Slide 10 - Section of ice bulb showing clear ice layer near test pipe and thin lens of clear ice in soil

Slide 11 - Clear ice lens adjacent to Test Pipe KPI. Note dendritic pattern in frozen soil
The authors are to be commended for this contribution on experience with foam cell enhanced ice platforms. The paper presents current performance experience, design approach and construction details. Ice platforms have proved very effective in supporting exploratory drilling and now the addition of foam cells to add buoyant support is a significant advance. Panarctic Oils Ltd., is to be congratulated for releasing this information.

The design approach for normal ice platforms based on equation (1) and experience from 16 cases is well in hand. Interpretation of equation (1) and Fig. 1 would be aided if numerical values of m and n were given. Also in presenting the two previous load cases in Fig. 1 it should be pointed out that they are derived from ice platforms constructed prior to Char G-07.

In extending existing experience to foam cell enhanced ice platforms a problem arises on superimposing the effects of foam buoyancy and rig load. Conceptually the foam effect can be interpreted in terms of superimposing deflections which create excess freeboard and thus extra allowable deflection, or else in superimposing loads as postulated in equation (2). The experience at Char G-07 supported the superposition of loads. Initial results for Maclean I-72 are given in the paper. What was the total season experience here?

It is most encouraging to see experience such as this published in a timely fashion.
ICE PLATFORMS WITH URETHANE FOAM CELLS IN THE NEUTRAL AXIS ZONE AND THEIR APPLICATION IN ARCTIC OFFSHORE DRILLING

Vol. 1 page 49

AUTHORS' REPLY
By:

C. Maclean, W. Semotiuk, A. Strandberg and D.M. Masterson

The authors would like to thank Dr. R.M.W. Frederking not only for his discussion on the above paper but for his many years of involvement with the entire ice platform projects. Dr. Frederking's attention on the numerical values of m and n reflects his and our continuing research in the area of the long-term load carrying capacity of an ice sheet. Equation (1) has been developed over the past five years for loads of drill rig size and has been found to be reliable for such. In 1980, when reference 1 was written, we were using a value of m = 1.8 and n = 3.0 and since then additional data have further reinforced our confidence in these values.

Equation 2 was used to predict the deflections of Panarctic Rig A at Maclean I-72, based on data from Panarctic Rig B. This was due to the fact that Maclean I-72 was the first well that Rig A drilled on an offshore location. The long-term deflection of Rig A on the Maclean platform was predicted at 390 mm below the 11% freeboard level. The recorded value was 298 mm below the 11% freeboard level, indicating a conservative prediction. It should be noted, however, that we feel the agreement is reasonable, given the fact that the load predictions for Rig A and Rig B are not exact and the area of loading of the two rigs is slightly different.
"MARINE PILING AND BOAT HARBOR STRUÇTURE DESIGN FOR ICE CONDITIONS"
By C. Allen Wortley

Vol. 1 page 70

DISCUSSION
By:

James E. Muschell, United Design Associates, Inc., Consulting Engineers, Cheboygan, Michigan, USA

The information on "Marine Piling and Boat Harbor Structure Design for Ice Conditions" as presented by C. Allen Wortley represents an accurate and meticulous compilation of data with regards to the winter regime in Great Lakes' boat harbors. The design criteria and related recommendations represent the best available information with regards to the present state of the art for the design and installation of marine piling and boat harbor structures in the northern Great Lakes latitudes. As a designer for the past 32 years engaged not only in the design of harbor structures and marinas but also as a field researcher in the areas of ice damage and its causes, I am also cognizant of the need for further scientific research, in the laboratory as well as under insitu conditions, to further minimize the problems of a hostile ice environment.

"Harbor structures not protected with ice suppression systems must be designed to withstand horizontal and vertical forces. At this time these forces can only be approximated." The fact that the horizontal and vertical ice forces sustained by harbor structures can presently only be approximated clearly indicates the need for continued and extended research in these areas not only in the laboratory but under insitu conditions. I likewise concur with C. Allen Wortley that there are at present no published data of estimated or measured values for thermal thrusts on individual pilings. I believe on the basis of personal experience that the lateral thermal ice thrusts on individual piling act quite differently than the thermal ice thrusts on solid or gravity type crib structures and that these forces may be far greater under certain conditions than generally expected. I would also suggest that studies comparable to those done by Drouin and Michel on gravity type structures be done on individual piling in conjunction with an insitu investigation.
program to arrive at a correlation between the two. As a harbor designer I also believe that additional work should be done with regards to weather studies in individual harbor areas to prepare some type of frequency curves for purposes of cost benefit studies to arrive at a methodology of determining damage coefficients. Although it may not be possible to eliminate all ice damage, it may be economically feasible to arrive at alternative designs that would be acceptable with some annual ice damage in lieu of designing for the ultimate estimated ice load factors.

The summary conclusions offered by C. Allen Wortley are excellent based upon my own personal experience as a harbor designer also engaged in insitu testing of ice forces and ice properties. I do believe, however, that continued field and laboratory research regarding ice properties, the ice forces developed with respect to harbor geometry, weather studies for various areas for purposes of evaluating frequency curves with respect to changing ice conditions and related uplift and lateral thrust forces. These represent just a partial list of continued effort toward finding acceptable solutions to the problem of design under hostile winter environments. C. Allen Wortley is to be congratulated for his continued efforts in the compilation and dissemination of his data regarding his numerous harbor investigations.
AUTHOR'S REPLY
By:

C. Allen Wortley, Associate-Professor of Engineering and Applied Science, University of Wisconsin-Extension, Madison, Wisconsin, USA

The author appreciates Jim Muschell's continuing interest in and research on ice forces and actions in harbors, and thanks him for his discussion. He is one of the few dock structure designers whose structures have generally withstood the test of time and the ravaging effects of ice and winter.

The author is continuing the monitoring program of field conditions and observing ice actions on structures, both newly designed and older facilities. For each type of facility there are many examples of current damages. When this happens it is particularly discouraging to the designer (as well as the owner) who believes he has executed a safe and economical design; but then finds some unusual ice condition has appeared and overpowered his construction. It is anticipated that work will continue in this practical area and that the US Sea Grant program will continue sponsorship.

The expressed concern for information on thermal thrust on individual pilings is real. Damages are continuing to harbor structures from this phenomenon. Lateral forces on tall free standing structures, such as pole-type transmission line towers, have been reliably estimated and deep shaft foundations successfully designed using the latest methods provided by geotechnical engineers. The isolated pile structure in ice is analogous, yet reliable force estimates are unknown and hence pile-foundation design is speculative—often resulting in structure failure. The author agrees that additional laboratory and field research is advisable.

Mr. Muschell's suggestion that weather related frequency curves for cost-benefit analyses and ice damage coefficients be formulated for northern harbors is intriguing and should be explored further.
The Cobourg and Peterborough Railway engineer Dumbell [1] more than a hundred years ago stated,

"I may add, that the ignorance, or want of proper appreciation, of the properties of ice, evinced in the construction of numerous wharves, piers, and bridges on the inland lakes and rivers of Canada and the northern States, has proved a source of infinite annoyance and of immense expense".

Let's hope yet another hundred years doesn't go by and we're still annoyed!

References

Response to Questions about "ICE RESISTANCE EQUATION FOR FIXED CONICAL STRUCTURES" by Larry D. Brooks

Question: What comparisons have been made between results of this equation and those of Croasdale-Edwards, Bercha and Ralston?

Response: First let me say that the purpose of this paper is to present a general expression (Equation 3) with a method for determining the best exponents and coefficients. The model test data were used only to demonstrate that method, and the resulting exponents and coefficients (Equation 4) are valid only for that data base.

However, in response to the question, the following results are presented, with average model test data, tabulated in the paper, used to calculate these values:

- Average measured force: 51.7 lbs. (230N)
- Equation 4 calculated force: 49.7 lbs. (221N)
- Croasdale-Edwards (empirical): 30.5 lbs. (136N)
- "Bercha" (elastic plate on elastic foundation): 30.9 lbs. (137N)
- Ralston (limit analysis): 40.3 lbs. (179N)

These disparities reinforce my opening statement and illustrate the basic problem of modeling, both mathematically and physically, a complex, inelastic material such as sea ice. Laws of similitude, modeling medium, simplifying assumptions, failure mechanisms, and many other topics must be addressed for each application.

Question: Does the range of variables from the model tests restrict extrapolation of resistance equation results?

Response: The caveat expressed in the previous response also applies to this question. The ranges of variables used in the model tests were selected as representative of nominal expected values for ice strength, thickness, and velocity of the first-year ice inside the Alaskan Beaufort Sea transition zone. The range of waterline diameters was governed by operational requirements. With these ranges, extrapolation is not necessary; only interpolation.
DISCUSSION

By:

Tom Dafoe, Environment Canada, Quebec

What was the nature of the negative environmental impacts that the various groups were bringing to your attention?

AUTHOR'S REPLY

By:

C. Argiroff, Chief, Planning Division, U.S. Army Engineer District, Detroit

The negative environmental impacts as stated by various groups involved the lack of existing baseline environmental data. The most common concerns involved the possible adverse impacts of a winter-time hazardous substance spill, and potential impacts of winter navigation on fish-spawning areas.
"EXTENSION OF THE NAVIGATION SEASON ON THE GREAT LAKES AND THE ST. LAWRENCE SEAWAY SYSTEM" BY PHILIP MCCALLISTER AND CARL ARGIROFF
Vol. 1, Page 107

DISCUSSION
By:
Captain P. Toomey, Canadian Coast Guard, 101 Boulevard Champlain, Quebec, Canada

To whose economic benefit is the extension of the navigation season of the St. Lawrence Seaway? And who is encouraging the studies for such prolongation, is it the inland companies or is it the intention to encourage ocean vessels to use the system for a longer period each year?

AUTHOR'S REPLY
By:
C. Argiroff, Chief, Planning Division, U.S. Army Engineer District, Detroit

Extension of the navigation season on the St. Lawrence Seaway would benefit agricultural interests in both the United States and Canada who depend on the system as an outlet for the export market. Iron ore, coal, and general cargo are other commodities either shipped or received in both countries which could benefit from extended Seaway operations.

The interest in the study is from both national and international users of the Great Lakes-St. Lawrence Seaway System, and by those within the marketing region of the Great Lakes Basin. Interest from users of the Great Lakes (lakers) is for incremental extension to year-round. International shippers/users are interested in a guaranteed opening and closing date and desire an extension of the historical season—but prefer a 12-month season.
DISCUSSION
By:
Walter E. Webb, The St. Lawrence Seaway Authority, Tower A, Place De Ville, Ottawa, Ontario, Canada

It would perhaps be helpful to emphasize that the benefits and costs in the Corps' report are only United States' costs.

Although Canada has not carried out in-depth studies, it did commission an economic study by LBA Consulting Partners. The conclusion of this study was that from a Canadian point of view, the costs would exceed the benefits for a 9 1/2 month season or longer. It only found that "firming up" the present 8 1/2 month season was economically justified for Canada.

AUTHOR'S REPLY
By:
C. Argiroff, Chief, Planning Division, U.S. Army Engineer District, Detroit

The statement on page 7 of our paper (page 113)—"System benefits to costs are favorable to the Nation."—apply to the United States only for this study. The Survey Study relates to U.S. benefits to U.S. costs. The economic evaluation in our study identified benefits only on commercial traffic either shipped or received at U.S. harbors.

In establishing U.S. costs on the Great Lakes boundary waters, two assumptions are made: (1) for the St. Lawrence River, the U.S. would pay 100% of all improvements within the U.S. territorial area and 50% of the total costs for facilities bridging the international boundary. In turn, it is assumed that Canada would pay 100% for improvements within its territorial boundaries and 50% of the total costs for facilities bridging the international boundary; and (2) for the St. Clair River-Lake St. Clair-Detroit River System, the U.S. would pay 50% for required ice control structures and compensating works in the system. The U.S./Canadian cost split is an initial assumption and would be subject to negotiations between the governments.
DISCUSSION BY F.U. HAUSLER ON
"TRANSIT ANALYSIS FOR DELIVERY OF LARGE
BARGES TO ARCTIC DESTINATIONS"
VOL. II Page 136

AUTHOR'S REPLY
BY:
K. Takekuma, Nagasaki Technical Institute Mitsubishi Heavy Industries Ltd.

In the case of the vessels in question on this study, they were non-powered barges. Thus there was no requirement to perform self-propulsion tests to predict full scale performance.

In other ice transitting studies undertaken by MHI & ARTEC CANADA where self-propelled vessels are involved self-propulsion tests have been carried out in the ice model basin.
As a representative of the shipyard that developed, designed and built the icebreaker Ymer I would like to make some comments on the hull damages that Ymer sustained during the Ymer-expedition. It is worth emphasizing that the ship was designed for operations only in the Baltic. Wärtsilä has delivered five ships of the Urho-class to which also Ymer belongs. These ships operate in the northernmost part of the Baltic and none of them have sustained any kind of hull damages due to ice. The first ship, Urho, has been in operation since 1975. The experience in the Baltic as well as in the Arctic with Ymer proves without doubt, that the dimensioning of the hull is optimum for Baltic operations.

A representative of Wärtsilä partipated in one of the trips and his opinions where that the crew did not operate the ship properly in view of the severe ice conditions. More careful handling would have resulted in less damages.

It is worth noting that the critical components of the ship, such as the propellers, shafting, propulsion machinery, rudders and rudder gear were not damaged at all.

The experience from the operation of all five ships in the Baltic as well as the results of the Ymer-expedition show how well the iceloads were known and how accurate the dimensioning criteria were as early as the late sixties when breakers were made at Wärtsilä.
AUTHORS' REPLY

By: G Liljeström, Gotaverken Arendal, Sweden

The authors would like to thank Mr. Mäkinen for his comments on the icebreaker Ymer. We agree it is important to point out that this class of icebreaker is designed for Baltic operations, where their performance is excellent.

The fact that no damages to the hull have occurred in the Baltic but during the extremely difficult conditions in the Arctic, is in our opinion no proof that the hull strength is optimum for the Baltic. The ice conditions in the Arctic were generally 1.5 - 2.0 m thick ice, occasionally even thicker, maximum abt. 4 m, compared to 1.0 m or slightly less in the Baltic.

The relatively low frequency of damages during the Arctic probe, in our opinion shows that large parts of the hull have excessive strength for Baltic conditions.

The fact that the icebreaker would generally have been handled without care, we cannot agree with.
I was interested in the measurements conducted on the Swedish Icebreaker Ymer in Arctic. The authors showed us two slides, one of which is the trace of ice-milling of a propeller on ice and the other is the records of thrust and torque measurements. I suppose you might get many data on thrust and torque fluctuation. Did you find out some specific data which strongly implied the ice-milling condition of propellers? Such data will be useful to promote understanding of propeller-ice interaction.
The authors would like to thank Mr. Sasajima for his discussion of the paper.

During the voyage to the Arctic, the two bow propellers milled ice frequently and especially in the ramming mode on a few occasions the milled ice floes could be seen from the fore deck.

There is no other documentation than slides from these occasions. However, thrust and torque recordings on one forward shaft were made, during normal ice transit. The dynamic loadings were significant but in the same range as other published data.

Our measurements have not been analysed in detail yet.
This interesting paper offers a new approach to an important problem in Arctic sea ice studies, that of inferring the nature of the ice underside (especially bottom roughness or ice thickness distribution) from measurements that can be made from aircraft or satellites. In the absence of an airborne ice-thickness radar the best that can be done is to infer a bottom surface statistic from a measurable top surface statistic, and to date the airborne laser profilometer has seemed the most fruitful instrument since it directly measures top surface roughness. Now LeSchack proposes the use of airborne TIR (thermal infrared) imagery, and this could well become a usable technique after further topside/underside verification. However, with respect to the present study, I have reservations about the technique that he has used.

He is not comparing like quantities. He is relating the skewness of the infrared emission temperature distribution, calculated from multi-year ice alone, to the standard deviation \( \sigma \) of ice draft, calculated from all the ice in the region. Now contributions to \( \sigma \) can come from first-year ice and young ice, and the very variable percentage of young ice in the ten sections suggests a large noise signal from that source. The three physical mechanisms suggested by LeSchack for generating TIR skewness are all functions only of the degree of ridging in the icefield. Therefore he is comparing ridging intensity in multi-year ice with a roughness parameter dependent partly on young and first-year ice. This suggests that it may be more valid to compare the TIR skewness with some other statistic of the submarine profile, but I can find none that offers a correlation. Comparison with more data is necessary to test whether the present correlation is purely fortuitous.
To update the reference list, please note that reference 11 has now been published (Sea Ice Processes and Models, R.S. Pritchard ed., Univ. of Washington Press, 1980, pp. 283-299) and that a more complete analysis of the data from the "Sovereign" cruise has been made in a further paper in press (Wadhams, P., 1981. Sea-ice topography of the Arctic Ocean in the region 70°W to 25°E. Phil. Trans. Roy. Soc., London, A302, 1464-1504). The latter paper includes a correction for the effect of beamwidth, which will reduce the values of \( \sigma \) to below those quoted by LeSchack.

AUTHOR'S REPLY

I appreciate Dr. Wadhams' thoughtful review of my paper. This study, like many in the logistically expensive Arctic remote sensing business, is based on hand-me-down data from disparate sources, where I have had no hand in conducting or controlling the experiment, but merely have had the opportunity to analyze the results. The set of data used in this analysis, like the two data sets alluded to in Ref. 9, have simply been compared statistically to investigate any functional relationship that may exist, since under the circumstances, this is the most reasonable first step in any correlation study.

I have no quarrel with Dr. Wadhams' concerns, and his observation that... "more data are necessary to test whether the present correlation is purely fortuitous..." has been a common one throughout this research. With the first data set (used in Ref. 9), my colleagues and I were doubtful that a meaningful relationship existed, although there was a good correlation with five data pairs in which the submarine data were recorded in 1960 and the satellite data were recorded over the same area in 1974. Therefore, when the opportunity came to correlate submarine data recorded in April 1976 with satellite data of the same area recorded a month earlier, I did so and, using essentially the same parameters, obtained a good correlation, but again, with only a handful of data pairs. The data set discussed in the present paper was used for the third attempt to show a correlation using an independent data set, and again there was such a correlation.

Dr. Wadhams has observed that I have used only multi-year ice in my TIR analysis. With respect to this I observed in Ref. 9 that... at this stage of the research it appears necessary to use only multi-year ice for the correlation analysis; mixtures with first-year ice have distorted skewness patterns that are not easy to correlate with under-ice data as are the pure multi-year ice distributions. Although at first glance, the requirement to choose only multi-year ice distributions for analysis purposes may seem limiting in the context of conducting a large-scale
survey, in fact, the selection process appears easy to do by computer, an important criterion for any operational use of this prediction technique. Figure 10 (in Ref. 9) is a plot of TIR temperature skewness vs kurtosis for the 58 16x16km data blocks chosen. It is significant that essentially all of the data points identified by photo-interpretation as being pure multi-year ice are clustered together, whereas data representing a variety of mixtures of multi-year and first-year ice are scattered about the plot in no order as yet discernible. This appears significant since, as suggested by Figure 10, an algorithm can easily be constructed to select, with high probability, all the pure multi-year ice data in a given population...

I agree with Dr. Wadhams that much more data, collected under more rigorous control, are needed to attempt to move my observations from the realm of scientific curiosity to that of a useful sea ice surveying tool. I hope this may eventually be done.

Reference 9 is available from the U.S. Defense Documentation Center or from me directly at P.O. Box 646, Long Key, Florida 33001. A complete statistical analysis of under-ice roughness data recorded by U.S. nuclear submarines between 1960 and 1962 and discussed in Ref. 14, as well as Dr. Wadhams' HMS SOVEREIGN data recorded from point B (see Fig. 1 of present paper) to the North Pole and then southward, has been conducted by us under Office of Naval Research Contract # N00014-76-C0757, NR 307-374 and is available for study by the interested researcher at the U.S. Army Cold Regions Research and Engineering Laboratory, Hanover, N.H., at the University of Washington, Seattle, and at the University of Alaska, College, Alaska.
Comparison of Pseudo-Parallax and Cross-Correlation for the Computation of Ice Surface Velocities in Northern Waters by E.J. Langham, J.E. Glynn and D.A. Sherstone.

Discussion By:
J.B. Mercer, Dame Petroleum Ltd., Calgary

If the ice floes being photographed should change their aspect during the time between successive photos, a spurious velocity calculation would result. This might happen if the floes are small enough to be tilted by swell or long waves. (I have seen the effect in the Beaufort Sea).

AUTHOR'S REPLY
By:
E.J. Langham, Environment Canada, Ottawa

The problem raised by the speaker is not important in the cross correlation procedure because 1) the ice floes individually don't rise above the water surface to a height which would cause position parallax. 2) The real parallax appears across the flow. 3) The method is based on the correlation of a large number of point measurements (~1000) and the aspect of individual floes is not important.

There is a related problem, however, that is worth commenting on.

1. For small pieces of ice, for which the method is best suited (because otherwise the correlation is one and doesn't change for any points falling within that piece of ice and this upsets the search for a correlation peak) the current across their dimensions is not sufficient to rotate them appreciably within the correlation time.

2. For large pieces this is a problem and the one dimensional method is not suitable. By implication a two dimensional vector velocity field is not unidirectional and divergence and curve would need to be measured unless the flow is through a section (such as a river) with shorelines where the velocity is parallel to the shore as a boundary condition. Then all other components must sum to zero between the boundaries i.e. components that are not downstream don't appear.
GENERAL DISCUSSION BY:
L. Gold, National Research Council of Canada, Ottawa, Ontario, Canada

When considering crack formation in ice a distinction should be drawn between crack initiation and crack propagation. For fracture in tension we are concerned with the conditions associated with propagation. Under certain conditions it is possible to initiate a small crack in polycrystalline ice by a uniaxial tensile stress, which is stable and does not propagate to cause failure. For failure in compression, as cracks that form do not normally propagate, crack initiation and the factors upon which it depends are of interest. In this case it is the crack initiation processes that are responsible for the cracking activity causing determination of the structure and, ultimately, the onset of failure.
DISCUSSION BY:
D.S. Sodhi, CRREL, Hanover, U.S.A.

The tensile strength of ice is indicated in your presentation to be approximately constant. Has there been any attempt made to relate the tensile strength to the fracture toughness $K_{lc}$ of ice?

DISCUSSION BY:
I.M. Bayly, Transportation Development Centre, Montréal, Canada

This paper remind us again of the extreme complexity of ice as an engineering material. Would you please comment on the fiability of effectively modelling the behaviour of ice, either with other material or in scaled down dimension. What properties must be matched or scaled for effective modelling for ship hull/ice interaction and for propeller milling? Is it reasonable to expect to duplicate the fracture pattern? Can useful modelling be achieved without duplicating the fracture mode and pattern?

Thank you for a very interesting lecture.

AUTHOR'S CLOSURE

Dr. Sodhi has raised the interesting question of relating the tensile strength of ice to the fracture toughness $K_{lc}$ of ice. One such trial has been made by Goodman* with some success. The problem is however that the relationship between the two values is not simple and depends on the length of the crack including the
length of the plastic zone at its ends. As this later depends on the physical model used to represent it, you can get a number of different answers in the litterature. Goodman has used Dugdale's model with fairly representative results.

The point raised by Mr. Bayly is a most important one in physical modelling of the properties of ice. In general, modelling is restricted to the behavior of ice in the brittle range and this is particularly true for ship hull-ice interaction in full scale as well as propeller milling. In the brittle range ice behaves very much like a purely elastic material and the simulation then is rather simple because only design values of the mechanical properties are simulated at a reduced scale like the design tensile, compressive strength and apparent elastic modulus of the ice. The design field values have to be determined in advance according to the conditions of operation of the ship (worst, average or least ice conditions and strengths and so on...). The modelling material then has to behave also like a brittle material at the reduced speed of operation in the model. Some artificial material meet this difficult prescription but it is strange to find, that in many cases, ice is the worst material to simulate itself because at reduced velocities it behaves like a ductile material and all its properties and failure modes are completely different that for its brittle fracture in the field. Modelling can be relied upon if it is ascertained in the model simulation that the model material behaves correctly at the reduced speed of the ship's model.

"PLASTIC LIMIT ANALYSIS OF ICE SPLITTING FAILURE"
BY T. D. RALSTON

Vol. 1 page 205

DISCUSSION
by:
G. A. M. Ghoneim, Dome Petroleum Limited, Calgary, Alberta

The author of the paper should be congratulated on his fine work presented in this paper. The following points, however, need some discussion:

A) Either the details or a reference giving such details of the derivation of expressions 2A and 1A of the rate of energy dissipation per unit area on the velocity discontinuity surface, should be given in the paper.

B) The equations used to describe the discontinuity of the velocity on the sides of the wedges shown in Fig. 3 are not quite obvious. Please provide some clarification.

C) The results reported in the paper indicate that the sheet crushing load will govern for Df/W ratios greater than 6 or 8 for warm or cold ice, respectively. In practice, however, large ice floes (1.0 km or more in diameter) are not uncommon, giving rise to much higher values of Df/W. Therefore it is expected that in most cases the floe splitting mechanism will not be the critical one which is needed for design.
DISCUSSION BY G. A. M. GHONEIM ON
"PLASTIC LIMIT ANALYSIS OF ICE SPLITTING FAILURE"

Vol. 1 page 205

AUTHOR'S REPLY
by:

T. D. Ralston, Exxon Production Research Company, Houston, Texas

A) Expressions (1A) and (2A) were originally derived by Prodanovic (1978) and Reinicke and Ralston (1977), respectively. Reference [1] of the original paper also discusses the plane stress and plane strain yield surfaces and dissipation expressions.

B) The failure mechanism shown in Figure 3 contains four rigid regions that are separated by velocity discontinuities. The wedge at the lower platen is stationary and thus has zero velocity. The wedge at the upper platen moves with the platen and has vertical velocity \( V \). The left and right sides of the specimen have equal vertical velocity components and opposite horizontal components. The magnitudes can be expressed in terms of the upper platen velocity \( V \) and the angles \( \beta \) and \( \Theta \) defined in Figure 3b. \( \beta \) is the angle of the lower platen wedge with respect to the horizontal. \( \Theta \) is the direction of the right hand sample fragment velocity with respect to the lower platen wedge surface. The horizontal velocity component has magnitude \( V/2 \tan(\beta-\Theta) \). The vertical velocity magnitude is \( V/2 \). The normal and tangential velocity discontinuities given in the paper follow from these expressions and the geometry of the mechanism.

C) I agree that in most cases the floe splitting mechanism is not the most critical one needed for design criteria. There are two potential uses for this type of analysis in full scale applications.

First, the procedure provides one means to infer large scale ice properties on the basis of physical observations of ice floe splitting events. Of particular interest would be observations that can distinguish between floe sizes that fail by splitting and those that fail by more local (e.g. crushing) mechanisms.
Second, this procedure provides a first step in quantifying the load history that would be generated by an ice cover that consists of ice floes with a distribution of floe sizes. A statistical description of design force events might be generated from a population of force values that correspond to a variety of failure mechanisms and ice conditions. A description of splitting forces for finite floe sizes would be relevant to design criteria development with such an approach.

REFERENCES


DISCUSSION

by:

Donald Haynes, U. S. Army CRREL, Hanover, New Hampshire, 03755

One phenomena we observe on the Yukon River and it is also evident on the Landsat photo is the following: When a large floe impinges upon a structure, the ice will fail in a crushing mode until it is more than half way past the structure then it will fail in a splitting mode. Will the author comment on the accommodation of this phenomena in the plastic limit analysis?

AUTHOR'S REPLY

by:

T. D. Ralston, Exxon Production Research Company, Houston, Texas

The present analysis was only applied to the initial failure of an edge-loaded, square ice sheet; however, the extension to the continuous penetration of large floes is readily apparent. When a large floe moves past a structure (or when an ice breaker penetrates a large floe) local ice failure should occur until the distance between the structure (or ice breaker) and the far edge of the floe is small enough so that the force required to fail this distance by splitting is less than the force required for local ice failure. For the ice properties assumed in this analysis, the splitting results of Figures 5 and 6 would apply to the penetration case if the "normalized ice sheet size" $D_f/W$ is replaced by "normalized distance to the floe's free edge" $L/W$, where $L$ is the distance from the structure or ice breaker to the edge of the floe. Documentation of field observations of such events could possibly be used to estimate the relative values of large scale ice properties.
DISCUSSION
by:

B. Ladanyi, Ecole Polytechnique, Montreal, Canada

Did you take into account the size effect in your evaluation of the results of Brazil tests? One would expect, e.g., that in the brittle failure domain, there would be a tendency towards a strength decrease when the length of the Brazil specimen is increased at the same diameter, because of increased volume of highly stressed material.

AUTHOR'S REPLY
by:

T. D. Ralston, Exxon Production Research Company, Houston, Texas

No. I did not try to account for size effects of the type that you indicate. Theoretically quantifying that type of effect would require a description of the distribution of internal defects and the solution of a rather complex fracture propagation problem. I do not think that the science of ice fracture mechanics has advanced to that stage at the present time.
DISCUSSION
By:

B. LADANYI, École Polytechnique, Montreal, Canada

When speaking about tests performed in the field with field-laboratory oriented testing equipment, it seems appropriate to mention also the potential use of borehole dilatometer tests for obtaining design parameters of ice of comparable quality directly in the field. Experience with such tests in ice has been reported in two previous papers (Ladanyi and Saint-Pierre, 1978; Ladanyi et al., 1979). It was shown in these papers that when the tests are performed in a short-term mode, either stress- or strain-rate controlled, they can furnish the whole stress-strain response of ice in horizontal direction as well as its tensile strength. It is noted that for evaluating such short-term tests no previous assumption on the type of mechanical behaviour of ice is required. On the other hand, for evaluating the borehole expansion creep tests adoption a priori of a creep law is necessary, but it is not limited to any particular creep law.

The tests can be performed with relatively inexpensive, easily transportable equipment. The size of the test cell can be selected to correspond to the problem at hand, either for testing the whole thickness of ice cover at once, or for making a series of small-scale tests along a borehole for getting the changes of ice properties with the depth.

REFERENCES


The borehole dilatometer does provide an additional means of measuring strength and deformation properties of ice in the field. The technique is particularly well suited to establishing the plane strain compression strength and tensile strength with loading in the horizontal direction. It also has the advantage of being in situ and minimizing disturbance of the sample. We would encourage the pursuit of this type of testing.

The reason we carried out uniaxial compression tests in the field was to obtain data that could be compared with uniaxial strength data obtained by other investigators as well as our own laboratory tests.
I would like to comment on the uniaxial compression tests carried out in this field program.

First of all, the test system was a very soft one. This can be observed from Figure 5 of the paper where representative stress-strain and stress-time curves are shown. From these curves, the approximate average strain rates were computed. They were $0.32 \times 10^{-4}$ sec$^{-1}$ for the columnar ice sample, and $0.18 \times 10^{-4}$ sec$^{-1}$ for the granular ice sample. These values were about one order of magnitude smaller than the nominal strain rate of $2.5 \times 10^{-4}$ sec$^{-1}$ computed from the cross-head speed. While part of the difference was due to the machine stiffness, the major contribution is from the use of compliant platens. Because of the softness of the system, neither the strain rate nor the stress rate could be maintained constant. This is one of the disadvantages of field tests, where loading conditions are not very well controlled.

Although the author plotted the test results vs. average stress rate, comparison with other constant stress rate tests should be done carefully, particularly for those tests with ductile failure, where the actual stress rate deviates significantly from the nominal value near the end of the test.

The author has made an interesting point in comparing the test results of first-year ice samples with multi-year ice samples. Strengths of the two kinds of ice were comparable if plotted against average stress rate. However, the multi-year ice strength did not follow the expected trend when plotted against brine volume. This result suggests that brine volume alone is not sufficient to describe the combined effects of temperature and salinity. For instance, if the salinity approaches zero, the brine volume also approaches zero but the ice strength still varies with temperature instead of being a constant as suggested by the brine volume dependency.
The authors have conducted a valuable field study on the strength of sea ice. Additional studies of this type are necessary in order that ice properties be known for the design of ice breaking ships and marine structures in the Arctic. The results from these tests compare very well with those from previous investigations. Future tests should be conducted with temperature and strain rate as variables. Also, tests should be made on ice cores taken in the horizontal (in plane) direction as well as in the vertical direction. If possible a stiff testing machine should also be employed.

It would be useful to have the authors comment on the purpose of the Brazil test. Is it an index for ice strength which can be used in comparing one set of data with another? It would be helpful to know the temperature history of the samples, especially as it relates to brine drainage, and its effect on the strength of the ice. The authors conclude that the compressive strength appears to be a function of grain structure. Could they comment on any corollary with respect to grain size?

The authors are to be commended for their field work and presentation of results.
AUTHORS' REPLIES TO DISCUSSIONS

Reply to Y. S. Wang

Several points have been raised with respect to the influence of test system compliance on the reported results. There is no doubt that the compliant platens contribute significantly to the over-all test system softness.

It should be emphasized that the strain results presented in Figure 5 of the paper are calculated, not measured, so that the relation between average strain rate and nominal strain rate is undoubtedly qualitatively correct but not necessarily quantitatively correct. In laboratory tests at DBR/NRC, using a 100 kN capacity "open-loop" machine with carefully machined specimens on steel platens, the average strain rate is typically one half to one quarter the nominal strain rate. In future field tests it is planned to measure strain on the specimen. With this additional information it should be possible to define more precisely the influence of test system stiffness on the measured results. Nevertheless, loading stress rate, as illustrated by the straight lines on Figure 5, is still a much better basis for interpreting test results than nominal strain rate.

The understanding of the strength behaviour of multi-year sea ice will require new approaches. Brine volume has limitations in its application. Factors such as porosity, air bubble size and shape, and grain size will no doubt be found to be significant.

Reply to F. D. Haynes

The reviewer has raised a number of valid points. Brazil tests are, at best, an index of ice strength and as such can be used to compare data from various types of ice measured in the same way. The purpose in performing the Brazil tests in the field program was to have a value that could be compared with results in the literature. Brazil strength should not be interpreted in terms of a tensile strength.

The samples were recovered from the ice cover and stored at ambient temperature that varied between -30 and -40°C. Provided the sample temperature is less than -23°C, brine drainage is minimal. Brine drainage was not, therefore, considered to be a significant factor in the reported results.

Grain size would appear to have some influence on measured strength values, although the exact nature of this influence is not yet understood. Such investigations should be carried out in the laboratory under controlled conditions rather than attempted in the field.
Dr. McKindra and Capt. Lutton should be complimented on their attempt to obtain the size distribution of broken ice pieces in an ice breakers wake. It is, to say the least, a difficult undertaking and may very well find more value than in just developing a successful clogged channel clearing device. There are some preliminary indications that a vessels icebreaking efficiency may be related to the size of the pieces of ice it breaks. A reliable technique to determine the piece size is required to pursue this line of research. The authors have provided one technique that may be utilized.

Despite the difficulties encountered in obtaining the required data, the authors have presented some idea of what one could expect for mean ice lengths for sheet ice broken by a 140 foot icebreaker. Unfortunately, due to the scatter of the data, it is difficult to draw any conclusions as to the effect of the dimensionless parameters plotted versus piece length in Figures 4, 5, and 6. One would expect that piece size would vary directly with thickness and elastic modulus of the ice and that piece size would be smaller for higher speeds.

I believe it is a mistake to include the brash ice data in the current analysis. Brash ice is a result of the passage of many vessels of various configurations and any attempt to relate the piece size to the 140 icebreaker could be very misleading. I would recommend removing the brash ice data from any statistical analysis.

It would be interesting if the authors could pursue the subject of piece size comparison between the 1/48th model tests and the full scale measurements, particularly in light of the ongoing research related to the effect of elastic modulus on model test results and piece size.
DISCUSSION:

CONDITIONS IN BRASH ICE COVERED CHANNELS WITH REPEATED PASSAGES.

by

Jim Sandkvist - University of Lulea, Sweden.

The author is to be congratulated on presenting a paper which adds to the sparse field data on refreezing of ship channels. In particular it is useful to have the raw data presented so that alternative analyses can be carried out.

The purpose in studying ice growth in ship channels can be considered as two-fold:

- to obtain estimates of the increased volume of ice generated so as to allow for proper design of harbours, wharves, and their associated ice management systems.
- to compute the transit times of ships using refrozen channels.

The author has used his field program data to study the first point in detail, but as a naval architect I could not resist using the data to study the second point.

By plotting the percentage loss in ship's speed as a function of freezing degree days, an approximation to the decrease of ship's speed as follows can be derived for the initial passage through the refrozen channel.

\[ \Delta v = 35 + 0.62 \times \Delta s \]

where \( \Delta v \) = percentage decrease in ship's speed

\( \Delta s \) = freezing degrees days between passages, degrees centigrade.

Other data presented where multiple passes were made in a refrozen channel tend to support the above approximation, since the first pass through a newly broken refrozen channel showed speed losses of 25-50%, and subsequent passes in the same channel showed progressively diminishing speed reductions levelling out at about 15-20%.

The author's analysis of the data to present brash ice growth predictions will be extremely useful to all of us active in naval architecture and marine engineering in Arctic waters. It would be interesting to have the author's comments on the applicability of his semi-empirical degree-day equation to other ship types and freezing regimes, and to know whether he has plans to extend his data base by using different ship types and sizes and by, perhaps, carrying out tests in different freezing regimes at other ports.

Peter Noble

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T2H 2L6.
Plotting the percent loss in speed as a function of freezing degree days gives following figure:

Figure. The percent loss in speed as a function of freezing degree days since last passage.

The plotted results show that the transit velocity is reduced to 50 percent after 24 freezing degree days, in general. Also plotted is the result of using the eq. describing the relation in the figure, the dashed line, giving a residual speed reduction of 35 percent when passing in the channels with no freezing degree days. The field data show that the residual speed reductions caused by the presence of brash ice, when passing several times with no freezing between the passages, vary from 15 to 60 percent.

As described in the paper the field tests were made with only one type of ice breaker. Varying types of hullforms and propulsion systems, etc, will have an influence on the distribution of and the growth of the brash ice.

It would be interesting to extend the field data by using different ship types in different freezing regimes, studying the growth coeff. ALFA. The ALFA contains variables as the common degree day coeff, $C_d$, and factors typical of brash ice conditions: the distribution of water and ice at the surface, the thermal regime, the amount of mush and snow mixed with the water, etc. Knowing more about the growth coeff. the modified degree day method can be applied to different conditions in different areas.
This paper, which obviously represents a tremendous amount of difficult field work, has a great deal of valuable information. The observed size distributions for brash particles are very helpful in defining brash in quantitative terms. The profiles of water content indicate that porosity increases progressively with depth, a characteristic which will now have to be included in theoretical analyses which previously assumed almost constant porosity. Porosity variations are not treated in much detail in the written version of the paper; the statement that surface water content is about 20% is not reflected in the plots of Figure 5, and the very small values of porosity in the upper layers, presumably due to refreezing, are not discussed. The typical cross section of a ship channel in brash, shown in Figure 3A, indicates that two parallel pressure ridges are formed, something which has been observed in other areas. The ship data in Table 1 show how speed was reduced by brash growth, and it would be useful to have a graphic presentation of the data. However, for more complete analysis the total brash thickness is needed, and it would be interesting to have plots of $V_0/V_1$ against total thickness for zero values of $\Delta S$. The significance of $\Delta S$ may be indirect, with speed reductions caused more by grain bonding and decrease of porosity. Incidentally, in trying to read brash thickness values from Figure 7, the vertical scale appeared too big by a factor of 10 on my copy.
Answer:
As described in the paper lots of problems occurred reaching the central parts of the broken tracks for observations just after a passing event. The distribution of water and ice at the surface can be estimated from photos etc. At least in the beginning of the winter the water content at the surface has been estimated to about 20%. The water content will decrease with increasing growth of brash ice in the tracks.
Correlating the data obtained from various experiments with propellers in ice, is an important step towards the understanding of the correct modelling of propeller ice-milling process. In that regard the authors' efforts are highly appreciated.

In the paper, the Authors called the testing in the basin with the propeller, working at bollard condition, an impact test. I think that it is actually ice milling of a similar type to the "dry" milling on the milling lathe. The only difference is that each run on the lathe pertains to one specific relative speed between the propeller and the ice block, and the tank testing covers many more cases in one output as the relative speed of propeller and ice vary. Therefore, the two experiments represent the same process, even in the case of only one blade hitting the floating ice. However, it is a little unrealistic to feed the ice floes to a propeller that is not behind the ship model as their size and motion is not necessarily representative.

The equations that the Authors are using to predict the full scale loading are not clear to me. If we assume that the ratio between the rigidities of the full scale and model scale propeller is at least $\lambda^4$ and the ratio between the full scale and model scale propeller speed is $\lambda^{-5}$ then, it appears that the full scale thrust and torques are multiples of the model results of at least $\lambda^{4.5}$ and $\lambda^{5.5}$ respectively. If so it would not be correct scaling.
Regarding conclusion two on Page 4 of the paper, I find it hard to accept that different thicknesses of blades would create such a significant difference in torque between the two propellers. I wonder if it would also include a difference in shape of the blades, pitch and expanded blade area ratio, in which the two propellers differ. These could definitely create the stated difference in torque reported.

On page 6, the Authors conclude that the curve on Figure 6 takes a roughly sinusoidal shape. From reference [4] in their paper, it can be seen on Figure 17 that the curve actually takes different shape having the value of zero at the angle of incidence equal to negative pitch angle of the blade. Similar results are also reproduced in the POAC paper by Sasajima, Bulat, Glen.

In Conclusion 3 on the same page, the Authors state that CPP is superior to the FPP especially in cases when, due to heavy milling, the propeller is forced to stall or change the effective pitch in case of CPP. Knowing that heavy loads during the ice-milling occur extremely fast, it is hardly of any advantage that the propeller can change the pitch as loading occurs much faster than this can be done.

The Authors answers on the above comments would be appreciated.
Comments by Dr Ralph Norrby\textsuperscript{x}) to the paper
"An experimental investigation of two candidate propeller designs for ice capable vessels"
by T Sasajima, V Bulat and I Glen

This paper adds some information to the sparsely documented field of propellers for operation in ice.

It is questioned if the statement by the authors that the blade geometry is designed in view of the strength against ice load can be used generally for designing propellers to be operating in ice. We have found in a number of cases that the condition that governs the blade strength is the full power and design speed point even for vessels capable of operating in heavy ice. Consequently, each case must be studied separately.

Based on the cavitation patterns shown in figure 4, propeller A seems to be clearly superior to propeller B from the cavitation point of view, which is logical due to type of blade section and blade shape, but it is very hard to understand why propeller B has a higher open-water efficiency than propeller A. Also it would be of interest to know why the contour of propeller B has that particular shape. Could the authors give the lengths of the blade sections for both propellers? Further the important performance at bollard pull ahead and astern for cavitating conditions should have been documented both regarding thrust and torque as well as cavitation patterns and it is suggested that the authors include those results in the discussion.

It is felt that some more thoughts should be made regarding the rather low ice-speed and propeller revolutions used in relation to the scaling law. Further the authors' reflexion on the scatter in indentation tests figure 6, and the effect on ice-milling tests are welcome. For comparative tests the accuracy of ice characteristics may not necessarily have to be high. If that is correct, then figure 7 is valuable as it at least shows the relative ice-load on propeller and shafting as a function of advance angle. However, the relatively large difference in ice-load amplitudes shown in figure 7 is remarkable and explanatory comments by the authors are again required as it is hard to see how the rather small differences in blade sections and contours can give such a result.

\textit{Ralph Norrby}

\textsuperscript{x}) Technical director, KaMeWa AB, Kristinehamn, Sweden
First, my thanks to the authors for their very interesting account of the method and results of comparing the performance of different propellers in icebreaking service. Questions and comments which occur to me relate more to the use of this type of model testing to assess absolute rather than relative values of design loads, which may put them beyond the scope of the subject. Reference to total performance and material selection does suggest some relevance to absolute values, however, so I will raise the question of strain rate and scale effect. Obviously, many propeller/ice interactions occur at very high strain rates, and I notice also that the model ice test results show significant strain rate sensitivity. I wonder then if some method of allowing for this discrepancy, applicable to full scale propeller design, could be explained; also, how do the measured model results for blade bending moment compare to values estimated by the methods referred to (2) and (4) in the paper? I wonder too whether the authors have attempted to adjust their model ice so that its low strain rate properties match the high strain rate properties of real, full scale ice - assuming the latter to be measurable with some degree of reliability.

I understand that blade failures in ice often occur when the shaft is nearly stationary while there is relative motion between ship and ice; which would give rise to the maximum load conditions indicated to be close to 90° and 270°, in fig. 7. Full scale strain rates would be lower and perhaps more correctly modelled under such conditions by the test described; assuming that to be so, what is the maximum full scale blade bending stress indicated by the model test?

Figure 7 shows a substantial difference in the blade bending and thrust values for the two propellers at the maxima close to 90° and 270°; it seems hardly likely that the blade section shape would make a difference at these angles, and the blade profiles hardly seem to vary enough to account for the difference - can the authors explain it?

Some additional queries are:

Were the open water backing characteristics of the two propellers not tested? If not, does this mean that the blade sections are symmetrical enough to allow identical characteristics to be assumed for both directions?

What is the fatigue strength of the "MSS" stainless steel selected?

In addition to crushing strength for both, shear strength is given for real ice and flexural strength for model ice: is there a relationship or equivalence between the two? Unfortunately, many of the references quoted are not readily available, so it is difficult to look up details not included in the paper.

My thanks to the authors again, I look forward to their reply.

Ian M. Bayly
I compliment the authors for addressing a very important and timely subject, propellers used in ice service. Propellers for icebreakers and icebreaking cargo vessels, particularly in high power ranges, are probably one of the most significant problems confronting the designer and operator in developing the polar regions.

Of all the aspects in designing propellers for icebreaking, the loads while milling ice are the least understood. The authors show in figure 8 a comparison of ice milling loads for two propellers with different blade sections but almost identical in every other design parameter. The results vary widely, in some areas there are 100% variances in the bending moments, thrust and torque between the two propellers. The differences are greatest where the angle of attack presents the face of the blade to the ice rather than the leading or trailing edge. It would appear that the blade section shape would have the least effect on the loadings in these areas.

The authors state that ice is encountered most frequently at angles of attack of 0-40 and 170-210 degrees. Those two areas of the curves plotted in figure 8 show the best correlation between the two model propellers.

This raises a few questions:

1. How do the authors account for the large differences in experimental ice milling results?
2. What statistical evidence do the authors have to support their statement about most frequently encountered angles of attack?
3. Based on their results what blade bending moments would they suggest for design purposes?
4. What blade section was selected for the propellers for the new vessel?
Japanese icebreaker?

I thank the authors for their contribution and for the opportunity to comment.

Donald G. Langrock

Commander, U.S. Coast Guard
Authors reply to the discussion


Discussors: Mr. Norrby, R., KaMeWa AB, Sweden
Mr. Bayly, I.M., Transport Canada, Canada.
Mr. Langrock, D.C., U.S. Coast Guard, U.S.A.

The authors appreciate the discussors' contributions to our paper.

Mr. Norrby's comment that propeller geometry design must be studied case by case is reasonable. If we design the propellers for ice-going vessels following the rules of the classification society, it is true that the full power and the design speed are the key factors for designing blade geometry. But it is our contention that the blade strength should be checked by using ice load, taking into account the relative strength to the propeller shaft. The propellers for the Antarctic Research Vessel of Japan have been designed using the procedure mentioned in the paper [1].

The purpose of this comparative study was to study if the lenticular blade section with the blade contour as shown in Figure 2, introduced by Ignatev [2], as having good characteristics in ice, has acceptable capability in open and ice covered sea, in comparison with a conventional propeller. It is a little hard to explain why propeller B has a higher open-water efficiency. The small camber ratio and blade contour may be responsible, but further analysis will be necessary.

As to the dimensions of the propellers, we cannot include all the values, but the following data will be useful for the discussor.

<table>
<thead>
<tr>
<th>r/R</th>
<th>Propeller A</th>
<th>Propeller B</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.4</td>
<td>c/R 0.5854</td>
<td>0.6245</td>
</tr>
<tr>
<td></td>
<td>t/c 0.0816</td>
<td>0.0784</td>
</tr>
<tr>
<td>0.7</td>
<td>c/R 0.7236</td>
<td>0.7049</td>
</tr>
<tr>
<td></td>
<td>t/c 0.0430</td>
<td>0.0417</td>
</tr>
</tbody>
</table>
We did not conduct cavitation tests at astern condition, since the blade contour and blade section are symmetrical or very close to symmetrical to mid-chord. We think that the cavitation patterns at bollard pull astern are not as bad as those for the bollard pull ahead condition.

Strictly speaking, the bollard condition of the propellers could not be simulated in the cavitation tunnel due to recirculating flow caused by the model propeller itself. So the operating condition of the propellers, the sketches of which are shown in Figure 4, were as follows:

- Propeller A: \( K_T = 0.286 \) (\( s = 0.75 \))
- Propeller B: \( K_T = 0.285 \) (\( s = 0.75 \))

Mr. Norrby and Mr. Bayly asked about the strain rate and scaling law problem for the propeller milling in ice. As mentioned already, the objective of this project was to compare two propellers and only evaluate their relative performances in ice. For that reason, the strain rate of model ice as well as its shearing strengths were not examined. Therefore any attempt to extrapolate the results to full scale could bare errors. Nevertheless, if the angle of advance \( \alpha \) and uniaxial crushing strengths of the full scale ice are known, the following scaling laws are applicable.

\[
T = \bar{T} \times \lambda^2 \times \sigma_{cu} \\
Q = \bar{Q} \times \lambda^3 \times \sigma_{cu} \\
BM = \bar{BM} \times \lambda^3 \times \sigma_{cu}
\]

where, \( \bar{T}, \bar{Q} \) and \( \bar{BM} \) are normalized thrust, torque and bending moments respectively,

\( \sigma_{cu} \) is full scale uniaxial crushing strength of ice.

A design value check was conducted using eqns. (2) and (4), in response to the question by Mr. Bayly. Both propellers were designed at an ice-milling torque of 1.225 MNm (at 66% blade length cut) at an advance angle \( \alpha \geq 10 \) degrees. If we use the scaling law mentioned above, extrapolated bending moment from the model data is as follows:

\[
BM/\sigma \text{ exp.} = 0.4804 \ (m^3)
\]

while the design value is:

\[
BM/\sigma \text{ des.} = 0.75 \ (m^3)
\]
This correlation is considered reasonable for the present state of experimental evidence.

Adjustment of strain rate to full scale from model ice is as follows. Figure 7 of the paper demonstrates the relative performance of the model ice used for testing. The results of indentation on piles (in full scale) can be correlated with the results from confined crushing strength tests using the bore-hole jack. Since we have related our indentation tests on model ice with these same full scale tests, the results for the full scale bore-hole jack tests can be related to our indentation tests in model ice. Full scale loads can thus be derived.

Blade bending stress at design and at maximum ice load conditions are calculated and shown in the following table.

<table>
<thead>
<tr>
<th>Condition</th>
<th>Blade stress</th>
<th>Propeller A</th>
<th>Propeller B</th>
<th>$\alpha_v$ (deg.)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Design ice load</td>
<td>$\sigma$ (MPa) $\sigma/\sigma_y$</td>
<td>192</td>
<td>2.8</td>
<td>10</td>
</tr>
<tr>
<td>Maximum ice load</td>
<td>$\sigma$ (MPa) $\sigma/\sigma_B$</td>
<td>3840</td>
<td>4.6</td>
<td>3350</td>
</tr>
</tbody>
</table>

$\sigma_y$; 0.2% proof stress of material (MPa)

$\sigma_B$; Tensile strength of material (MPa)

These results show that almost the same level of blade stress was obtained at the design ice load condition for both propellers, since the safety factor of 3 was used in the design. At the maximum ice load condition, the estimated results show that the blade can not sustain such a high ice load and will fracture.

The large difference in blade bending moment between the two propellers near $\alpha_v = 90$ and 270 (degrees) was queried by the discussors. The authors believe that the blade contour shape will have more effect on the blade bending moment near these advance angles than the shape of the blade section. Figure 9 compares the projected blade shape, and the distribution of projected area clearly indicates why the bending moment compares in this way.

In response to the additional questions by Mr. Bayly, our answers are as follows:-

(1) The backing characteristics were not examined for the reason stated by the discussors.
(2) The fatigue strength of MSS (Mitsubishi Special Steel) is as follows:

\[ \sigma_f = 290 \text{ (MPa) } \left[ = 30 \text{ kg/mm}^2 \right] N > 10^8 \text{ in sea water} \]

\[ \sigma_f = 390 \text{ (MPa) } \left[ = 40 \text{ kg/mm}^2 \right] N > 10^8 \text{ in sea water with cathodic protection} \]

(3) The shear strength of the Antarctic ice was only presented in the paper to give some idea of the strength of ice. No special relation exists between the shear strength of the sea ice and the flexural strength of the synthetic ice.

The questions by Mr. Langrock, have already been answered to some extent. In response to the question on assuming the range of the ship speed from 2 to 7 kn and propeller speed from 140 to 40 rpm, we get the range of angle of advance \( \alpha \) as follows:

\[ \alpha = 1.6^\circ \text{ (Ship speed 2 kn and propeller speed 140 rpm)} - 19.3^\circ \text{ (Ship speed 7 kn and propeller speed 40 rpm).} \]

These are the most frequently encountered situations in which the propeller ice-milling is occurring. Usually they happen during ramming the multiyear ridge or thick level-ice breaking.

Addressing another point, according to the experimental results which do reflect truly the full scale situation, the root blade sections would have to be manifold thicker than they are if we wanted blades never to fail. However it is more reasonable to design the blades so that they can sustain the loading up to 30 degrees of angle of advance and also to provide enough power to be able to overcome excessive torques at the deeper cut depths so that the engines can be speeded up and therefore the effective \( \alpha \) brought down to design range around 30 degrees or less.

The propellers, which are going to be fit to the new Antarctic Research Vessel of Japan "SHIRASE" were finally designed by the people in NKK [1]. We are manufacturing the propellers. So we are not the position to mention about the geometry of the actual propellers in detail. But I guess that the contour shape and blade sections of the propellers are similar to the propeller A.

Again the authors wish to express their appreciation to the discussors for their contribution to our paper.
Fig. 9  Comparison of projected contour of propeller blade
Has the floating breakwater been considered as an alternative concept for your "PACK ICE BARRIER"?

AUTHOR'S REPLY:

The author would like to thank Dr. D. V. Reddy for his discussion of the paper.

As he pointed out we have considered some floating pack ice barriers as alternative concepts. Some of those are shown in Figure A.

These concepts were reviewed for using them at coastline of Okhotsk sea of Hokkaido as shown in the paper.

We recognized that these floating type of pack ice barrier have some following demerits.

1) It is difficult to keep the structure stable against ice force despite of using mooring cables which act as decreasing the motion of the floating structure.

2) If the floating structure has a slope to break the ice by bending for reduction of ice force, weight of pile-up ices on the floating structure will affect to push and rotate the structure down.

3) The floating structure has to withstand heavy wave loads in summer if the structure would not be moved away.

Generally taut cable system for mooring is not available for large floating
structure because of heavy dynamic wave load.

Despite of the above mentioned demerit of the floating barrier, in the case of moderate ice condition moored floating boom system may be available.

(1) REMOVABLE FLOATING CAISSON

PACK ICE

(2) MOORED FLOATING CAISSON (BOOM)

PACK ICE
(3) MOORED FLOATING STRUCTURE

PACK ICE
The program described by the author was very important for the equipment that was developed, as well as for the data that it generated. The original hydraulic loading system and supporting structure have undergone various modifications from year to year, and have been used on lake ice in Canada and on sea ice near Prudhoe Bay, Alaska for a variety of indentation tests, continuous crushing tests, in situ testing of ice pressure sensors, and large scale ice property tests.

The test in which the stress distribution through the ice sheet thickness was measured is of particular interest. Since this test was conducted in warm test pond ice, the crystallographic structure and temperature of the ice were homogenous through the thickness. The uniformity of the measured distribution confirms that the mechanical response due to the loading is uniform for homogenous ice properties. Additional tests of this type in ice with non-homogenous properties would also be useful for quantifying the effect of stiffness and strength variations in larger scale ice/structure interactions.

The data analysis that indicates a significant size effect for ice crushing strength does not include the effects of strain rate. Most of the tests were conducted with a constant rate of separation between the load plate and the backing plate; however, the absolute motion of the load plate was not measured. If we assume that the tests are constant indentation rate tests, then the analysis discussed by Michel and Toussaint (1977) should apply. Their work implies that for a fixed indentation rate, and a fixed ice sheet thickness, the indentation pressure should be proportional to \( (D/T)^{-0.32} \). This dependence is illustrated with the test data in Figs. 1 and 2. As in the subject paper, these curves were normalized by the results of Tests 23 and 24R, respectively. This rate effect describes the decrease in crushing pressure with increasing aspect ratio at least as well as the theoretical curves given in the paper.

REFERENCES

Discussion by M. Mellor, CRREL, USA

The Exxon group have succeeded in the very difficult task of making precise mechanical tests on sea ice, and they have provided a tantalizing glimpse of the data obtained.

An interesting thing about the stress/strain curves shown in Figure 3 is that peak stress and failure occur at small strains, say less than 0.2%. Our recent tests on isotropic fresh water ice at -50° usually showed two peaks, or stress drops, on the curves for strain rates up to about $10^{-4}$ s$^{-1}$, the first at small strain and the second at about 1% strain. As strain rate increased, the initial yield point eventually became dominant and second finally disappeared. The strain data in Figure 4 are also very interesting, providing convincing evidence that columnar ice does indeed behave like a bundle of pencils lightly glued together.

The compliant platens used in this study were designed strictly for simple field tests; they were never intended to be used in precise laboratory testing. To avoid the need for precise centering jigs, the Flexane was recessed and there was a generous annular clearance between the specimen and the aluminum. In principle, suitably designed compliant platens can be used in precise testing as long as the stroke control of the machine receives signals from displacement transducers mounted directly on the specimen. However, at high rates, any compliant elements in the systems will degrade the feedback response rate of the servo. In considering the disturbance of radial strain records when compliant platens were used, it is necessary to point out a flaw in the Exxon testing technique. The radial displacement transducers were fixed relative to the lower platen, so that the pickup points slid relative to the ice surface as axial strain
occurred. This may be unimportant for very small axial displacement, but the effect would be accentuated by putting a rubber plug under the specimen.

Having done with the minor complaints, one must congratulate Dr. Wang and his colleagues, and hope that we shall soon see their detailed results.

Author's Reply

The author realizes that the flexane platens were designed primarily for field tests and were not intended to be used in laboratory tests. However, it was felt that they could be used to indicate whether the sample finish, particularly the end surfaces, was adequate. If the sample finish had not been adequate, then using flexane platens would have made a difference in the test results. Since no significant differences were seen in tests with and without these platens, we concluded that the sample finish was adequate.

The author agrees that the lateral strain measuring device was not ideal. Because it was fixed on the lower load platen, the measurement was accurate only when the deformation was moderate. To accommodate large deformation, the measuring device would have to be fixed on the sample. Such a device would be more difficult to design and would provide only marginal benefit to the present work.

Discussion by N. K. Sinha, National Research Council of Canada

It is pleasing to note that the author's experience coincides well with ours at DBR/NRCC, which contributed much to the IAHR recommendations on the subject of ice strength testing. His discussions on Poisson's ratio, however, require some comment.

Traditionally, Young's modulus, $E$, and Poisson's ratio, $v$, are associated with the average distortion of the lattice and are practically independent of time. The values of $10 \text{ GN} \cdot \text{m}^{-2}$ for $E$ and 0.3 for $v$ are reasonable for dense polycrystalline ice. These are for what we might call pure elastic loading conditions, which we seldom encounter; nor do
we face conditions of pure viscous flow at large strains under which the effective modulus, $E_e$, defined as the ratio of stress and strain, approaches a negligible value and the effective strain ratio, $v_e$, defined as the ratio of the transverse and the longitudinal strain, approaches a value of 0.5.

The anisotropy in $v_e$ (strictly speaking 'not' Poisson's ratio) in columnar grained ice is not only expected, as shown here, but recognized as early as 1958 (L.W. Gold, Canadian J. Physics, 36 (10), p. 1265-1275, 1958). Gold reported a value $v=0.88$. The present author should be congratulated for confirming the earlier observation and for extending it to sea ice.

The contribution of the grain-boundary or interplatelet sliding (gbs) strain to the total strain, in addition to the pure elastic and viscous flow, influence both $E_e$ and $v_e$. Analyses (N.K. Sinha, POAC 81) indicate that $E_e$ depends on stress and strain rates, temperature and grain size. These analyses can also be extended to $v_e$. For columnar-grained ice (e.g., first year sea ice) loaded perpendicular to the columns, the interdependence between the two lateral strains (one parallel to the axis of the columns and the other normal to it) and their relationship with the longitudinal strain can be estimated by giving consideration to textural anisotropy. It can be shown that $v_e$ depends on rate of loading. Figure 4 of the paper under discussion in fact confirms the rate sensitivity of the degree of anisotropy in the lateral deformations. The author did not, however, discuss this subject.

**Author's Reply**

The author admits that the term "Poisson's Ratio" was used loosely in the paper to represent the ratio of lateral strain to axial strain, which might be called "lateral strain ratio" to avoid ambiguity. It is reasonable to expect that the anisotropy of this ratio will decrease as strain rate increases since at higher strain rate, the material behaves more elastically and elastic deformation does not involve as much grain boundary sliding, which is the main source of anisotropic deformation. At very low strain rate, on the other hand, the deformation will be mainly viscous flow. Again, less anisotropy will be expected with this type of deformation. The data presented in Figure 4 of the paper does
show a decrease of anisotropy as strain rate increases from $10^{-6}$ sec$^{-1}$ to $3 \times 10^{-5}$ sec$^{-1}$. This phenomenon will be further investigated in future tests.

Discussion by J. J. Kolle, Flow Research Company, USA

Uniaxial compression tests on sandstone and ash-fall tuff commonly give an apparent value of Poisson's ratio in excess of 0.5. These results are associated with micro-cracking and dilatation in rock. The values for Poisson's ratio of 0.8 to 1.2 suggests that micro-cracking and consequent void formation must be important in unconfined sea ice tests.

Author's Reply

This is apparent when the strain rate is $10^{-5}$ sec$^{-1}$ and higher. At lower strain rates, say $10^{-6}$ sec$^{-1}$, no apparently visible cracks can be seen after a sample has been tested. Presumably, the primary deformation mechanism in this range is grain boundary sliding with little cracking activity.
DISCUSSION

By:

J. Molgaard, Memorial University of Newfoundland, Canada

As a newcomer to the subject of ice mechanics, I am wondering why the average value of fracture toughness obtained in experiments such as yours is significant.

You report results which do not exhibit a distinctive distribution about a mean value, i.e., normal, resembling a Gaussian or other recognizable statistical distribution to indicate the operation of random 'errors'.

I am also reminded of the kind of approach one can adopt to tensile tests for the tensile strength of glass whiskers. Occasionally one may come across a perfect whisker, which will therefore exhibit the highest tensile strength which may be the 'true' value. Is it not possible that the greatest value obtained in a series of fracture toughness tests will be the most significant value? That is, the value least affected by extraneous disturbing effects in the experiment.
This discussion again points out the need for a better means of introducing a starter crack in an ice fracture specimen. In computing means and variations about the mean for our fracture toughness values, I have assumed a normal distribution based on previous fracture toughness data, which do appear to show a Gaussian distribution, and the assumption that any scatter is due to random experimental influences and the random distribution of crystal orientations about the preferred orientation indicated in Figure 1.

It is possible that in these tests the minimum value of fracture toughness is more significant because the true fracture toughness requires nucleation of the sharpest possible crack. This would correspond to fracture in an H crystal orientation if in fact H crystals have a lower fracture toughness. Any effects of failing to produce a sharp crack should affect the apparent fracture toughness in the same way for both sets of crystal orientations since the beams were prepared in the same way. Thus, the observation that apparent fracture toughness is significantly higher for the V orientation would still indicate a significant difference in true fracture toughness.
"FRACTURE TOUGHNESS OF ICE: CRYSTALLOGRAPHIC ANISOTROPY" By J. J. Kolle

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DISCUSSION
By:
K. J. Miller, Department of Mechanical Engineering, Sheffield, U.K.

The work described by J. J. Kolle is a welcomed extension to our knowledge on the fracture behavior of ice, especially the possible effect of crystal orientation on fracture toughness.

For the purpose of comparison, the results of six other workers are presented in the attached figure, the majority of results being taken on three point bend specimens of Roslin Glacier ice, north east Greenland during the 1978 Sheffield (U.K.) University Expedition. The tests and field equipment used are described elsewhere (17). Also included in the attached figure is the scatter band of results by Goodman (10) who tested 128 specimens over a wide range of fast loading rates and three temperatures.

With regard to these results and those in the paper presented above, several points are worth noting:

1. More tests need to be performed to clearly establish the effect of crystallographic anisotropy on fracture toughness especially when there is such a wide scatter in all tests performed to date.

2. Stress intensity rate may be a more meaningful parameter to assess fracture toughness than either loading rate or straining rate since the fracture process occurs at the crack tip rather than at the outer fibers of the bending specimens.

3. From the results presented I would not yet agree that "the fracture toughness ... is significantly higher for $S_1^V$ than for $S_1^H$ ice".

REFERENCE

Fig. 11.1
FRACURE TOUGHNESS AS A FUNCTION OF STRESS INTENSITY RATE
DISCUSSION BY K. J. MILLER ON "FRACTURE TOUGHNESS OF ICE: CRYSTALLOGRAPHIC ANISOTROPY"
By J. J. Kollé
Vol. I, page 366

AUTHOR'S REPLY
By:
J. J. Kollé, Flow Research Company, U.S.A.

In response to the three points made by K. J. Miller:

1. I certainly agree that more tests should be made to define the effect of crystal orientation on fracture toughness. D. Goodman has suggested that a V-notch should be formed at the base of the saw cut in addition to nucleating the crack with a razor to reduce the scatter associated with stress concentrations near the corners of the saw cut. Ideally a fatigue crack should be introduced using a stiff load frame to provide the most realistic starter crack.

2. Stress intensity rate is indeed a more meaningful parameter for comparing fracture toughness data. In our tests the stress intensity rate ranged from 1.5 to 5.0 kPa m/s depending on notch depth. However, strain rate is most meaningful for applying our results to the engineering problems of failure loads in ice structures.

3. If our fracture toughness results are normally distributed, there is a significant difference in apparent fracture toughness at the 95% confidence level. There is some question, as discussed previously, as to the appropriateness of this distribution which should be resolved by further experiment. Nonetheless, the results are suggestive and should be pursued.
Discussion on "Transverse Pressure Effects on An Embedded Ice Pressure Sensor" by A. C. T. Chen

Discussed by Y. S. Wang
Exxon Production Research Company
Houston, TX, U.S.A.

The author has shown that the ice pressure sensor developed by Exxon and used in its field projects has very little cross sensitivity and can correctly measure the ice pressure in the sensing direction. This is an important characteristic of the pressure sensor in that no ambiguity exists in the analysis of the data collected. The following comments or questions are mainly for clarification and more information on the analysis.

1. The ice is assumed to be an elastic-plastic material in the analysis. This is a reasonable approximation when the pressure build up in the ice sheet is gradual. If the pressure builds up very fast, then the ice may behave more like an elastic material and the resultant transverse pressure coefficient may be closer to the value corresponding to the "initial yield pressure" in Table 2. However, even in this case, the cross sensitivity is still insignificant.

2. The "initial yield pressure" resulted from the analysis probably depends upon the sizes and shapes of the finite elements used in the analysis. It can be used as a reference pressure in the computation but should not be given undue physical significance.

3. The "transverse pressure coefficient" in Table 2 is calculated by dividing the average sensing face pressure by the applied transverse ice pressure. This implies that the pressure sensor measures the average pressure across its face plate.

4. The way that the transverse pressure coefficient is calculated seems to indicate that the pressure sensor can measure tension as well as compression.
AUTHOR'S REPLY
By:
A. C. T. Chen Exxon Production Research Company USA

The author would like to thank Dr. Wang for his discussion. The author agrees with the comments described in Items 1 and 2.

In regard to the comment in Item 3, it is true that the EPR pressure sensor does not measure the average pressure across its sensing face exactly. However, in the usual treatment of the data, it is assumed that the measurement is proportional to this average pressure. On the basis of field response tests and our previously published theoretical work [4], the error incurred with this assumption is known. It is small enough to be neglected in the analysis of transverse pressure effects.

In regard to the comment in Item 4, the EPR pressure sensor is not designed to measure tension. However, as a result of the way the sensor is built with preload, it does respond properly to a certain degree of tensile sensing face stress, and that degree is greatest near the edges. Furthermore, it is expected that a pressure sensor would generally be subjected to ice pressure both in normal and transverse directions. The normal ice pressure would induce a sharp compressive stress concentration at the edge of the sensor [4]. Therefore, in examining the transverse effect of the pressure sensor, the tensile stress concentration at the edge of the sensor caused by the transverse ice pressure needs to be considered, because this tensile stress concentration would negate the compressive stress concentration.
MECHANICAL PROPERTIES OF LOW DENSITY
ICE UNDER CYCLIC AXIAL LOADING

DISCUSSION BY

Hamdy Youssef  Génie Civil, École Polytechnique de Montréal
The University of Montreal, Québec, Canada

Cold regions are rich in natural resources, particularly in oil and natural gas, which are vital to the Canadian and American economies. The tapping of these resources often requires large engineering projects of which the Alaska pipe line, is an example. The execution of these projects requires knowledge of the properties and deformation of frozen ground used for structural foundations.

The analysis of the response of frozen ground (ice and frozen soils) to dynamic loading conditions arises in connection with earthquake loadings or vibrating machinery requires determining appropriate material properties. In this respect, the experimental results shown in the paper by Vinson and Chaichanavong are very interesting and represent a valuable contribution to the low frequency ice dynamics literature. They show very clearly the dependency of the dynamic Young's modulus of ice on confining pressure, density, frequency, temperature and strain amplitude, i.e.

\[ E_d = F_1 (\sigma, \gamma, f, \Phi, T, \epsilon) \]

and the damping ratio on frequency, temperature and strain amplitude, i.e.

\[ \lambda = F_2 (f, \Phi, T, \epsilon) \]

However, the dynamic properties of ice (as well as of frozen soils in previous studies, Vinson, 1978) are considered constant with time. Since, ice is an ideally flowing solid and therefore stresses and strains due to the influence of an external load are not constant, but vary with time; the writer appreciates the authors comments on the

1 By Ted S. Vinson and Thira Chaichanavong (Proc. Paper - POAC 81)
stability of the dynamic properties of ice with time and the capability of the used testing technique (cyclic triaxial method) for investigating the influence of the time parameter. This information is important if the experimental dynamic properties are used as an input to suitable analytical technique to predict the dynamic creep behaviour of ice and frozen soils.

Due to the increasing demand for investigation of the dynamic behaviour of frozen ground, the number of ice and frozen soil dynamics laboratories is increasing. In order to make the exchange of knowledge easier and to enable a comparison of data it is advantageous to researchers to utilize the same technique. This will eventually lead to standard testing equipment and methods. The writer believes that the resonant-column method is capable of investigating the dynamic properties as well as the dynamic behaviour of ice and frozen soils. Youssef et al. (Youssef 1979 and Youssef and Kuhlemeyer 1981) developed an apparatus (Calgary apparatus) for laboratory testing of the dynamic properties (dynamic shear modulus and damping ratio) as well as the effect of the torsional vibration on the static creep rate of ice and frozen soils. This was achieved by utilizing the Drnevich (1976) free-free resonant column as a vibration device. If the more recent Drnevich (1978) resonant column is used instead, vertical as well as torsional vibration could be applied to cylindrical frozen sample, accordingly the dynamic Young's modulus, dynamic shear modulus and damping ratio can be determined at the sample resonant frequency. Moreover, Stevens (1975) showed that in the resonant-frequency technique it is possible to determine the wave velocity in a cylindrical sample in the non-resonant conditions.

The proposed testing technique should be capable of permitting investigation the influence of the following parameters: (1) temperature, (2) strain or deformation with time, (3) axial and confining stresses, (4) frequency and amplitude for longitudinal and torsional vibration, (5) different material type and composition, and (6) the dynamic properties during different stages of the test. The writer appreciates Prof. Vinson opinion regarding the proposed testing technique.

REFERENCES
(1) DRNEVICH, V.P. (1978), "Resonant Column Test", Report No. S-78-6, Geotechnical Lab., U.S. Army WES, Vicksburg, Miss, U.S.A.

The authors have presented similar results on denser ice elsewhere. The use of low density ice makes this work interesting.

Young's modulus for any polycrystalline material results from primary bond distortion or average of lattice deformation. It is known to be practically independent of time and only slightly dependent of temperature. Ice is a polycrystalline material and is expected to behave like any other polycrystalline material. It would be useful if the authors could give an explanation as to why the Young's modulus of ice and the damping ratio, as reported here, depends so much on time (frequency), temperature and confining pressure. Moreover, it would also be beneficial, from the practical point of view, to know a method of extrapolating these results to conditions other than the ones used in the present investigations.

The authors have shown that the longitudinal wave velocity, $V_L$, and the compression wave velocity, $V_P$, calculated from the experimental values of $E_d$, underestimate the values determined by previous investigators. Could this be due to the fact that the equation $V_L = \sqrt{E/\rho}$ applies only when $E$ is the pure elastic modulus associated to the deformation of the lattice and not an effective modulus that might reflect additional deformation mechanisms?
The authors appreciate the discussion by Messrs. Youssef and Sinha. Youssef requests the authors' comments on "the stability of the dynamic properties of ice with time and the capability of the testing technique (cyclic triaxial method) for investigating the influence of the time parameter." The authors did not conduct tests over extended periods of time. Over the short time intervals associated with the conduct of a test (generally less than one minute) the dynamic properties of ice were stable with time. The cyclic triaxial test method could be used to investigate the effect of time (period) of loading on dynamic properties of frozen materials. With the test system both stress and strain control tests may be performed. A recent paper on the effect of duration of cyclic loading on resultant properties of isotropic polycrystalline ice has been presented by Mellor and Cole [8].

Youssef suggests that researchers involved in dynamic property evaluation of frozen soil standardize their test methods to facilitate exchange of information. The authors agree with this suggestion.

Sinha requests "an explanation as to why the Young's modulus of ice and the damping ratio, as reported here (i.e., authors' paper), depends so much on time (frequency), temperature and confining pressure." The values of Young's modulus reported in the paper reflect deformation mechanisms that include, but are not limited to, "primary bond distortion or average of lattice deformation." These additional deformation mechanisms might reflect opening between the ice grains or grain-boundary sliding [9]. The additional deformation mechanisms are dependent on frequency, temperature, and confining pressure, and, therefore, dynamic properties associated with the deformation mechanisms would reflect a similar dependence.
Sinha requests "a method of extrapolating these results (i.e. presented in paper) to conditions other than the ones used in the present investigation." At the present time, the authors have not attempted to develop a method to extrapolate the results to other conditions.

Sinha expresses the thought that the lower values of compression wave velocity reported in the paper might be due to the elastic modulus reflecting additional deformation mechanisms than associated with previous studies. The authors agree with this thought and note that the additional deformation mechanisms are undoubtedly associated with the greater strain amplitudes of loading inherent in cyclic triaxial testing.

References
SURFACE WIND DIRECTION ANOMALIES ALONG THE ALASKAN BEAUFORT SEA COAST

By Thomas L. Kozo
Vol. III

DISCUSSION BY:

E. F. Roots Department of Environment Ottawa, Canada

Is there information on the nature of the transition between typical winter and typical summer conditions? Presumably, the switch from winter to summer takes place in a fairly simple and definite manner, while the change from summer to winter is gradual, confused and perhaps repeated several times each fall. Does the change take place in a series of separate cells in the ocean shore or mountain front, which coalesce and migrate, or is there just a period of general turbulence caused by breakdown of the vertical structure of the lower atmosphere?

AUTHOR'S REPLY

There is little data available in the general literature, but meteorological studies for oil companies of a proprietary nature along the Beaufort Sea coast are accumulating vast amounts of data which could be useful. In a paper by Kozo (1980) a graph of the average daily albedo over the arctic tundra indicates that the albedo transition from winter to summer is indeed simple and fast (less than one week), while the transition from summer to winter is more gradual with several false "starts" taking more than one month.

The second question is an extremely interesting one that I can't answer, and requires an effort beyond any single budget I've seen, but with a coordinate program involving several institutions, it may be possible to devise an experimental plan.

This winter I will be involved in a project for the National Science Foundation in the Barter Island area. We will attempt to look for precursors to, and horizontal extent of (especially seaward), the mountain barrier wind reversals.
How much of the direction anomaly can be explained by the thermal wind?

AUTHOR'S REPLY:

The thermal wind refers to the vector difference between the geostrophic wind at two levels. The thermal wind results when the pressure gradient at the surface differs significantly from that at the free stream (geostrophic wind) level due to strong horizontal gradients in temperature. Evidence presented in Kozo (1979 and 1980) and some evidence still to be published, have shown that calculated geostrophic winds from surface pressure network data agree quite closely with simultaneously measured free stream winds in the Prudhoe Bay area and those at Barter Island (250 km east). It must also be remembered that in the arctic, the depth of the planetary boundary layer will often be less than 400 meters, even in the summer, (especially over water) so that the wind direction seen in Figures 6 and 9 above the inversion layer are quite representative of the free stream winds.

The rotary spectra graph in Figure 5 and the profile in Figure 6 showing temporal clockwise rotation at the surface are ample evidence of seabreeze effects for the summer work. Thermal wind induced counterclockwise turning of the geostrophic wind with height (backing) associated with cold air advection is not exhibited in the pilot balloon data in Figure 6 (shows clockwise rotation from the surface to 200 meters). The complicating factors of monsoonal effects, thermal winds, and seabreezes in combination cannot be ruled out however.

The winter mountain barrier phenomenon is most certainly a thermal wind produced effect (Schwerdtfeger, 1974), but due to rawinsonde data being taken at standard heights, we cannot determine backing or veering from data such as seen in Figure 9.
SURFACE WIND DIRECTION ANOMALIES
ALONG THE ALASKAN BEAUFORT SEA COAST

BY: Thomas L. Kozo

Vol. III

DISCUSSION BY:

M.A. Estoque

1. Since the land is always warmer than the sea in the summer, I would expect a shallow monsoonal flow (onshore) to prevail. This monsoonal flow should be perturbed duirnally by the sea breeze. Is this a correct expectation? If so, what are the relative magnitudes of the monsoonal and the sea breeze components?

2. The abstract of the paper states that the surface winds can be shifted as much as 180° from their free stream level (geostrophic wind) directions in both the summer and the winter seasons. This statement may not be entirely accurate during the winter. In his Fig. 8 Dr. Kozo provides an example of this large shift (about 180°) between the geostrophic wind and the surface wind. However, on the basis of the observations, an alternative pressure analysis can be done (see diagram below) which shows a much smaller shift.

An alternative isobaric analysis (to be compared with Dr. Kozo's Fig. 8) showing better agreement between surface wind and the geostrophic wind.
DISCUSSION BY:
Dr. Guenter Weller
Geophysical Institute
University of Alaska

This paper discussed the anomalies in the winds along the Alaskan coast of the Beaufort Sea, centered on Prudhoe Bay and the areas of offshore oil development. The paper is of considerable interest to such a development, since it demonstrates that the winds are far more complex and variable than indicated by the large-scale open synoptic network of observation stations maintained by the National Weather Service. The relevance of knowing the more complex wind patterns is in better trajectory analysis, particularly in the pathways of spilled oil. In a more general sense, the wind fields determine the motion of all coastal surface waters, and thus the currents that transport ice, biota, sediments and pollutants. The paper demonstrates that during the summer months sea breezes are appreciable. In winter, the Brooks Range produces a mountain barrier effect, with pronounced baroclinicity. This is responsible for wind direction changes of up to 180° between Pt. Barrow and Barter Island. An operational network of simple, preferably automated, weather stations is thus highly desirable for coastal areas where offshore oil development will occur.
I am honored that Dr. Weller had taken time from his sabbatical to Australia to respond in a discussion of my paper. The sentiments expressed are quite close to my own.

The effort expended by Dr. Estoque and the depth of the questions indicate more than a "mild" interest in these phenomena. Again, I am honored, but I cannot answer these questions properly. They have been the same ones I have asked myself.

In response to question 1, I have chosen the cases to present which had characteristics of sea breezes (clear days) only, such as the temporal clockwise surface wind vector rotation (Figure 6) and indirect evidence such as Figure 5. Figure 12 and Figure 4 are perhaps more indicative of the monsoonal nature of the summer arctic coastal winds with periodic reinforcement by sea breeze perturbations. It must be noted however, that winds due to monsoon effects will tend to become parallel (not blowing onshore) to the shore in the arctic due to the increased coriolis effect with increasing latitude and larger time scale (evidenced in Figure 4 also). There is a great need for more vertical sounding and surface data 50 to 100 km seaward of the coast. To pin down the magnitude of the monsoonal component a study of late evening wind data and that from overcast days should be undergone. The synoptic conditions must be favorable also.

Question 2 is one that can be perhaps partly answered by release of proprietary surface pressure buoy data from oil company studies to the east of Barter Island in the MacKenzie delta region. The only evidence (one simultaneous rawindsonde launching) that seems to refute the alternative pressure analysis of Dr. Estoque's is that of Figure 9 which shows that the upper level wind (Free stream) matches the analysis in Figure 8, and the calculated geostrophic wind from surface pressure networks (Kozo, 1980) at least for position H (Barter Island). In addition Kozo (1980)
shows a similar situation on March 12, 1979 where the surface pressure network calculation matches the upper level winds. To help unlock this mystery, a set of simultaneous sea level pressure observations together with a grid of infrared temperature soundings from satellites, and a reasonable distribution of vertical soundings (pilot balloon) could determine the three-dimensional distribution of the geostrophic wind.

"Influence of an ice layer on storm surge amplitudes"
by T. S. Murty, M. I. El-Sabh and J. M. Briand

Discussion
By:
N. K. Saxena, Department of Civil Engineering, University of Hawaii, Honolulu, U.S.A.

This is an interesting piece of work to explain the influence of an ice layer on long waves in general, and on storm surges in particular. There is a large amount of literature on the influence of ice on wind waves which fall into the classification of short waves. However, it is not clear whether the concept of dissipation of short period wind waves in the presence of ice would hold for the long waves.

Statistical analysis of the data showed that positive surges up to 3.0 metres in amplitude occurred only during ice free months. Maximum amplitudes of negative surges occurred during the period when ice was present. Another interesting result in the St. Lawrence estuary was that the duration of the positive surges was shorter on the north shore and the duration of the negative surges was shorter on the south shore.

The influence of the time-dependent meteorological forcing terms on the storm surges was also considered. The authors showed clearly that the rate of growth of the atmospheric forcing terms is important in determining the amplitudes and times of occurrence of the maximum surges. The amplitude of the surge is directly proportional to the rate of growth of the storm whereas the time of occurrence of the maximum surge is inversely proportional to the rate of growth of the storm. The time taken to achieve the maximum amplitude is different for different growth rates. This time difference becomes important when the tide-surge interaction is considered.

The authors are to be congratulated for a pioneering piece of work in elucidating the role of an ice layer on storm surges.

Discussion
by:
Judith Bobbitt, St. John's, Newfoundland

You mentioned that you also did this analysis for the Labrador coast. How did you obtain your tidal data, because there are no tidal stations along this coast.
Author's Reply

By:

T. S. Murty, Institute of Ocean Sciences, Department of Fisheries and Oceans, P. O. Box 6000, Sidney, B. C., V8L 4B2, Canada

We had tidal and storm surge data for the Station Nain, Labrador for the following years: 1965, 1966, 1967, 1969, 1972 and 1973. You can obtain these six years' data from the Marine Environmental Data Service of the Department of Fisheries and Oceans in Ottawa.
Discussion of "Internal Waves in Davis Strait and their Measurement with a Real-time System"
by D.O. Hodgins and H.G. Westergard

This is a very interesting and informative paper dealing with the practical detection and propagation of nonlinear waves just below the sea surface. Similar nonlinear internal solitary waves have been observed on the New York shelf, and in the Andaman and Sulu Seas. However, this paper appears to be the first report of their existence in arctic waters. Since the observations discussed here were made during summer, it is natural to ask whether these waves also occur in Davis Strait during winter, when the water is ice covered. And if they do, are their properties (amplitude, wave-induced currents, propagation speed, shape) very different from those found under summer conditions?

It is suggested by the authors that the mouth of the Hudson Strait is a likely source area for the waves, which are probably generated by the tides flowing over uneven topography. However, since the average propagation direction is 54°T (i.e., to the northeast), it is also conceivable that the waves may be generated by intense tidal currents flowing back and forth through Graves Strait (sounds treacherous!), between Resolution and Edgell Islands. Satellite images of the region including Resolution Island, the mouth of the Hudson Strait and site HEKJA would be of considerable help in locating the most common source region.

It is also of considerable practical and theoretical interest to know what happens to the waves beyond site HEKJA. If they travel in the sector 50-80°T, they will presumably reach the edge of the shelf, beyond which the water depth increases rapidly (see Figure 1). Do they continue to propagate here? Or are they severely attenuated due to radial spreading, turbulent viscous dissipation or radiation damping? On the other hand, if they propagate in the more northerly sector 30-50°T, are they affected by changing hydrography, uneven bathymetry or the local circulation (the Labrador current)?

Lawrence A. Mysak
University of British Columbia
If our hypothesis that the waves form as a result of tidal flows over shallow areas in straits along the coast is correct, then we would expect to see the internal waves in winter also. It is hard to judge how different their properties would be in winter for two reasons: 1) We have no information on the density structure under the ice, but although pack ice conditions are dominated by advection of ice from the north at certain times of the year, one expects local ice growth, at least during part of the winter, would tend to produce a more homogeneous upper water column. This would reduce the upper layer thickness and the density contrast, reducing thereby the phase speed of internal waves and perhaps also altering the wave modes present. 2) One expects a greater momentum exchange with an ice cover—proportional to the ice bottom roughness characteristics—than with the atmosphere in summer. This exchange could be effective in damping the wave motion, but to a degree which is not presently understood. For these reasons, then, we would anticipate that the waves would be formed in winter but they would propagate more slowly than in summer and, at distance from the coast, would be of smaller amplitude. It is certainly not clear if they could be distinguished from the background currents at HEKJA, 120 or 100 km from the likely source areas.

We agree that Graves Strait could be a source area: it is of a similar size to the source barrier in the Sulu Sea (pers. comm. J. Holbrook, NOAA). Satellite or remote sensing imagery would, indeed, be useful.

One of the major unknowns concerning the Davis Strait internal waves is the degree to which they spread out radially. Because our observations points are so close together (3 n.m.) we have no good measurements of wave attenuation, that could, for example, be explained by radial spreading which is proportional to $1/\sqrt{r}$, $r$ being the distance from the source (Johnson, 1980). About the best we can do is draw an analogy with the Sulu Sea waves which were observed to propagate as a plane wave packet for hundreds of kilometers away from the source area: a source which in scale is virtually a point source with respect to the distance travelled by the waves. The satellite images show the wave troughs to be straight and not curved.
The waves could, in principle, be refracted. From two-layer shallow water theory the propagation speed, $c$, is given by

$$c = \sqrt{\frac{g'h_1}{(1+h_1/h_2)}} \cdot \left[ 1 + \frac{\eta_0}{2h_1} \left( 1 - \frac{h_2}{h_1} \right) \right]$$

where $g' = g\Delta \rho/\rho$, $\eta_0$ is the wave amplitude and $h_1$, $h_2$ are the upper and lower layer depths. Over the shelf $h_1/h_2 \sim 0.1$ and $c$ can be approximated by

$$c \approx \sqrt{g'h_1} \left( 1 + \frac{\eta_0}{2h_1} \right)$$

Refraction, due to the slow change in $c$ along the wave trough, would be caused mainly by changes in $h_1$ in areas where $\eta_0$ is not changing rapidly and $\Delta \rho/\rho$ is constant. However $h_1$ appears in two places in this equation and changes in $h_1$ tend to be self-cancelling. For example, for $\eta_0 = 43$ m and $g' = 1.34 \times 10^{-2}$ m/s$^2$, $c = 1.13$ m/s for $h_1 = 40$ m and $c = 1.08$ m/s for $h_1 = 20$ m, values for $h_1$ that bracket all our observations. Thus refraction is not likely to be a major influence on the wave propagation.

For waves travelling at about 75°T, then, the largest of which should reach the shelf break, we expect radiation damping due to the rapid increase in depth to be the most important attenuation mechanism (Maslowe and Redekopp (1980). Pereira and Redekopp (1980) give some theoretical insight into the problem. In terms of their nondimensional time, $\Delta t$, we find that for $\Delta t = 0.5$ the attenuation of wave amplitude would be about 50% to 60%. This corresponds to about 40 wave periods or 8 hours. In 8 hours the wave would propagate approximately 25 km to 30 km. So that the attenuation is quite rapid within a short distance out from the shelf break. Presumably turbulent frictional dissipation (Djordjevic, 1980) would also reduce the wave amplitudes all along their travel path but this appears to be a minor factor.

Large internal waves crossing HEKJA at, say, 30°T should continue northward and in doing so would encounter a significant submarine ridge (not shown in Fig. 1) at 63°10'N, 61°30'W. Here the water depth changes from ~350 m to ~200 m to ~400 m over the ridge in the direction of travel. It is possible in this circumstance to have the initial large soliton break up into a number of smaller waves by a process of soliton fission (Djordjevic and Redekopp, 1978).
In fact using their formula (4.15) with \( h_1 = 40 \text{ m}, h_2 = 200 \text{ m}, h_{2\infty} = 300 \text{ m} \) and \( \eta_0 = 40 \text{ m} \) we might expect one wave to produce four waves behind the ridge. However because the water then deepens again, these smaller waves should dissipate rapidly by radiation damping. Thus internal waves surviving to the north-northeast of this ridge are not expected to be large.

Of course, these are hypotheses based exclusively on theoretical results. The influence of the mean Baffin Current shear may be quite important over large distances, since we do know that spatially it is highly variable (LeBlond et al., 1981), although detailed mappings of its properties are not presently available.

REFERENCES


Some expansion of the model mentioned by Hodgins and Westergard assists in forming and understanding comparisons between data and theory.

For example:

\[
U_1 = \frac{c_0 \eta_0}{h_1} \sech^2 \left( \frac{(x-ct)}{h} \right) \\
\eta(x,t) = -\eta_0 \sech^2 \left( \frac{(x-ct)}{L} \right) \quad (1)
\]

\[
U_2 = -\frac{c_0 \eta_0}{h_2} \sech^2 \left( \frac{(x-ct)}{h} \right)
\]

which give opposite velocities in the upper (1) and lower (2) layers.

Note also that the velocity is constant with depth contrary to the data presented.

If \( c_o^2 = g \frac{\Delta \rho}{\rho} \frac{h_1 h_2}{h_1 + h_2} \), \( \alpha = \frac{-3c_o}{2} \frac{h_2 - h_1}{h_1 h_2} \),

\[
\gamma = c_o h_1 h_2 / 6 \quad \text{and} \quad c = c_o \left( 1 - \frac{\alpha}{3c_o} \right)
\]

the scale length \( L = (-12 \gamma / \eta_o a)^{1/2} \).

At the Hekja 0-71 site \( h_1 + h_2 = 350 \text{m} \) where \( h_1 \sim 35 \text{m} \) and \( h_1 / h_2 \sim 1/10 \) and \( \eta_0 \sim 35 \text{m} \).

If \( c_o \sim 1 \text{m/s}, \alpha \sim -\frac{3}{2} \cdot \frac{1 \text{m/s}}{35 \text{m}}, \eta_0 a \sim -\frac{3}{2} \text{m/s}, \eta_0 a/3c_o \sim -\frac{1}{2} \)

\( c \sim 3/2 \text{ m/s} \) and \( L \sim 130 \text{m} \).

This value for the scale length \( L \) is less than half that given by the authors. Why?

Although there may be solitary wave events in the data, interpreting most of what they see as solitons is questionable in view of the poor comparison between the theory and the data.

It also should be mentioned that the waves could not survive the 130km (15-20 hour) trip from Hudson Strait and it would be more reasonable to consider closer sources and invoke refraction principles.
CLOSURE TO JOHN LAZIER

The form of the two-layer model presented here is that which we have used (and also Osborne and Burch, (1980). Although intended as an order of magnitude analysis, the numbers chose by Lazier give too large a value for the phase speed. Using actual field values for rip 216-2b gives:

\[ \eta_0 = -43 \text{ m} \quad \frac{\Delta \rho}{\rho} = 1.34 \times 10^{-3} \quad h_1 = 40 \text{ m} \]
\[ \alpha = -0.222 \text{ s}^{-1} \quad \gamma = 1405 \]
\[ c = 0.99 \text{ m/s} \quad L = 134 \text{ m} \]
The observed values were 0.90 m/s and 0.84 m/s between the three moorings, although we reported only the latter figure in the paper.

To be consistent with Osborne and Burch (1980) we have used \( 2L = \ell \) as the "scale length" and not \( L \) itself, since this is what is required in comparing the internal "wave length", i.e. \( \ell \), with the total water depth \( h \). Our \( \lambda \) is about 4L since this is what is most easily measured directly from the thermistor chain data. These are not precise measures and were intended as a first fitting between model and observations.

We feel that a soliton interpretation is reasonable both on the basis of the wave properties and the correspondence, if not absolute agreement, between model and observations. A resume of properties includes the distinctive surface choppy water bands propagating with the waves, the large non-linear waves of depression on the main pycnocline, and on occasion, rank-ordered wave trains. The discrepancy between model and observations reported here can arise from a number of sources: current measurement error, the difficulty of extracting the internal wave properties from the substantial background currents in the area, the natural variability in water properties due to the Baffin Current and the difficulty of accurately measuring \( \eta_0 \) and \( h_1 \). The model is sensitive to these parameters. We are presently examining more rip events in terms of a two-layer Korteweg-de Vries model to better define the agreement between theory and observations, and hope to report these results in the next few months.

We are not convinced that the waves can not survive the journey out to HEKJA from the coast. Following the 350 m contour
passing through HEKJA, which is largely parallel to the 500 m contour in Fig. 1, indicates a fairly constant depth between the site and Resolution Island. Therefore, again drawing the analogy with the Sulu Sea waves and assuming they are largely planar in Davis Strait, we would have to explain their attenuation by turbulent frictional dissipation mechanisms. In view of the longevity of these waves elsewhere it would appear difficult to account for their complete disappearance by HEKJA by this means. Refraction does not appear to be too important (see above Closure), however hydrographic data are not available along the travel path to make more precise refraction calculations. We would, of course, welcome suggestions for alternative source mechanisms!
"A THREE-DIMENSIONAL MODEL OF NORTON SOUND UNDER ICE COVER" BY S. K. LIU AND J. J. LEENDERTSE
Paper B3-4

DISCUSSIONS
By:
T. A. McClimans, Norwegian Hydrodynamic Lab
Trondheim, Norway

(1) Do your solutions show any tendency to form horizontal surface fronts and mesoscale instabilities as observed in most coastal regions?

and By:
D. O. Hodgins, Seaconsult Marine Research Ltd.
405 - 1200 W 73rd Avenue
Vancouver, B.C. Canada

(2) You commented that at certain times of the season the water column becomes highly stratified and that to resolve the pycnocline you altered the layer spacings to give more planes at the pycnocline level: this is the classic resolution problem in highly stratified flows. Why did you not use a two-layer model with a dynamic free surface and dynamic interface to calculate the near-surface currents?

(3) In the examples you showed there was no discussion of boundary conditions. How did you specify the water level and volume (mass) fluxes on the open sea boundaries?

(4) How did you initialize the velocity, salinity and temperature fields in Norton Sound and how long did you run in the model to establish a dynamically balanced field? What time step did you use for the surface wave mode and was a different time step used for the salt and heat balance equations?
S. K. Liu and J. J. Leendertse, The Rand Corporation 1700 Main Street Santa Monica, California 90406 USA

The authors would like to thank Drs. Mc Climans and Hodgins for their discussion on the paper. Answers to the four questions are as follows:

(1) Under ice-free conditions, particularly during earlier summer frontal structures do form in the system as well as in the simulation. The large volume of Yukon River runoff and local ice melt that create the sharp pycno-structures interact with the tidal residual circulation forming a counter-clockwise net circulation pattern within Norton Sound. Under the predominant summer winds from the SW, a frontal structure is usually formed near the head of the Sound where pronounced downwelling exist during the entire ice-free season. In fact, even during the beginning of ice melting process, homogeneous water of higher salinity is located in the northeastern part of the Sound. The reason for the higher salinity in this area can be traced back to the local ice formation and melting processes. When the ice was formed during the previous winter, salts rejected during the freezing process were added to the local water, thus creating certain transient vertical density instabilities and vertical mixing over the local water column. Under the prevailing northerly wind, locally formed new ice was transported toward the western part of the Sound. This is evident from satellite photos in which ice-free areas are always present in the eastern Sound. During the subsequent ice melting period, homogeneous water with high salinity are found in this particular area. Warmer winds from the SW during the ice melting process creates downwelling which creates homogeneous water in the same general area. Similar processes exist throughout the entire eastern Bering Shelf. [Ref. 1,2] However, in the southern Bering shelf, the frontal areas are located in waters between 40-50 meter depths. In the stratified area, the model, in fact, generates temporary instabilities in the vertical (salt fingers) that have been observed frequently in the southern Bering Sea.
(2) From the answer given in (1), we have to simulate vertical stratification as well as frontal structure in the shallow areas induced by the up/downwelling process which is ecologically important. Consequently, a layered model was not used.

(3) The open boundary conditions were specified as the variations of water level computed by tidal predictions (16 components) using coefficients derived from field data.

(4) The initial temperature and salinity conditions for the simulation were derived from field data collected at 24 locations. Contour lines were drawn by scientists from NOAA. Integration time step used for the simulation was 3 minutes. It was used for both surface wave and density computation to resolve the fine features of the vertical instabilities induced by density computation through the equation of state. The initial run-in period was approximately 5 days followed by various restarts for various dynamic tests.

References


Vertical distribution of temperature and currents through a cross-section at 0748 hr 18 June 1976. The plotting scale for the vertical velocity component has been enlarged 727 times.

Vertical distribution of salinity and currents through a cross-section at 0748 hr 18 June 1976. The plotting scale for the vertical velocity component has been enlarged 727 times.
I would like to present the following questions about the presented ice structure interaction model:

(1) What (and why) are you using for the added mass in the mass matrix $[m]$?

(2) How do you determine the lengths of the indentation and failed zones $i_j$ and $f_j$?

(3) Please give some description of the finite element model used in the analysis and outline its capabilities and limitations.
The authors would like to thank Mr. Ghoneim for his discussion. The added mass concept is used in the dynamic analysis to include hydrodynamic effects in the response of the structure. In this case the added mass in the mass matrix is simply the mass of the replaced water concentrated in the node points. The approximation is not accurate, but because of its small effect on the results it is felt to be acceptable.

The estimation of the lengths of the indentation and failed zones was based on laboratory indentation tests pursued with real sea and lake ice plates. The ice force was recorded as a function of indentor penetration and the lengths of indentation and failed zones corresponding to each force peak were estimated. The lengths were found to be proportional to the force peak. Due to the effects of plasticity and local crushing the indentation zones were found to be considerably longer than the estimates based on theory of elasticity. The scale effects were estimated simply by setting the constants A and B proportional to the ice thickness h.

It must be realized that the estimates for indentation and failed zone lengths are based on a limited set of small scale laboratory experiments. Proper rules governing the lengths of indentation and failed zones can only be obtained from field measurements with variable aspect ratios D/h and structural shapes in different conditions. However, the proposed method is not very sensitive to the values of the lengths of indentation and failed zones if the structure
is not very rigid.

Simple beam elements with lumped masses and an ice force affecting only at one node were used in the presented analyses. Also other kinds of elements and loading configurations can be used although this may in some cases mean replacement of the convinient mode shape analyses by another method.

In this paper the interaction between a slender structure and a moving uniform ice field or floe was analysed. The analysis can be extended to wider structures by using the zone analysis (the failure process of ice is at different phases at different zones along the perimeter of the structure). Zone approach also provides a convinient tool to analyse the transverse and torsional vibrations of for example a bridge pier with triangular nose subjected to dynamic ice forces. Finally the interaction between the structure and a first year pressure ridge can be analyzed for example by analyzing the effects of the frozen upper part using the proposed method and by including the unconsolidated lower parts in the analysis as a static envelope force function and added damping.
DISCUSSION BY PROF. G. MOE ON
"RESPONSE OF OFFSHORE TOWERS TO
NONSTATIONARY ICE FORCES"

Authors' Reply by: D.V. Reddy, P.S. Cheema, and
M. Arockiasamy
Faculty of Engineering and Applied Science
Memorial University of Newfoundland
St. John's, Newfoundland

The authors would like to thank Prof. G. Moe for his interesting
comments on our paper. In this paper, damping matrix \([C]\) is related to
the mass \([M]\) and stiffness \([K]\) matrices by

\[
[C] = \alpha [M] + \beta [K]
\]

where \(\alpha\) and \(\beta\) are constant damping coefficients. In reality, the modal
decoupling is permissible without any loss of accuracy when damping is
small in comparison with critical damping.

Variable damping for different modes can be prescribed for the
\(m\)-th mode by

\[
\lambda_m = \frac{\alpha}{2f_m} + \frac{\beta f_m}{2}
\]

where

\[
f_m = m\text{-th mode undamped natural frequency in Hz,}
\]

\[
\alpha = \lambda^* f^*,
\]

\[
\beta = \lambda^*/f^*,
\]

and \(f^*\) = the frequency at which the modal damping ratio \(\lambda^*\) is
minimum.
AUTHOR'S REPLY
By:

M. Määttänen, University of Oulu, Finland

I would like to thank Dr. Reddy for his interesting proposal for vibration absorber. Usually it is difficult to get heavy internal damping in offshore structures. However in the case of a vibration isolated lighthouse it is easy to increase damping with conventional shock absorbers which can be isolated in the joint between the foundation and the superstructure.

The proposed liquid sloshing for damping purposes is also a good approach for the vibration isolated lighthouse. Even though the liquid sloshing tanks occupy more volume in the superstructure they simultaneously replace the concrete frequency tuning weights. Liquid sloshing concept gives also possibilities for damping in other fixed aids-to-navigation where shock absorbers would not work. It would be worth testing the liquid sloshing concept on a model structure.
Discussion

By:

Dr. D. V. Reddy

Memorial University
New Foundland

Canada

For example, you have a 5 cm cantilever pile moving through a 5 cm thick ice field. If the material is mainly epoxy resin, the E value of the composite epoxy-steel core specimen would be low. This would make the effect of the pile flexibility very important, eg, inertia effect.
Author's Reply

By:
H. Nakajima
N. Koma
M. Inoue

Nippon Kokan
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Japan

The author would like to thank Dr. Reddy for his discussion of the paper. As he pointed out in his discussion, we also used a five centimeter diameter model in our tests. This model was made from a steel core of four centimeter diameter with 0.5 centimeter thick epoxy resin coating. So we think that the flexibility of this model is low enough. Thus we do not consider that the flexibility of the model affects the test results.
Discussion
By:

R. Frederking  
National Research Council  
Canada of Canada

This paper provides new data on ice forces acting on a cylindrical pile measured both at small scale in an ice model basin and larger scale in nature. It is very useful to have such field and model data available for direct comparison and the authors are to be commended for making it available.

The importance of the compressive strength in normalizing ice loads on the pile is pointed out in the paper. In Figure 1 a histogram of measured compressive strengths is shown. Could the authors indicate the loading time for these tests. Were any differences noted in failure mode or failure time between each of the individual compression tests?

The laboratory pile tests were conducted with reduced strength ice. What similitude laws were followed and what scale factor was used for the tests? Two failure modes, crushing and buckling, were observed in the model tests. Have the authors examined their buckling failure results in terms of Sodhi and Hamza's [1] buckling analysis?

As was pointed out in Figure 4 ice pressure is function of towing speed. In comparing the laboratory and field results (Fig. 6) what velocities were used?

DISCUSSION BY Dr. R. Frederking ON
"THE ICE FORCE ACTING ON A CYLINDRICAL PILE"

Author's Reply
By:
H. Nakajima 
N. Koma 
M. Inoue

Nippon Kokan 
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Japan 
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The author would like to thank Dr. Frederking for his discussion of the paper. The stress rate in compressive strength shown in figure 1 is 1.8 ~ 3.6 kg/cm² sec. Failure mode in compressive strength is the separation of the crystals in almost all cases as Dr. Abdelnour pointed out in his discussion of our paper.

The reason for using the reduced strength of ice is that the towing carriage and the load cell have only small load capacity, therefore we needed the reduced strength of ice.

We used the wet seeding method in order to decrease sufficiently the grain size of model ice in comparison with the model size. At this time we have no exact idea what similitude laws will be followed and what scale factor should be used in the towing tests for crushing mode failure.

We haven't examined our results with Sodhi and Hamza's buckling analysis. In their analysis, they analysed symmetrical buckling to the towing direction. In our results, however, buckling of ice sheet occurred asymmetrically or locally to the towing direction. Therefore we considered that it is not easy to compare our results with their analysis. But we intend to try to examine the results with the analysis in future.

The data shown in figure 4 was gotten only from the towing test in ice tank, and data from field tests is not included in the figure. Field tests were the indentation test and we could not measure the indentation velocity because of the trouble with the instruments.
"THE ICE FORCE ACTING ON A VERTICAL CYLINDRICAL PILE"

BY H. NAKAJIMA, N. KOMA AND M. INOUE

Discussion
By:

Razek Abdelnour
Arctec Canada Limited
Canada

Ice pressure on vertical piles obtained from model test experiments using Horizontal C-Axis ice (saline or urea dopped ice) should be very carefully interpreted. The authors presented in figure 6 the normalized effective pressure versus the pile diameter and based upon, they concluded that the pressure measured was higher than obtained by other investigator.

The ice pressure was normalized using the unconfined (uniaxial) crushing strength of the ice. However, based on our experience at Arctec model ice basins the technique to measure the uniaxial crushing strength of the model ice is not yet well developed and we believe that the value obtained using this method is usually low. That is due to the mode of failure of the sample where the vertical fine crystals tend to separate under compression rather than crush as observed during the experiments which is believed to be the effect of confinement.
The author would like to thank Dr. Abdelnour for his discussion of the paper. As he pointed out in his discussion, we think that failure mode between towing tests and uniaxial compressive tests is different, and it is necessary to carry out the confined compressive test.

We think, however, that it is hard to carry out the confined compressive test for such a thin model ice because it is not easy to make up the proper size of specimen. A good machine for tests and standard testing procedure are not yet developed. Therefore, we tried to use uniaxial compressive strength based on Timco's method to normalize the ice pressure.

If the ratio of ice pressure to compressive strength, called indentation factor, is equal to three as proposed by Dr. Michel et al, we think that we estimated high value as a compressive strength, as opposed to Dr. Abdelnour's estimate of low value.
The authors have produced some very interesting information on the behaviour of beams. I wish to raise a point with respect to their analysis of the deflection measurements. There is a tendency to explain such measurements using the so-called flow law based on uniaxial compressions tests. It is normal to obtain the exponent, \( n \), for the stress in this law by determining the minimum creep rate and plotting this against the stress. The minimum creep rate usually occurs in the range of strain of 1 to 2\% and could look at this form of the relation, therefore, as the dependence of strain rate on stress for a given strain.

For the beam and plate problem the situation is quite different. Here, we wish to know the dependence of strain rate on stress at a given time. In this case, each element has undergone a strain that depends on the deflection and the distance from the neutral plane. If the deflection is sufficiently large, the elements furthest from the neutral plane may have been strained into the "steady state" region. Elements closer to the neutral plane, however, have still not been strained out of the transient range. The value of \( n \) that is obtained for conditions of given time is different from that which is usually for given strains. I wish to ask the authors to comment on this and, in particular, if they have taken this factor into consideration.

Can you predict the plate or beam response for a given load, time and temperature? If not, then all you have developed is a curve fitting method!
For information, the basal-plane dislocation density in previously undeformed S2 ice has been found by this discusser to be in the range of a few million per cm\(^2\) (*) and not just a few thousand per cm\(^2\).


AUTHORS' REPLY:

Our paper bring some light to the problem brought up by Gold of the difference between uniaxial tests and bending tests whereas not all the ice at various levels in a beam is subjected to the same creep law. The fact we found that a linear vertical section of a beam remains linear as it deforms show that for a constant load test, once a steady rate of deflection is attained, there must necessarily be a constant rate of deformation for all fibers in the beam at all levels. Because the resisting moment stays a constant, the only way that the stress could increase in the central, low rate of deformation part of the beam would be a decrease in stress in the extreme fibers.

This is quite compatible with the known uniaxial behaviour of polycrystalline ice at constant strain rate where there is a stress reduction after yield to a new constant value.

After a long enough given time the main part of the section is under true secondary creep and the central part, still in primary creep, contributes very little to the resisting moment. The stress distribution then corresponds to that shown in Figure 6 where the Glen's laws does apply at almost all levels in the ice beam with the same value of \(n\). This is why, the computed value of \(n\) by taking a reference strain rate on the extreme beam fibers should give results comparable to those obtained with uniaxial samples. In our case we found an \(n\) value of 3,5 which is very similar to that obtained with columnar ice at high stresses.

In answer to Sinha's question it is obvious that the objective of the rheological model is to obtain the values of the \(\alpha, \beta, n_0\) and \(m\) coefficients for each type and history of an ice piece in order to predict eventually its creep behavior under any condition of loading. Because each of these coefficients have a physical meaning, this knowledge will help very much in understanding in depth the actual deformation processes in the ice.
Sinha states that he has observed by an etching technique, dislocations density of a few millions per cm$^2$. We obtained with our model density of mobile dislocations of the order of $10^3$ to $10^4$ per cm$^2$. The obvious explanation is that we are computing only the mobile dislocations that account for creep and not all the dislocations where a large number are fixed and pinned down by impurities, singularities, boundaries, etc. It must also be remembered that the computation of mobile dislocations is obtained by using a reference velocity of movement of dislocation of $v_0 = 10^{-4}$ cm s$^{-1}$ for a shear stress of 100 kPa at 0$^\circ$C. As this is related directly to the number of mobile dislocations by Orowan's formula $\tau = b n_0 v_0$, where $b$ is the Burgers' vector, it is obvious that a lower value of $v_0$ would indicate a larger number of mobile dislocations. It would be very important if the etching technique could be developed to measure the actual number of mobile dislocations and their velocity.
DISCUSSION BY:
Andrew Assur, Chief Scientist, U.S.A.

Theory and practice for the bearing strength of ice

Johnson presents a worthwhile attempt to develop practical equations for ice crossings with various vehicles based upon exact theory.

Nevel (ref. 17) went as far as classical plate theory can go. This is what I meant by Johnson's ref. 1. The only further desirable improvement could be the treatment of elliptical loading surface. The method outlined by Assur (1961) for arbitrary loading surfaces gives virtually the same results as ref. 17, but Nevel added the convenience of breaking rectangular loads and the convenience of a pocket calculator program.

That does not mean that the approximate presentation of theoretical results proposed by Johnson cannot be improved. In addition the ultimate bearing capacity leading to collapse of the ice sheet must be considered.

The general exact equation is simply:

\[ P = \pi \sigma \alpha \frac{h^2}{6} \]  

(1)

with \( P \) - the load, \( \sigma \) - flexural strength, \( \alpha = 1 \) for the elastic case, \( h \) - ice thickness \( \pi = f (B) \) - a complicated function based upon theory, \( B = \frac{b_1}{\ell} \) - relative load distribution in terms of \( \ell \), a characteristic length which is a function of ice thickness, Poisson's ratio and Young's modulus. \( \pi \) can be calculated exactly from theory.

Eq. (1) yields \( \sigma = \frac{6}{\pi} \frac{P}{\sigma} \frac{h^2}{6} \) which can be compared to Johnson's

\[ G = \frac{c_6^{5/3} h^{1/3} c_B^{5/3}}{h^{5/3} \sigma h^2} \]  

(2)
with the approximation $\frac{1}{\pi} \alpha h^{1/3}$.

This is, of course, the relationship for a given load, while the others equations, deal with classes of vehicles, with increasing loading surfaces for heavier loads.

To present $\pi$ as a function of $h$ is a disadvantage, since it is tied to a definite Young's modulus and Poisson's ratio. It would be more efficient to approximate it as a function of the characteristic length $l$. This makes it applicable to ice sheets with varying properties, including sea ice. A "thick plate" effect causes some minor complications.

Johnson's approximation (1) holds for $h = f(\sigma)$ for a given load distribution or a given vehicle. However, he introduces the unnecessary inconvenience of having to calculate another $C_6$ for different $P$ (an empty or a loaded brick).

One must also consider the effect of ice properties varying with depth which affects $\sigma$ and $l$, as outlined in several of my publications.

Finally a word about the ultimate collapse of an ice sheet. This is a catastrophe for the vehicle. One does not worry about cracking the ice sheet (first crack) which is being predicted by classical theory.

Assur (1956) used:

$$P_a = \frac{1}{S} [P_1 + \zeta (P_2 - P_1)]$$

in his criteria for aircraft landing on ice

$P_a$ - allowable load

$P_1$ - first crack load, predicted from classical plate theory, considering the load distribution

$P_2$ - load leading to collapse of ice sheet (from theory and experiments)

$\zeta$ - risk factor for standard or emergency operation

$S$ - safety factor.

Nevel (ref. 17) or Johnson do not consider the collapse load. It is here that further advances can be made for practical criteria.


DISCUSSION BY A. ASSUR ON

THE REACTION OF A FLOATING ICE SHEET TO SIMPLE LOADS AND CERTAIN CLASSES OF VEHICLES AND MACHINES.

POAC Vol. 1, page 571

AUTHOR'S REPLY

By
Phil Johnson

Dr. Assur’s comments and discussion as well as his clarification of a number of points are greatly appreciated and will be of great value to anyone using Thin Plate theory in solving Bearing Capacity problems.

A point I failed to make in the paper is that the solutions and other relationships are for STATIC loads. A moving load introduces complications I did not wish to address.

My equation (1) is for a vehicle with a given load and load distribution. However, eq. (4 and 3) yield Bearing Constant values in terms of the gross weight as well as the Gross Loaded Area and a constant. If the weight of a tracked machine or a car or light truck is found and then later changed, it is simple to insert the new weight into the proper equation and find a new solution. Alternatively, either equation could be differentiated and an adjustment to Bearing Constant could be calculated for a change in weight.

I have looked at the effect of changing the weight of the load on the ice reaction of other vehicles. Ref. [9] and this paper discuss this problem with cargo weight change of the M35A2. The problem is complicated in that the "critical wheel" changes as the cargo weight is increased.

In the case of the Lockheed Hercules (C-130) aircraft [22], the Bearing Constant for the machine proved to be linear with the weight of the aircraft and cargo on the two main gear when a reasonable value was used for weight on the nose gear.

A case that cannot be solved without additional information is that of a tractor-trailer rig with a heavy but compact load. This load could be placed in several locations that cannot be predicted so the axle and wheel loads could not be calculated. Furthermore, the rig geometry is not fixed from the tractor and trailer in line to the possibility that they might be at 90° or more with each other in the case of jackknifing or turning around.

It would be convenient if the allowable load and collapse load could be expressed in terms of maximum tensile stress rather than gross weight.

The author of the paper has developed an approximate equation to the known solutions for the bearing capacity problem. His observation that the same approximate equation can be used for similar vehicles is significant. It is interesting to note that previously, some authors have proposed that the stress is inversely proportional to the ice thickness squared. This paper shows that for many real problems, the exponent should be 5/3 rather than 2.0. These approximate equations will certainly make bearing capacity computations much easier for field use.

Author's Response by Phil Johnson

I appreciate these comments by Dr. Nevel. He and I are using the same thin plate solutions but a different philosophy in their use. He uses his great skill in computer programming to develop programs on newer hand-held calculators [17] to make the solutions in the field. I favor making the basic calculations in the office and developing Bearing Constant values and relationships that can be taken into the field and used there.

As I mentioned in the paper, I have and am continuing to use a Thin Plate computer program [20] originally written by Dr. Nevel and he first instructed me in its use. I am greatly indebted to him for these matters and the many illuminating discussions we have had.
Discussion by R. Frederking

of

THE REACTION OF A FLOATING ICE SHEET TO SIMPLE LOADS AND CERTAIN CLASSES OF VEHICLES AND MACHINES. POAC 87 Paper, pa. 571

by

Phil Johnson

In any estimate of ice thickness required to safely support a given load, there is some element of risk. This is most often due to uncertainties in ice strength or unanticipated variations in thickness. Assuming, however, that ice conditions are equivalent, are you equally confident in making predictions for tracked vehicles versus wheeled vehicles? Also, would you say that the failure behavior in the above two cases would be the same?

Author's Response by Phil Johnson.

The first step in evaluating the ice thickness required by a load, such as a vehicle, is to find the ice thickness required if the ice sheet is "perfect". If the ice sheet has flaws or its thickness cannot be measured accurately, an increased thickness may be required. Thin Plate solutions are the best means of finding the thickness of a "perfect" ice sheet that is required and engineering judgment must be used to adjust the thickness for flaws in the ice and variations in thickness.

The questions regarding ice response to tracked and wheeled vehicles does not indicate whether the vehicles are moving or static. I will comment on the static case since much more is known about ice reaction to a load if the load is not moving. I suspect that at least part of the following comments would also apply to a moving load.

As I mention in the paper, I express the reaction of an ice sheet to a particular vehicle, machine, or other load in terms of Bearing Constant values. If two vehicles, one tracked and the other wheeled, have the same Bearing Constant values and are placed on the same "perfect" ice sheet, they will generate the same maximum tensile stresses in the ice. If they slightly exceed the tensile strength of the ice, the "first crack" will develop under both vehicles and there is no difference in ice reaction to this point. However, the ice will not collapse under either vehicle at this "first crack" load condition. Please examine Dr. Assur's comments on collapse loads in his discussion on this paper.
If the ice is thinned or weakened under the two vehicles, or if they are parked on the ice for a time, the ice will collapse. As Assur points out in his Discussion, Nevel [17] and I both fail to address the matter of collapse loading. It is easier to state that there is a factor of safety of around 2 when the first crack forms and go on to other matters. Regarding the comparative behavior of tracked and wheeled vehicles at the collapse point, I merely state that if the ice is thin enough, it will collapse under either type of vehicle. I have no reason for believing that there would be much difference in the ice response to the two types of vehicles up to this point.

At the time of ice collapse, the type of ice behavior would vary greatly and, to illustrate this, I will consider two typical vehicles, an 18-wheel tractor-trailer rig and a bulldozer tractor. There are other types of wheeled and tracked rigs so the comments here may or may not apply to them.

Substantial tensile stresses are set up in the ice under the wheels of a loaded tractor-trailer rig and, with a normal load distribution, the stresses are greatest under the driver wheels of the tractor. If the ice will collapse under the truck, it will do so under the driver wheels and it will probably not collapse under the front and trailer wheels for the time being. Once the ice fails under the driver wheels, they will settle into the water but the truck "superstructure", the chassis and frame as well as the fenders, bumpers and truck bed will hang up on the ice and support the truck.

A bulldozer, on the other hand, is a heavy compact load riding on tracks that can punch out a hole in the ice that the entire machine can fall through with the exception of the mounted bulldozer blade. The maximum tensile stress will occur under the centers of the two tracks and, if the ice begins to collapse there, the collapse will probably continue until most or all of the ice under the tracks is gone. This can happen quite rapidly and the machine will immediately begin to fall through the ice. It may hang up momentarily as the blade of the bulldozer catches on the ice but, between breaking the ice and sliding on it, the blade can be expected to do little to support the machine which can be expected to sink rapidly.

It can be seen that the ice behavior at the time of collapse can vary greatly. Extreme examples of wheeled and tracked machines and vehicles were used in this discussion and do not illustrate a general rule regarding tracked and wheeled vehicles. We could look at a wheeled bulldozer or front end loader and a large transporter riding on four widely-separated tracks to reach different conclusions.
Considering the tractor-trailer rig, the chance of ice collapse under the driver wheels (and thus under the entire truck) would be reduced if the load were balanced so that the tensile stresses under the various wheels (not the weight on the wheels) were approximately equal. The chance of ice collapse under the bulldozer tractor would be reduced if the blade rested on the ice rather than being carried in the air so that blade weight was not carried by the tracks. Applying down pressure on the blade would be even better as this would relieve the tracks of additional weight.
Authors of discussed paper: E. Leavitt, J. Sykes and T.T. Wong

Number of papers of session: B4.1

Discusser name and address: Erik Banke, Martec Ltd.
5670 Spring Garden Road
Halifax, N.S.

Discussion:

1. What is the status of the model?

Coding has been completed and some testing of the model has been carried out. Further validation of the model will be necessary, however it is not envisioned that significant changes will be necessary. What is required now is to develop the full forecast system of which the actual ice model codes will be one component.

2. Who are the potential users?

The initial specified user was to be a winter drilling operation in the Beaufort Sea.

The model was designed with the potential to aid in route selection for tankers or other shipping. There are of course differences in forecast requirements or rather in the emphasis which is placed on different forecast elements. However the model code will be capable of being utilized in forecasts for exploration, production and transportation.

3. How applicable is the model to offshore Labrador, vis-à-vis LIDEX?

With reference to question 2, it was realized that ice forecast requirements would extend to other areas of the Arctic besides the Beaufort Sea. The Fine Scale Model should be adaptable to other areas of the Arctic where ice conditions are similar to the Beaufort. That is the ice sheet is broken up into a matrix of thick floes and areas of thin ice or open water. There would be questions about the description of the ice edge. For example, how to model the wave ice interaction and the resulting redistribution of ice. It is also likely that ocean currents will play a larger role in ice motion in the Labrador Sea than they typically do in the winter in the Beaufort Sea. However, the basic model should be transferable.
4. How significant is sea surface tilt with respect to the overall model?

No data was available during the time period of the experiment to evaluate the sea surface tilt. However, estimates of typical magnitudes suggest that this term can normally be neglected in winter ice conditions for the Beaufort Sea.
QUESTIONs

By:

Donald O. Hodgins, Seaconsult Marine Research Ltd., Canada

QUESTION:

Your categories for ice seem predicated on building a numerical sea ice model. Are you satisfied that they are sufficiently physically based to be useful for operational ice descriptions also? Are they unique to the Beaufort Sea or can they be applied to other geographical areas?

AUTHOR'S REPLY:

The model that we have presented provides a new framework that allows us to describe ice conditions in terms of only four categories. This, of course, is a simplification from the full thickness distribution which has an infinite set of categories. The starting point for developing the framework for this model was to develop one that was capable of describing the essential physics needed to determine ice strength accurately, while at the same time having features that were observable using remote imagery. Therefore, yes, we do believe this model is sufficiently physically based to be useful for operational ice descriptions. Although we developed the choice of categories based on first-year ice in the Canadian Beaufort Sea, I believe the framework can certainly be applied anywhere. However, one should be careful to look at the particular problem, the spatial resolution, and the time scale before applying any model indiscriminately. For some problems the model might not be applicable or different values of categories should be used.

QUESTION:

You have described "isotropic" openings and closing: I assume your categorization and modeling are therefore isotropic. Is ice behavior really isotropic? How important is anisotropy and how greatly does it limit the generality of your ice categorization?

AUTHOR'S REPLY:

I must clarify the meaning of the word "isotropic" when it is used to describe stretching tensors. Isotropic in this sense means that no shearing occurs but only the area changes. The word in this sense has nothing to do with the isotropy of the ice character. With regard to the latter point, it is extremely difficult to know how important anisotropy is in describing ice behavior. It seems that anisotropy becomes more important as the scale of the problem gets smaller. At smaller scales, we have less richness in the distribution of floes and fewer ridges and leads, which therefore tend to be less uniformly distributed in their orientations. It would be very useful to formulate an anisotropic model to see how much better it does at predicting some selected variable. Along this line, I would like to point out that the importance of anisotropy could well differ for different applications.
QUESTION:

You have discussed the categorization in terms of ice growth. Does it apply and work for sea ice breakup and dispersal?

AUTHOR'S REPLY:

The model accounts for both ice growth and decay. It does so in terms of changing the thickness of the boundaries of each category of ice according to the thermal growth or decay rate at that thickness. So within this framework it does apply to breakup and dispersal of sea ice. However, one should be extremely careful in defining a constitutive law for redistribution and also for evaluating the strength of the ice cover as it breaks up. Our ice strength is determined by identifying energy sinks into which the large-scale stress field dissipates energy through deformation. I would expect substantial differences in the sinks during breakup, when perhaps the crushing of rotted ice becomes the dominant sink of energy and very little energy goes into either gravitational changes or frictional sliding, the important sinks in ridging.
QUESTION
By:
J. B. Mercer, Dome Petroleum, Canada

QUESTION:
What are the differences between the ice redistribution function described in this paper and that which has been used in the 'fine-scale model' presented by Leavitt et al. [1]?

AUTHOR'S REPLY:
The primary differences involve the participation function. Leavitt et al. used a participation function similar to that introduced by Thorndike et al. [2], in which ice of many thickness categories participates in ridging. In that model, the thinnest ice participates more than the thicker ice. The model presented in our paper makes a different assumption in that only the thinnest ice present participates in ridging. This means that there is no range of ice categories participating in the ridging; instead, only the thinnest ice participates.

The framework of this four-component model does not require such a specialized redistribution function. Reimer et al. [3] show the development of this model with a redistribution function that leads to the development given by Leavitt et al. The starting point for this assumption was the participation function selected by Thorndike et al. [2].

There are differences in the ridging process (which defines the range of thicknesses into which ice is ridged). However, I believe the details of this operator are less important than the distinction made in the participation function because the thinnest-ice-first assumption is the key assumption for allowing closed-form exact integration. In fact, exact integration has been performed in terms of arbitrary ridging processes.

REFERENCES
QUESTIONS
By:
V. R. Neralla, Atmospheric Environment Service, Canada

QUESTION:
How easy or difficult is it to increase the number of thickness categories in your model?

AUTHOR'S REPLY:
Mathematically, it is easy to change the number of thickness categories in the model. The framework of the model allows additional equations to be added with no change in form. However, to increase the number of categories, we must know more about the redistribution function. The more categories that are added, the closer the model comes to the full thickness distribution model of Thorndike et al. [1]. It also becomes more difficult to validate the performance of the model in comparison with observed changes in ice conditions. It is our belief that the four components capture the essential physics needed to estimate the ice strength in a wide variety of conditions. Therefore, we suggest thorough testing of this model before more categories are added. Of course, in late winter when the flat ice category thickens to roughly 2 meters, it could be useful to subdivide the thin ice category into two categories. Conceptually, this is easy.

It is possible to look more carefully at the rubble category to learn how to describe the properties of the ridges distributed in this category. Hibler [2] and Lepparanta [3] have introduced the ridge intensity distribution, and I believe these ideas could prove useful in better defining the rubble category. I must point out, however, that it is the thin ice and open water that control the strength of the ice cover. Therefore, in order to model successfully, we do not need more detail in describing the rubble ice, but we need accurate descriptions of the thin ice categories. What makes the introduction of ridging intensity desirable is the fact that it is observable in remote imagery and might allow us a better means of estimating the fraction of coverage of this category. It is also useful because the large features present the worst hazards for operations in ice-covered waters; thus, tracking of these individual features is also desirable.

QUESTION:
Since the growth of sea ice depends also on salinity distribution, how do you incorporate this into your formulation?

AUTHOR'S REPLY:
The growth rates may be specified in any appropriate way for the desired application. For developing the model, we have used climatological mean values. A more sophisticated
formulation, including salinity effects, could be introduced. Any formulation, empirical or analytical, that provides growth rates of ice at the thicknesses $h_0$, $h_1$ and $h_2$ is acceptable. It should be remembered that growth rates of ice of thicknesses $h_1$ and $h_2$ serve only to define the range of thicknesses of ice in the T and F categories, while the growth rate $f_0$ of the $h_0$-thick ice serves to define the rate of thermal advection of ice between the $f$ and T categories.

REFERENCES


Have you tried to instrument the island, with accelerometers perhaps, to measure the response of the island to the ice floes as they decelerate?

Note: Commercially available accelerometers to measure accelerations down to approximately $10^{-6}$ g are available.

Author's Reply by: M. Metge, Consultant, Calgary, Alberta.

We have not tried to instrument the island itself. Hans Island is a rock, the stiffness of its foundation is extremely high, therefore ice impacts would probably not cause measurable accelerations of the island. Accelerometers sensitive to $10^{-6}$ g were used on the ice floes themselves.
How many auger holes were needed to adequately measure the thickness distribution of the multi-year floes?

Author's Reply by: M. Metge, Consultant, Calgary Alberta.

About 5 to 10 holes were drilled in each ice floe. In addition, the average freeboard of the floe was calculated from lines surveyed with rod and level. The impulse radar thickness measurements were not successful because of equipment malfunction.

It is also important to note that the "effective ice pressure" measured is independent of ice floe thickness, as both the ice force and the area of contact are proportional to ice thickness. Therefore, as long as the floe is uniform in thickness, the actual value of the ice thickness is not a critical parameter in the pressure measurements.
DISCUSSION

By:

V. Vivatrat, Brian Watt Associates, Inc., U.S.A.

First I would like to congratulate the authors on presenting clearly a systematic and rational approach to ice load prediction.

My comment is regarding the failure load for in-plane flexure of ice ridges. The paper presents the failure load vs. ridge length for central impact of the ridges. Noncentral impact can give a higher failure load for any given ridge length.
Dr. Vivatrat's comment concerning the higher failure loads resulting from noncentral impacts is appreciated. The main objective of this paper was merely to demonstrate a rational approach towards a multi-modal ice-structure interaction analysis. Hence, the consideration of noncentral interactions was beyond the scope of this paper.

Noncentral interactions can indeed generate higher in-plane flexural failure loads if the associated rotational motions are restrained. Since the tendency to rotate is directly proportional to the interaction's excentricity, then rotational effects should be considered. The magnitude of the interaction force can then increase or decrease depending upon the given excentricity. In my opinion, many of the open water leads which occur at the sheet-ridge interface can be correlated with the superimposed rotational motions due to direct tension or excessive shear failures. When the sheet behind the ridge is significantly cracked, then the simple elastic foundation model becomes invalid and perhaps different mechanisms need to be introduced.
DISCUSSION

By:

C.R. Neill, Northwest Hydraulic Consultants Ltd., Canada

I support the authors' views on 2 points: (i) the value of analyzing ice-structure interaction by iterative calculations; (ii) the necessity of introducing an indentation coefficient varying with aspect ratio, i.e. penetration width/ice thickness. I have used a computer program to do iterative impulse-momentum calculations for the simple case of an ice sheet impacting a vertical cylinder. If the calculation is done with a constant effective ice pressure, the graph of force vs. time shows a quite gradual increase, as the width of contact is initially quite small. If an indentation coefficient is introduced as a function of aspect ratio, the force can be increased more rapidly at the beginning of the interaction. All the records I have seen of impact forces on bridge piers show a very rapid growth of force immediately upon impact. I conclude that the usually quoted relationships for indentation coefficient, which show a steep increase of aspect ratios below 1.0, are essentially valid, and that the curve should perhaps be even steeper than commonly assumed.
The authors would like to thank Mr. Neill for his discussion on the paper. As he points out in his discussion, there is a significant advantage to analyzing ice-structure interactions by iterative calculations. Furthermore, the introduction of an indentation factor varying with the aspect ratio provides a better match between the theoretical force-time traces and the actual measured records.

The slope of the indentation factor curve is the steepest in the range of small aspect ratios (less than one). The steepness of this curve can vary slightly depending upon the assumed failure criterion and failure mechanism. Regardless of these assumptions, a correction must be introduced into the mathematical solution so that the indentation factor will not approach infinity for aspect ratios approaching zero. The large magnitudes of the indentation factors at the low aspect ratios result in initial peak interaction forces which are in agreement with experimental records. This is, perhaps, a positive feature in the design of offshore structures due to their ability to accommodate large momentary kinetic energy during the initial stages of the interaction.
DISCUSSION
By:
G.A.M. Ghoneim, Dome Petroleum Limited, Canada

The paper is a step in the right direction. More analytical models are needed to describe the interaction phenomenon between ice and offshore structures. The points that require further clarification in the paper are:

1. What is the effect of the choice of the ridge end boundary conditions (fixed, hinged or free). Which boundary condition is the best?

2. The determination of the stiffness of the linear springs placed behind the ridge to represent the ice sheet (taken as \( k = \frac{f}{2} \)) does not make sense because the sheet ridge combined action should be a function of sheet stiffness rather than strength.

3. Derivation of the indentation factor in Eq.8 is not clear. The angle \( \theta \) should be shown in Fig.7 together with the assumed stress field.
DISCUSSION BY G.A.M. GHONEIM ON
FAILURE MODES AND FORCES OF
PRESSURE RIDGES ACTING ON
CYLINDRICAL TOWERS
Vol. 2 page 663

AUTHOR'S REPLY
By:
M. Rojansky, University of California, Berkeley, U.S.A.

Mr. Ghoneim's discussion, for which the authors thank him, requires a more detailed examination of several topics which are presented in the paper.

The boundary conditions which were chosen in this paper (fixed) reflect only one set of environmental conditions when the ridge is surrounded by and embedded in sheet ice. The fixed boundary conditions result in higher interaction loads which can represent the extreme design events. Other boundary conditions can be chosen based on the physical and geometrical properties at hand.

The foundation modulus is defined as the force per unit area (foundation pressure) per unit of displacement and as such it can be correlated with the strength characteristics of the sheet. Since it was assumed that the sheet will fail in crushing due to excessive shear then the corresponding assumed failure criterion (fig.6) was chosen. It is my opinion that some experimental work should be carried out to determine the magnitude of this parameter.

The indentation factor is derived by equating the external work and the internal strain energy. The ratio between the external pressure and the uniaxial compressive strength can then be extracted and this is, by definition, the indentation factor. The angle $\theta$ is defined by the intersection of the indentor's face and the wedge slope. The assumed stress field is described by a horizontal wedge bounded by two parallel vertical planes (fig.7).
For the keel soil interaction studies, would permafrost have to be considered in shallow water?

AUTHOR'S Reply:

Permafrost indeed would have to be considered in shallow water. Even further offshore where relic permafrost exists close to the seabed, ice scoring would be limited by the permafrost.

Offshore the ice keel/score interaction method takes into account local soil features as we determine the ice keel impact rate with the seafloor and multiply it by the score depth distribution which depends on local soil type and would certainly include permafrost if it was near the surface.

In shallow water we propose evaluation of shallow seismic records to determine pipeline burial and this method too would take ancient permafrost into account.
METHODS OF DETERMINING PIPELINE TRENCH DEPTHS IN THE CANADIAN BEAUFORT SEA

G.R. Pilkington
Dome Petroleum
Canada

and

R.W. Marcellus
Canada Marine Engineering Ltd.
Canada

Session A5

DISCUSSION

By:

J. Vaughn Barrie, C-Core, Memorial University of Newfoundland, St. John's
Newfoundland, Canada

Maximum score depths in the deeper water portion are assumed in the paper to be relic based on ice/keel score statistics measured in recent years. As we cannot yet prove this, isn't there a reasonable probability that over a 100 year return period the greatest TOP depth could be in medium to deeper water?

AUTHOR'S

Reply:

We do not feel that there is a cut off water depth for scoring, thus scores in deep water can occur today, but with very low probability due to the ice keel depth distributions indicated by submarine transects under the polar cap; however, extreme features capable of scoring to 60 m and deeper may not fit into the measured ice keel distribution.

Based on our measured ice keel depth distribution which falls off exponentially with depth, the probability of ice keel-sea floor impacts falls off in deeper water and indeed, our calculations suggest that the return period for ice keel impact of the sea floor beyond about 60 m is in excess of 1000 years. Thus if we accept 1000 year return period for damage to our pipeline we do not need to bury it.
METHODS OF DETERMINING PIPELINE TRENCH DEPTHS IN THE CANADIAN BEAUFORT SEA

G.R. Pilkington
Dome Petroleum Limited
Canada

and

R.W. Marcellus
Canada Marine Engineering Ltd.
Canada

Session A5

DISCUSSION
By:

Roger McGovern, Monenco Pipeline Consultants Limited, Montreal, Canada

The ice keel/scour statistics method and the TOP depth optimization procedure each result in curves which show the depth to top of pipeline decaying to zero at water depths of 48 m and 52 m respectively. It has been my experience in ice scour related studies that scour depth will increase with water depth as a result of larger ice masses which are required to generate the scour. The rate of occurrence of ice scours will decrease with increasing water depth and, at some determined water depth, it may no longer be economic or practical to provide protection to the pipeline. However, at that water depth, pipeline burial would reduce quite abruptly from a maximum to zero. There would appear to be no justification for a decay curve. Since it is improbable that there would be a gradual reduction of pipeline burial with increasing water depth. In general, I do not believe there should be a correlation between the number of scour events and the design burial depth of the pipeline except, of course, when the number of scours approaches zero. I found the paper of great interest and I would appreciate the author's comments on the above discussion.
METHOD OF DETERMINING PIPELINE TRENCH DEPTHS IN THE CANADIAN BEAUFORT SEA

G.R. Pilkington  
Dome Petroleum Limited  
Canada

and

R.W. Marcellus  
Canada Marine Engineering Ltd.  
Canada

Session A5

AUTHOR'S reply:

Firstly, within our study area based on the existing statistics, the tendency of maximum score depths increasing with water depth is observed out to about the 47 m water depth as shown in Figure 3 of the paper.

Secondly, the justification for reducing the TOP depth gradually comes from the seabed slope in the area of interest (which is approximately ≥ 1:1000). For steeper (rocky) shorelines the abrupt change from score depth to zero (see Marcellus and Palmer in POAC 1979) would be more appropriate. In other words the gradual reduction in TOP depths in deeper water is a result of the combined factors of minimizing the trenching requirements while maximizing the pipeline protection.

Thirdly, with respect to your belief that no correlation between the number of scores and the design burial depth for a pipeline exists, we must disagree fundamentally. Burying below the maximum observed score depth, as outlined in the paper would not provide adequate protection for a pipeline in shallower water and be extremely unrealistic in deeper water.

In the ice keel score statistics method rationalization that not every score results in a maximum depth of score for a given water depth is emphasized. Given the fact that one score may occur in a given water depth during a given return period we believe that it would be unrealistic and uneconomical to assume that the keel scores to the maximum depth observed in the area. We do account for a realistic margin of safety by designing to a higher return period.
DISCUSSION ON
MODEL TESTS OF SEA BOTTOM SCOURING
R. Abdelnour, D. Lapp, S. Haider
S.B. Shinde and B. Wright

T.R. Chari
Memorial University of Newfoundland
Associate Professor
Faculty of Engineering, St. John's, Nfld.

The authors are to be complimented on their research attempt of a problem of great interest to the offshore operators in the arctic and subarctic latitudes. While it is recognized that the results of the reported study are still under the constraints of proprietary rights, the impact of the paper would have been enhanced by the addition of some results. Results of tests similar to those described by the authors, applicable to soil cutting by bulldozer blades and to iceberg scours, have been published in the open literature. Comparative analyses of the authors' findings with earlier results would have been helpful. Technical discussion of the paper in its present form is somewhat difficult. However, one general comment may still be appropriate. The horizontal force records shown in Fig. 8 of the paper appear to correspond to the three runs shown in Fig. 6 and is to be expected in that form. The records of the vertical force show disproportionately large magnitudes. A portion of this force is indirectly attributable to the scoured soil which builds up as a ridge of surcharge. Some caution is therefore suggested in interpreting the measured vertical forces. There are many limitations in the scaled interpretation of soil tests. It will be of interest to know how the authors solved this important issue.
"SEA BED FEATURES IN THE BLAENG A AREA WEDDEL SEA, ANTARCTICA" BY R. LIEN

Vol. 2 page 706

DISCUSSIONS

By:

R.H. Belderson Institute of Oceanographic Sciences England

This paper is to be welcomed because of the new data it presents from a vast region hitherto unexplored by side-scan sonar. Features formed by ice must be an extremely common element in the morphology of the sea floor around Antarctica. Dr. Lien has illustrated some of the intriguing patterns found there and offered plausible explanations for these. The regularity of formation and subsequently largely undisturbed nature of the "washboard pattern" is remarkable. It may perhaps seem surprising that under the mechanism suggested the leading basal protrusion of the ice should be able to impart this pattern, with so little subsequent disturbance by the passage of other protrusions in the base of the ice. However, if the suggested mechanism is correct, then perhaps the rise and fall of the tide associated with a more or less steady horizontal force might seem to be the best way of producing such a regular pattern.
Lien and his colleagues are to be complimented on assembling data on seabed features from this little-known region of offshore Antarctica, particularly because of important analogs in the northern hemisphere. Iceberg scour marks and related features are to be expected on this part of the continental shelf, where mainly tabular icebergs can be found. The coastline of Antarctica from the east side of the Antarctic Peninsula (the Larsen Ice Shelf at about 65°W), extending eastward along the coast for about 4,500 kilometers to an area (about 70°S, 30°E) of the author's study site is essentially a coast of nearly uninterrupted shelf ice of varying thickness at the barrier sides (Fig. 1). The Filchner and Ronne Ice Shelves, in the Weddell Sea embayment, comprise the second largest mass of shelf ice in the continent, and may well spawn tabular icebergs of great thicknesses that could possibly produce the scour features at the unusual depths that were measured in Lien's study. Some of the shelf thicknesses are known at various locations along the coast in this sector of Antarctica as a result of ice-drilling programs and geophysical surveys. For example, drilling in the Lazarev Ice Shelf near a Soviet station at 70°S, 12°E gave ice thicknesses from 350 to 450 meters. The Norwegian-British-Swedish Expedition (1949-52) provided detailed information on the ice shelf, including a 100-m-deep drillhole that did not penetrate the shelf, near their station at Maudheim (71°S, 11°W), very near Lien's study area. Unfortunately, no information was given in Lien's paper on ocean current measurements, which would be
Figure 1. Map of Antarctica showing place names mentioned in text.
helpful in determining where the icebergs in the area originated. I refer the
author to his countryman T. Kvinge at the University of Bergen, T.D. Foster, Uni-
versity of California, Santa Cruz, and S.F. Ackley, U.S. Army CRREL, Hanover, New
Hampshire, who have participated in several shipboard programs in the Weddell Sea to
measure currents and sea ice drift by means of buoys left there year-round [1, 2, 3]. The source of the icebergs, and hence approximate drafts of the bergs, might be
useful in determining whether sea bottom grooves at great depths might be contem-
porary or relatively ancient.

I have some reservations about Lien's proposition to explain the washboard pat-
tern of seabottom features. In Figure 9 he outlines a method that relies on a
recurring grounding-tilting-grounding mechanism that advances the berg along the
bottom. Bergs moving in tandem apparently produce the ridges found transverse to
the washboard patterns. Although I do not necessarily disbelieve this scenario
(mainly because I cannot come up with a better explanation), I question the regu-
larity of the washboard undulations. Why are they so evenly spaced? Why are the
washboard features and transverse ridges mostly unaffected by seabottom topography,
instead of showing an occasional arcuate pattern that would imply that an iceberg
rotated sidewise on a horizontal plane as a result of an occasional mismatch of
seabottom topography and iceberg bottom topography? Figure 9 indicates that the
berg maintains a steady course, perhaps because it is bounded by sea ice (p. 713),
but because the berg pivots upward on a hinge line (its forward base) to an unstable
position (fig. 9), it routinely breaks free of its "containing" sea ice and would
thus have more freedom of movement as a result of ocean currents (which could even
be contrary to wind movements that dictate the direction of movement of the
surrounding sea ice). Why doesn't the berg erase (by crushing) the ridge produced
previously, as the berg settles back to its position of new equilibrium (fig. 9)?
It would certainly have enough mass to do so, especially as it overrides uncon-
solidated sediments on the relatively shallow slope so indicated. John Anderson of
Rice University, Houston, Texas, and his colleagues should be consulted about sedi-
mentation processes in the Weddell Sea, of which there is published information [4, 5, 6, for example].

There is another possible explanation for the washboard pattern and the trans-
verse stripes that may be worth discussion. Hughes [7] has proposed a series of ice
streams within the ice sheet of West Antarctica that are capable of discharging con-
siderable volumes of ice into their contiguous ice shelves, and hence into the sea,
if they become unstable as a result of some triggering action related to the waning
stages of an ice age maximum. The entire ice sheet thus deteriorates as a result of
massive losses of ice by means of these ice streams. The details and chronology of why and how ice stream surges occur are not relevant here, but can be found in Hughes [7]. The point is that Lien's study area is immediately offshore from a major ice stream that drains the ice sheet (see Hughes, Fig. 1), and a surge of the ice stream would force the ice shelf (Riser-Larsen Ice Shelf) seaward at a higher velocity than normal. The concurrent action of a retreating grounding line beneath the ice shelf would permit seawater to come in contact with the bottom of the shelf, abating and thinning it toward the barrier. The force in Lien's Figure 9 would thus be the ice stream surge, pushing the shelf or its calved tabular bergs seaward. The direction of the force would also seem to agree with the orientation of the washboard undulations and of the parallel stripes ("...at right angles to the barrier front" -- p. 712). Assuming the West Antarctic ice sheet's disintegration to have begun many thousands of years ago [8, 9], the washboard pattern's antiquity (p. 716) would also be compatible with the relatively younger features caused by contemporary icebergs and superimposed over the washboard undulations. The one thing the surge mechanism does not explain clearly is the regularity of the washboard undulations, unless the barrier advance by surging and the concurrent tidal action worked in consonance to lift, advance, and lower the barrier or its fronting bergs in a regular manner.

Unfortunately, this discussion seems to raise more questions than it answers, particularly regarding the origin of the features. However, even though Lien's proposed mechanism (fig. 9) for a unique feature such as the washboard pattern may be correct, it needs more analysis before it becomes more realistic. This would be especially important as baseline data for the design of seabottom and sub-bottom structures that are required for hydrocarbon development and exploitation in iceberg-infested waters.

REFERENCES


I would like to thank Dr. Splettstoesser for his discussion of the paper and for the summary of the ice conditions in the Weddell Sea area.

Dr. Splettstoesser has discussed thoroughly my theory of the formation of the washboard pattern, and he evidently has some reservations about it. At this stage I think this is to be expected since it is the first attempt to explain the formation of this new feature. Since we have very little data on the phenomena, there may be many possible theories. But when more data about the phenomena is obtained, then the genesis will hopefully become clearer.

Thus all of Dr. Splettstoesser's reservations about the formation theory of the washboard pattern are pertinent; so I will give a short explanation of what I have taken into account in formulating the theory:

In the area where the washboard pattern is formed waterdepths vary from about 280 m down to at least 400 m. The pattern is formed on a smooth gentle slope with an inclination of about 1:150. The grooves or ridges in the washboard pattern are parallel to the slope and the distance between each pair is about 10-15 m. If the pattern is produced by icebergs it is thought that tabular icebergs are responsible.

If the icebergs do not break too often these conditions may be quite stable for long periods, and there should be no factors that disturb the rhythmic movements of the icebergs under these conditions.
Since the distances between the grooves or ridges in the washboard pattern are approximately 10 - 15 m; the vertical rotation of an iceberg should be very small (about 1 - 3°) because of its deep draught. In that case an iceberg will still be quite tightly held by the sea ice and individual horizontal rotations or sideways movements will be prevented.

As the icebergs move up the slope they will be lifted more and more. This elevation may be produced if the icebergs do not go right back to their equilibrium as they were before they grounded. That is, they will acquire a more and more stooping position as they move upwards. But the slope is very gentle, and if the movements are slow enough the icebergs will possibly thaw and shrink so much that the elevation may be unnecessary.

Because of the slope and the very low relief of the pattern and the shape of the tabular iceberg's base, it is possible that the imprints from the leading edge of an iceberg will survive overriding by the rest of the iceberg. The possibility of preservation of the feature increases if the iceberg still has a stooping position when it is in its lowest position, as described above.

Dr. Splettstoesser also asks about the origin of the icebergs, and suggests that they may come from the Filchner/Ronne Ice Shelf. This is not lightly. Literature of tracking instrumented icebergs (1 and 2) shows that at present icebergs enter our study area from the east. This also fits well with the bathymetry of the area which has an opening to the east and north, to let the bergs into the area.

As to the sedimentation processes in the Weddell Sea the norwegian investigations have yielded considerable information (3), but as this paper had to be limited, and as the main purpose with it was to describe the features, these processes were not discussed.

As to Dr. Splettstoesser's alternative explanation of the formation of the washboard pattern, I have also considered the shelf ice as a possible source of the drifting forces on the icebergs (4. line page 713). However, I have not considered that the shelf ice edge has produced the pattern, mainly because of the linear ridges, at right angles to the washboard pattern, which I think are produced by soil pressed up between the icebergs. I also think that tidal movements may influence the time of occurrence of the break between the soil and the iceberg (4. line page 714). However, from the records it looks like the
movements have not been large enough to get the bergs floating, because the linear ridges mostly seem continuous, which I would not have expected if the icebergs were afloat between each imprint.

Further Dr. Splettstoesser suggests that our study area is immediately offshore from a major ice stream that drain the ice sheet. This is unfortunately not correct. I presume he refers to Stancomb-Wills ice stream, but this is separated from Riiser-Larsenisen by a large ice rise, and a high hypothetical surge of this ice stream would not force the ice shelf seaward at remarkably high velocities. (O. Orheim pers. com.).

LITERATURE


I agree with the author's interesting interpretations of the ice scour features he has illustrated and described. Of particular interest is the washboard pattern with linear ridges formed by a group of closely spaced, wobbling icebergs. This compound pattern formed by individual bergs wobbling at different frequencies but drifting in a uniform direction described by the orientation of the linear ridges, does not seem to have been previously observed. It would appear that the patterns were formed by tabular bergs which presented a large surface to the seabed. Apparently the icebergs were fractured but still maintained their integrity during drift, and individual fragments were free to develop a characteristic rocking motion. The author favors the leading edge of the iceberg as the cutting edge and this in many respects appears to be the logical choice; however, it is surprising that there is no evidence of interference in the pattern from scour introduced by the base and trailing edge of the wobbling berg. If the trailing edge were to impart the impression to the seabed, there would be less chance for subsequent interference. Future sidescan sonar mosaic studies across such features will be of value in answering such questions as to how long fractured iceberg maintains its integrity as it moves in contact with the seabed, the importance of slope of the seabed with respect to the direction of movement of the grounded berg, the influence of winds and currents, and the influence of the sedimentary character of the seabed on the patterns.
I would like to thank Dr. King for his comments and discussion on the paper. Dr. King's comments on the undisturbed impressions on the sea bed, possibly made by the leading edges of the icebergs, may be pertinent. However, if the icebergs are tabular the inclination of the slope (ab. 1:150) will prevent the trailing edge of the icebergs from disturbing or producing interfering patterns on the features made by the leading edge.

Further I should point out that this is the first attempt to explain a new feature and I think the genesis will become clearer when more investigations, like what Dr. King has proposed in his comments, have been done.
DISCUSSION
By:
B. Ladanyi, Ecole Polytechnique, Montreal, Canada

The rigid plastic solutions used by the authors for interpreting the static penetration tests are known to be valid essentially for stiff clays or dense sands, where the deformability has only a little effect on the penetration resistance. However for weak and loose seafloor sediments it may be better to use the solutions which are available in the geotechnical literature (Schmertmann 1975) and valid for deformable materials.

Reference:
AUTHOR'S REPLY

BY:

T.R. Chari, Faculty of Engineering and Applied Science, Memorial University of Newfoundland, St. John's, Canada

The authors would like to thank Dr. Ladanyi for the comments which are very appropriate in relation to the surficial sediments on the seafloor. The compression and deformation of the soil has to be accounted for during a final analysis of the field results when the instrument is used at sea. The results reported in the paper primarily pertain to laboratory tests and the assumption of a general shear failure for such cases is justified.
DISCUSSION
By:

D.V. Reddy, Faculty of Engineering and Applied Science,
M.U.N., St. John's, Nfld., Canada

Can you get strain-dependent "G" values for the frozen soil specimens in the Calgary Apparatus.
DISCUSSION BY D.V. REDDY ON

"DYNAMIC AND STATIC CREEP TESTING OF ICE AND FROZEN SOILS"

Vol. 2 page 726

AUTHOR'S REPLY

BY:

H. YOUSSEF, Génie Civil, Ecole Polytechnique de Montreal, U.de M., Quebec, Canada

and

R. KUHLEMEYER, The Dept. of Civil Eng., The Univ. of Calgary, Alberta, Canada

The authors would like to thank Dr. Reddy for his discussion on the paper. The discussion question is a bit vague, but we assume that Dr. Reddy refers to operating the system as a resonance column device. The answer is "yes", but the range of strain levels would be "low strain". The vibration unit was designed by Drnevich as a resonance column device. The range of operating strain levels could only be increased by increasing the power supply and magnet sizes.
R. E. Muench, SAI Northwest, Bellevue, Washington

I would like to make a point vis-à-vis stress-strain relation in the Bering. Both the Bering (in winter) and the Beaufort-Chukchi (in summer) are marginal ice zones characterized by a free ice-water boundary. Ice rheology is not well understood in marginal ice zones for a number of reasons. Firstly, off-ice winds break the ice up, leads open, and there are no compressive forces. During on-ice winds there can be compressive forces but there is also often a sea swell which acts to break the floes into smaller pieces. Hence, behavior is a function of wind direction. Secondly, the actual ice edge in the above cases is the site of melting and the attendant changes in ice behavior brought on as the freezing point is approached. Finally, the lack of extensive ridging and rafting in the Bering, which is due primarily to lack of compressive forces, leads to a smoother ice surface and lower ice-water and air-ice form drag than in the Arctic Ocean. Bering Sea ice is, generally, first-year ice less than 1 m thick.

For the above reason, existing models of ice behavior and the resulting computations relating stress-strain should be used with caution in marginal ice zones such as the Bering Sea and the Beaufort/Chukchi under summer conditions.
It is very useful to appreciate Dr. Muench's remarks about the meso-scale stress-strain behavior near the free edge of the ice pack being a function of wind direction. There is unfortunately a dearth of direct evidence for his assertion that the ice-water drag near the free edge of the pack is lower in the Bering Sea than in the Arctic Ocean. In fact, very little information is available on under-ice topography of the free edge of the ice pack, and in order to gain a better understanding of ice dynamics in the marginal ice zone, upward-looking sonar data on this subject should be acquired.
Discussion by:
K.D. Vaudrey, Vaudrey & Assoc., Inc. U.S.A.

Q. Were your sonar auger holes that were drilled on the multiyear floes away from the ridges' influence or were they located on the ridge shoulders?
A. Probably on the shoulder - we wanted to stay within, see D.F. Dickins, meters of the ridge crest.

Q. Did you drill any floe thickness holes away from embedded ridges?
A. No.
AUTHOR'S REPLY
By:
D. Dickins, DF Dickins Associates Ltd., Vancouver, Canada

The sonar auger holes were drilled at distances varying from 35 to 80 m from the sail peak. Keels tended to be quite asymmetric in shape relative to the above water profile, but most keel half widths were less than 35 m indicating that the sonar holes were generally outside of the major zone of ridge influence.

The study was not intended to survey general multi-year ice thickness, but several test holes well away from ridges measured thicknesses in the range 4.7 to 5.5 m.
Your study has found keel to sail ratios that are larger than corresponding values reported in the Southern Beaufort Sea. You suggested that because of the origin of the multiyear ridges studied (80° N - north of) they were exposed to less ablation than those in the Southern Beaufort Sea. However, if these ridges were less ablated, the sail height should be larger, implying that the keel to sail ratio should be lower for your ridges. This is the opposite trend to that which you reported. Is there an alternative explanation for your larger keel to sail ratios, e.g., they are much younger than those studied in the Southern Beaufort?
DISCUSSION BY J. KREIDER ON
"MULTI-YEAR PRESSURE RIDGE STUDY,
QUEEN ELIZABETH ISLANDS"

Vol. II, page 765

AUTHOR'S REPLY
By:
D. Dickins, DF Dickins Associates Ltd., Vancouver, Canada

There are a number of possible explanations for the large keel/sail ratios reported in this study. Figure 7 of the paper shows a general trend toward higher ratios with more gradual sail side slopes. This result is to be expected in the high Arctic where the extremely short open water season combined with minimal seasonal water temperature fluctuations should lead to a generally constant keel geometry and draft over many years. The brief period of summer melt will result in surface changes only, thereby reducing sail heights and increasing the apparent keel/sail ratio.

In the Beaufort Sea, previous ridge studies have shown much lower and more uniform keel/sail ratios associated with multi-year ridges (eg. Kovacs, 1975). This difference may be due to a charged distribution of ridge ages penetrating to the more southerly latitudes, combined with the greatly increased potential for differential sail ablation and keel melt in the warmer summer waters of the Beaufort Sea.
MULTI-YEAR PRESSURE RIDGE STUDY,
QUEEN ELIZABETH ISLANDS

Vol. II  p. 765

Discussion by:
Mary E. Thro, Shell Development, U.S.A.

How are you estimating the ages of the multiyear ridges?
I would not have much confidence in estimating whether a ridge is 5 or 10 years old, for instance.
AUTHOR'S REPLY
By:
D. Dickins, DF Dickins Associates Ltd., Vancouver, Canada

It was not intended to indicate in the paper that ridge ages could actually be estimated. Some qualitative indication of relative ages can be gained from surface observations of the sail, i.e., are the individual blocks still clearly visible, or has the entire above water mass melted and fused into a smooth hump. Successive summers of melt gradually reduce the sail side slopes and this can be an indicator of relative age (see Figure 7 of the paper).
Discussion of
MULTI-YEAR PRESSURE RIDGE STUDY,
QUEEN ELIZABETH ISLANDS, by
D.F. Dickins and V.F. Wetzel

A. Kovacs
U.S. Army Cold Regions Research and Engineering Laboratory,
Hanover, New Hampshire

This paper gives unique information on multi-year ice features with sail height (H) to keel depth (D) ratios averaging 1:5.6. Typical H/D ratios from the published literature for multi-year pressure ridges are 1:3.25 for one ridge from reference A, 1:3.23 average for 20 ridge measurements from reference B, 1:3.0 average for three ridges (no. 5, 6 and 7 not included in reference B) from reference C, 1:3.28 for one ridge from reference D, and 1:3.28 for one ridge from reference E. These 26 H/D ratios imply that most multi-year pressure ridges have a sail height to keel depth ratio of the order of 1:3.2. It must be emphasized that the ridges discussed in the above reports were separate features, i.e. they were not ridges in a multi-year ice rubble field.

Cross section profiles 3 and 4 presented by Dickins and Wetzel suggest that the ice in ridge 13 was once a rubble field or was situated in an area of highly irregular surface relief. An aerial photo of the ridge site would help the reader better understand the surrounding sea ice terrain. If the ice features studied were indeed not separate, distinct features, then the H/D ratio results would not be expected to agree with those previously published for multi-year pressure ridges.

The fourth line in the second paragraph of page i should not read "37 meters was observed," but rather "40 meters was measured," since page 4 and Figure 3 both indicate a 40-m keel. The seventh line should end "keel/sail area ratio of 9.32, was 0.93," not "keel/sail ratio of 9.32, was 0.92." Since the keel/sail area ratio is given as 9.32 and is equal to \( \rho_i/\rho_w - \rho_i \) then, given a water density \( \rho_w \) of 1030 kg m\(^{-3}\) from the text, the density of the ice \( \rho_i \) must be 930 kg m\(^{-3}\). In any event, an effective bulk density in excess of 900 kg m\(^{-3}\) for multi-year ice is considered high. While an effective bulk density of the sail ice may be near 900 kg m\(^{-3}\), given the high H/D ratio of 1:5.6 in this paper, I believe the keel ice has a substantially lower effective bulk density, perhaps as low as 750 kg m\(^{-3}\) due to a large void structure. This density difference is in accord with the authors'
reasoning on sail and keel ablation, and would help explain the large H/D ratio noted but not the high keel/sail area ratio given.

In Figure 6 the number of ridges shown for Kovacs and Mellor (1974) should be 1 and the number for Kovacs et al. (1973) should be 3.


References
DISCUSSION BY A. KOVACS ON
"MULTI-YEAR PRESSURE RIDGE STUDY,
QUEEN ELIZABETH ISLANDS"

Vol. II, page 765

AUTHOR'S REPLY

By:

D. Dickins, DF Dickins Associates Ltd., Vancouver, Canada

The discussion indicates that if many of the ridges surveyed were once rubble fields, then it is not surprising that the keel/sail ratios departed from previous results for the Beaufort Sea. The author maintains that the ridges surveyed were separate discrete features with a distinct sail running for over 100 m in one general direction.

The effective bulk density of 0.9 - 0.92 derived from sail/keel area estimates, appears inconsistent with previous "southern" observations of large void spaces in ridge keels. Even applying a realistic maximum error of ±20% to the mean areas derived from largely 1/2 profiles obtained in this study, the effective bulk density of the ridges studies is still high. It is difficult to say whether or not the implied almost complete keel consolidation is possible. No other non-proprietary observations of deep draft multi-year ridges have been made in the Arctic Islands region. It is not inconceivable that over a 5 to 10 year period, ridge keels could approach total consolidation in the high Arctic.
Comments on "Ice Studies Aid in the Successful Completion of the Norton Sound C.O.S.T. Well" by Wolfson & Evans

W. Spring
Mobil Research & Development Corporation
Dallas, Texas

The authors, and ARCO Oil & Gas Company, are to be congratulated on performing a successful operation in a frontier environment. This paper shows the need for environmental data in the pre-exploration phase of hydrocarbon production and also the inter-relationship of several data bases to the overall project plan. The requirement of an ice breakup and freezeup model is obvious from the author's discussions and I wonder if other meteorological parameters, besides the mean atmospheric pressure, were tried in the development of the model described in the paper? In particular, I suggest the use of storm tracks and the resulting position of the Aleutian Low. The seasonal variation of storm movement is shown in Brower, et.al. "Climatic Atlas of the Outer Continental Shelf Waters and Coastal Regions of Alaska, Vol. II, Bering Sea." Its effect on the ice edge, or the effect of the ice edge on storm movement, are fairly obvious when compared.

I would also request the authors to discuss the number of storms actual hindcast to determine the 100 year return period event and the use of only 24 years of data. I realize data is sparse prior to 1955, but was any attempt made to account for storms outside the 1955-78 frame or was this period ignored? As the error bands are fairly wide, when a limited data set is used to extrapolate to the hundred year return period, some method should be used to account for the time frame not in the study period.
The authors appreciate Dr. Spring's review of the paper and constructive comments. Our first comments are in regards to the question whether other meteorological parameters other than the mean atmospheric pressure were tried in the development of the ice freezeup and ice breakup models. In the initiation of the project, it was recognized that several parameters such as atmospheric pressure, air and sea temperatures and storm tracks could affect ice conditions in Norton Sound. It was also recognized that the satellite data base was limited. For the years studied initially (1972-1978), the models developed were very accurate in predicting ice breakup and freezeup and the only meteorological parameter used was the mean atmospheric pressure. The predictions were of sufficient accuracy that including other meteorological parameters in the models would not have changed the results. It must be stressed though that considerable effort was undertaken to define the spatial extent of the atmospheric pressure data.

It was recognized that using such a small data base for predicting ice breakup and freezeup for future years could result in considerable error if data from subsequent years varied considerably from the original data base. Thus, the reason for testing the accuracy of the forecast models during 1979. Results from the 1979 study indicated that other meteorological data would need to be incorporated into the models thus the reason for relying on subjective forecasts which took into account temperature and storm data.

Studies are continuing to upgrade the models. Ice data for 1979, 1980 and 1981 and other meteorological parameters such as temperature and storm tracks will be incorporated.

In regards to Dr. Spring's comment on the number of storms studied to determine the 100 year return interval, thirty-six storms were studied during the 1955-1978 period. Storms prior to 1955 were not hindcast because of sparse data and low frequency that synoptic weather maps were prepared. The low frequency of reliable synoptic maps could result in considerable errors in analyzing windfields.
This paper is a very useful contribution to the continuing discussion of how best to describe, statistically, sea ice roughness caused by compressive forces. In particular, it deals with the results of attempts to describe this roughness as a series of discrete events called ridges, and the relative merit of four different sets of criteria for mapping from the natural event domain to the ridge domain.

These four techniques all involve the use of a minimum peak height (a cutoff) and some falloff criteria on either side of the peak, which is used to differentiate between main peaks (ridges) and small perturbations. These four sets of criteria are compared to the first- and second-order exponential distributions for ridge height, using the same data set. Although one of these sets is based on a misunderstanding of the techniques outlined in Lowry and Wadhams (1979), all are valid methods for describing ridges.

It is interesting to note that, although the differences in the distribution caused by variations in the selection criteria are not large, they show that the choice of model can be affected by the choice of criteria. For example, the Rayleigh criterion favours the first-order exponential (Wadhams) distribution, whereas the two-foot criterion favours the second-order (Hibler) distribution, using the same data and a chi-squared test.

The misunderstanding mentioned above also extends to equating the mean spacing of the ridges to the reciprocal of the parameter \( \mu \) in the first-order exponential spacing distribution. As pointed out in Lowry and Wadhams, \( \mu \) is related to the events before the ridge criteria are imposed. While this makes a small difference to the general shape of the spacing distribution, it could more seriously effect the predictions of
extreme values for ridge height, as evidenced by the data presented.

The authors thank Mr. Lowry for the clarification of our misunderstanding of the ridge definition described by Lowry and Wadhams (1979). The ridge definition used in our paper was a 50 percent Rayleigh criteria with the added criterion that the spacing between two adjacent peaks or events must be less than a critical distance $X_{\text{crit}}$ for the events to be considered separate ridges. This distance is given by

$$X_{\text{crit}} = H_m \cot \alpha$$

where $H_m$ is the height of the higher ridge and $\alpha$ is the approximate slope angle.

As presented by Lowry and Wadhams, ridges are defined only by the minimum spacing $X_{\text{crit}}$, and there is no requirement for a minimum drop between peaks. We have reanalyzed the data, using this corrected definition, with values of $\alpha = 7^\circ$ and $\alpha = 12^\circ$ to get the parameters $N$, $\bar{\mu}$, $\bar{h}$, and $\beta$ presented in Table 1.

### Table 1

PARAMETERS FOR CORRECTED LOWRY AND WADHAMS RIDGE DEFINITION

<table>
<thead>
<tr>
<th></th>
<th>1. Rayleigh 50%</th>
<th>2. Lowry &amp; Wadhams $\alpha = 12^\circ$</th>
<th>3. Lowry &amp; Wadhams $\alpha = 7^\circ$</th>
<th>4. Rayleigh 50% plus $\alpha = 7^\circ$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$N$</td>
<td>538</td>
<td>548</td>
<td>464</td>
<td>441</td>
</tr>
<tr>
<td>$\bar{\mu}$</td>
<td>13.9</td>
<td>14.4</td>
<td>12.2</td>
<td>11.6</td>
</tr>
<tr>
<td>$\bar{h}$</td>
<td>4.6</td>
<td>4.6</td>
<td>4.6</td>
<td>4.7</td>
</tr>
<tr>
<td>$\beta$</td>
<td>1625</td>
<td>.629</td>
<td>.613</td>
<td>.588</td>
</tr>
</tbody>
</table>

The columns for Rayleigh 50 percent and Rayleigh 50 percent plus $\alpha = 7^\circ$ are given for comparison from our paper. The value for $\mu$ given here is the average number of ridges per mile given for comparison with the other definitions and not the $\mu_e$ used.
in the Lowry and Wadhams paper.

The lines for cumulative probability for ridge height, \( F(H/L) = \Pr \) (all ridges in \( L \) miles \(< H \)) are very similar for these four definitions, even for large ridge heights.

An interesting point is that it is possible to choose a value for \( \alpha \) which will match the number of ridges determined by Rayleigh 50 percent criteria, but the two definitions do not necessarily pick the same events as ridges since combining the definitions gives a smaller number of ridges than either of the definitions individually. This difference appears in the low ridge height bins. Ridges are identical for all four definitions for heights above 9 ft; and while there is a maximum difference of 109 ridges between columns 2 and 4, only 5 of these ridges are greater than 6 ft.
DISCUSSION BY:
David F. Dickins, DF Dickins Associates Ltd. Canada

Researchers such as Barnett and Walsh have evaluated historical summer ice records in an effort to identify periods or cycles of ice severity of the Alaskan coast. Have you examined your data for similar trends?

AUTHOR'S REPLY
G.F.N. Cox, U.S.A. CRREL

No, although the regional summaries clearly showed cycles similar to those discovered previously.
The central aim of this paper is to provide some idea of the wind and wave climatology in Lancaster Sound in the Canadian Arctic. The area is of interest in offshore oil and gas exploration and in transportation, and quite naturally Lachapelle couches his derived statistics in terms of exceedance diagrams, return periods and extremes. One wants to be careful to err on the side of safety when compiling climatologies for engineering purposes especially in such environmentally delicate areas.

Lachapelle's work appears to strike a balance between two opposing sources of uncertainty in the wave climatology. On the one hand, the spatial and temporal smoothing inherent in the use of the Fleet Numerical data (6-hourly sea-level pressures on a 381 km grid) leads to significant underestimates of the wind climatology compared to the Summary of Synoptic Meteorological Observations (SSMO) in Figure 7. On the other hand, the use of minimum monthly ice cover and the ignoring of isobaric curvature (storm size) both tend to provide enlarged fetches to the Bretschneider hindcasting method. The second effect seems to more than compensate for the first and the hindcast waves are appreciably larger than the SSMO wave statistics.

Lachapelle points out these difficulties in deriving the wave climatology and acknowledges the rough ("first guess estimate of sea state") nature of his derived statistics. However, users of such climatologies often lose sight of this and rely rather more heavily on the given climatological statistics than is warranted. It is for this reason that I would be rather wary of a method of balanced errors - it being entirely possible that high winds and minimum ice cover (long fetch) could occur together or even be weakly correlated. The cross-over point of SSMO and hindcast waves at 4 m in figure 3 suggests this.
Would it not be better to build some skill, rather than mere interpolation, into the computed winds from the fleet Numerical grid, so that the computed winds are in better agreement with observations? In this context it is worth remarking that an accurate wind climatology is useful of itself, both for engineering purposes and in the calculation of ice movement.

DISCUSSION BY M.A. DONELAN ON
WINDS AND WAVES IN LANCASTER SOUND

REPLY by: J.B. Maxwell, Supt., Arctic Meteorology Section, Atmospheric Environment Service, Downsview, Ontario, Canada

Dr. Donelan's comments in regard to having some skill built into the winds computed from the Fleet Numerical gridded pressure data are well taken. This approach is presently being undertaken in work at AES, by testing the grid winds against surface observational data. Pending results of this work, Lachapelle's wave climatology is considered to be a good first cut as indicated in his paper. One can only monitor the use of this climatology, for example, through reviews of environmental impact statements incorporating it, and point out where inappropriate or incorrect conclusions have been drawn.
What range of ice strength values or stress levels were obtained in your standard simulation model? Are they high enough to support your evidence that neglect of $V \cdot g$ caused free-drift to be a poor approximation, or could the same result be found by using different mass, drag coefficients, winds or currents?
Strength magnitudes and stress levels were not examined in this study and may have little relative meaning in any case. The question can be answered by sensitivity tests that were carried out after the paper was completed. Figure 1 shows 60-day thickness fields for a zero winds simulation, a zero currents simulation and a simulation in which the currents were taken to be the 60-day average ice velocity field of the standard run. This latter simulation included the daily wind forcing as used in the standard run. In all cases, the results tend to be what one would expect from an alteration of the basic forcing fields. Without winds, coastal build-ups do not exist. Without currents, on the other hand, the results are quite similar to the standard run because the magnitudes of the geostrophic currents used in other runs are quite small. When these magnitudes (specifically along the coast) are significantly increased, only slightly larger coastal build-ups occur as opposed to the standard run. The predicted buoy trajectories also showed expected results. With zero winds the predicted final position was far shy of its observed position. With zero currents, the predicted trajectory was nearly the same as that predicted by the standard run and with the increased currents the predicted final position was too far to the south of the observed position. Likewise, changing ice mass and drag coefficients to other values within reason would certainly affect results, but not as drastically as did the zero strength simulation. The salient point is that the ice must be allowed to have strength to prevent the unreasonable thicknesses near the coast caused by the excessive shoreward velocity component and lack of ice interaction.

![Figure 1](image_url)

Figure 1. 60-day thickness fields for a) zero winds simulation; b) zero currents simulation; c) modified currents simulation.
DISCUSSION
By:

V.R. Neralla, Atmospheric Environment Service, Canada

You have stated that the dynamical simulation for 60 days gave you the best fit. Is it not true that the thermodynamical component is also important for long term simulations? Can you explain how you got the best fit without thermodynamics.
DISCUSSION BY V.R. NERALLA ON
"PRELIMINARY RESULTS OF ICE MODELLING IN THE EAST GREENLAND AREA"
Vol. 2 page 867

AUTHOR'S REPLY
By:
W.B. Tucker III, U.S. Army Cold Regions Research and Engineering Laboratory, USA

It is very true that the thermodynamic component is important in long term simulations. My point has been that the ice growth rates used in this 60-day simulation were excessive, probably due to the lack of inclusion of proper oceanic heat flux in the surface energy balance code. As a result, the ice extent is far in excess of the observed ice extent. A simulation in which no growth or ablation was allowed resulted in a relatively better fit to the observed ice extent (see figure below). It may be possible that dynamics were more important than thermodynamics in dictating ice extent during this particular time period, but it is also obvious from the figure that growth in the north and ablation in the south are necessary to improve this simulation. What needs further study then, is the thermodynamic portion of the model.

60-day thickness field from dynamics-only simulation
THE BIOLOGICALLY IMPORTANT AREAS
IN THE ARCTIC OCEAN

Erkki Palosuo
University of Helsinki
Finland

DISCUSSION BY G.R. PILKINGTON, Dome Pet. Calgary

1. Did you say that algae travel through the ice during the winter?

AUTHOR'S REPLY:
The only information available is the Meguro's paper. He presents the possibility that algae may travel through the ice during the winter.

2. We dived under the ice in May and found very poor visibility under open water leads due to algae, and good visibility under solid ice.

AUTHOR'S REPLY:
This is a new result. I agree your opinion that open water leads offer possibility for travelling of algae in spring.
"POOLING OF OIL UNDER SEA ICE" BY AUSTIN KOVACS, REXFORD M. MOREY, DONALD F. CUNDY AND GARY DECOPF

Vol. 2, page 912

DISCUSSION

By:

David F. Dickins, D.F. Dickins Assoc. Ltd., 3732 W. Broadway, Vancouver, B.C. V6R 2C1

You may be interested to know that divers on the Dome Oil and Gas Under Beaufort Sea Ice Experiment in 1980 identified similar underhanging frazil ice features to those inferred from your radar profiles. These growths were highly porous to spreading oil and easily shattered by either a diver's touch or the force of escaping gas from the blowout.

AUTHORS' REPLY

By:

A. Kovacs

The authors thank Mr. Dickins for his reference to sub-ice-sheet frazil-slush formations. The first author became aware of such formations in 1971 while investigating first- and multi-year pressure ridges and ice islands in Mackenzie Bay*. In this study we found frazil-slush ice accumulations under many ice features. These accumulations were frequently over 1 meter thick.

MOVEMENT OF OIL AND GAS SPILLS UNDER SEA ICE

Vol. II     Page 923

Discussion by:
David F. Dickins, DF Dickns Associates, Vancouver, Canada.

1. The Dome Petroleum Oil and Gas under Beaufort Sea Ice Study of 1979/80 showed that in the presence of gas flows representative of typical blowout flow rates, the oil exits at the sea bed as small oil droplets, which then rise and adhere to a smooth ice under surface as discrete drops decreasing in diameter as a function of distance from the blowout point. Except in the case of an irregular ice undersurface relief late in the growth season combined with much reduced gas flows, oil did not commonly exist in droplet sizes under the ice greater than 1 mm — much less than your limiting values indicated by laboratory studies.


2. It is interesting to note that the 1975 Balaena Bay Experiment (NORCOR) confirmed your observation that oil spreading under ice is not significantly different for oils of widely varying viscosities (in that case Norman Wells vs Swan Hills crude). Also, in the spring no discernible difference in vertical oil migration through the ice was observed between the two oil types.
Mr. Dickins comments on the experimental results of the Dome Petroleum field study are of considerable interest. It might be inferred from his description that the oil and gas seabed injection system used in the study produced sufficient energy to disperse the oil in the fine droplets described over a wide target area on the underside of the ice cover, and that the injected gas moved off target leaving the dispersed discrete oil droplets behind.

The theoretical analysis summarized by equations (1) and (2) is valid for both oil drops and gas bubbles when the contact angle is 180°. Equations (1) and (2) present values for the maximum height and the limiting height, but it should be pointed out that drops and bubbles with smaller heights, and correspondingly smaller diameter are entirely possible. The motivation for discussion of the upper bound limit cases arises from the knowledge that oil pool thickness controls the areal extent of contamination of a given spill volume and also affects the ability to burn off the oil after it migrates through the ice in the spring.

The absence of a gas bubble under the ice at the injection target area implied by Mr. Dickins comments was probably due to the local tilt of the ice surface to the horizontal which allowed the gas to move off target. Venting through cracks in the ice is another explanation.

The comments concerning the experimental finding that viscosity does not play a significant role in oil spreading under ice is also of interest. Our analysis shows the equilibrium shape of drops is independent of viscosity. However, the transient time required for an oil spill to achieve a steady state, and spread out to a static thickness and shape, is probably controlled in part by the oil viscosity. Presumably the time between oil spreading observations in the field is large with respect to the transient.
It is encouraging to have field experimental confirmation of theory and small scale laboratory experiments. Theory should be used to plan experiments and cohere the results. From a practical oilspill cleanup viewpoint, however, these studies are not encouraging. A widely dispersed spill in the form of tiny discrete droplets represents an insurmountable cleanup problem. The only hope remaining is that the dispersion is sufficiently wide spread that the resulting environmental damage is acceptably low. Under the controlled conditions of the study referred to by Mr. Dickins, manual cleanup and burning in situ accounted for only 49% of the oil spilled. Natural dispersion (21%) and evaporation (30%) account for the remainder.
"NEED FOR REAL WORLD ASSESSMENT OF THE ENVIRONMENTAL EFFECTS OF OIL SPILLS IN ICE-INFESTED MARINE ENVIRONMENTS" by G. A. Robilliard and M. Busdosh
B6-3

DISCUSSION
by:
M. J. Dunbar, McGill University

I agree entirely with the authors of this paper, in terms of fact and interpretation, and the need for experimentation, with the single exception of the point raised concerning the rate of the ecological process of damage and recovery (see point 3 in the "conclusions" paragraph).

The idea that the generally slow rates of processes (activity of oil degraders, growth, reproduction, etc., of Arctic animals and plants) when taken together, in some way cancel each other out, and that therefore the Arctic situation is not different in kind from the temperate and tropical, I find very interesting, and food for thought. But two points should be considered: (1) these slow processes do not apply to the homotherms in the system (birds and mammals), whose growth and metabolic rates are as fast as anywhere else (or faster), and which are the most vulnerable to oil spill damage; and (2) the question of the regulation of metabolic rate with respect to temperature, in polar poikilotherms, is still a controversial matter. The most that can be said about it from present available evidence is that in listing the species which do or do not show this regulation is "some do, some don't." See, for instance, Dunbar (in press, Arctic Ocean Symposium, March 1980, London -- Comite Arctique, Monaco), and Clarke, Biol. J. Linn. Soc., 14, 1980.
AUTHOR'S REPLY
BY:

G. A. Robilliard and M. Busdosh, Woodward-Clyde Consultants, San Francisco, California

We and Dr. Dunbar are, we believe, in essential agreement, and we all seek to satiate ourselves on the "food for thought." We present our hypothesis and supporting data concomitant of the need for further studies, especially on the rates of metabolism, reproduction, growth, etc., of polar poikilotherms. The work by Dayton, Robilliard, Dearborn, and others in the Antarctic identifies a number of "keystone" or "key industry" species where growth is very slow. However, they also identify other "key industry" species with rapid growth rates. Dr. Dunbar points out, and we agree, that of the several arctic species for which data are available, some do and some don't show a regulation of their metabolic rate with respect to temperature.

However, we believe it is reasonably well established that the breakdown of oil by physical, chemical, or biological means in polar waters is slower than it is in temperate or tropical waters. In other words, the oil, especially the toxic fractions, are likely to remain in the water column or in/on the bottom substrate for longer periods of time. Assuming this toxic material affects the organisms, the impact is likely to be greater on those that have a high metabolic, growth, and reproductive rate than it will on those with lower rates.

We agree that birds and mammals probably should not be included in the sense that they do have high metabolic rates. We also agree that birds are among the most vulnerable species in oil spill when they are present. However, it is important to recognize that most vulnerable birds (i.e., sea ducks and seabirds) are present only during the ice-free season (a few weeks to 3 months in most areas), while activities that could result in significant spills may soon be common place everywhere in the Arctic.
In general, there are few data to suggest that marine mammals are at much risk from an oil spill. It is certainly possible that a major spill in (1) polynyas or other confined open water areas in water or (2) confined areas extensively used by marine mammals in the ice-free season could cause significant mortality. However, we believe the probability is very low, though only some real-world documented experience is likely to provide a more definitive answer.
"NEED FOR REAL WORLD ASSESSMENT OF THE ENVIRONMENTAL EFFECTS OF OIL SPILLS IN ICE-INFESTED MARINE ENVIRONMENTS" by G. A. Robilliard and M. Busdosh
B6-3

DISCUSSION
by:
David F. Dickins, D. F. Dickins Associates, Ltd. 3732 W. Broadway, Vancouver, B. C.

You mentioned the lack of Arctic data relating to long term environmental effects of actual oil spills. In June 1981, a field team of biologists visited the site of the 1975 Balaena Bay crude oil spill. They collected beach and bottom sediment samples and benthic samples in areas affected by the oil and in control stations. In addition, a saltmarsh site in the vicinity oiled six years ago in conjunction with the main oil discharge, was checked for recovery. This new data will be available through Gulf Canada, Calgary by November 1981. (Contact D. Dickins for further details.)
DISCUSSION BY DAVID F. DICKINS ON
"NEED FOR REAL WORLD ASSESSMENT OF THE ENVIRONMENTAL
EFFECTS ON OIL SPILLS IN ICE-INFESTED MARINE ENVIRONMENTS"
86-3

AUTHOR'S REPLY
BY:
G. A. Robilliard and M. Busdosh, Woodward-Clyde Consultants, San Francisco,
California

We are pleased to see that Gulf Canada is taking an active role in monitoring
the long-term impacts of this spill. It is precisely this type of data that
we believe needs to be obtained, analyzed and synthesized if we are to document
the real world impacts of arctic oil spills.
I regret that there was insufficient time at the conclusion of the session that I chaired (C6: Interaction Between Ice and Shore) to provide even a brief summation of the papers that were presented. I think we saw and heard ample evidence in these fine papers for the importance of near-coast and shore processes relating to ice push and ice pile-up. Collectively, they are of great concern to any structures under consideration for near-shore and offshore hydrocarbon production; for example, pipelines leading onto the shore from seafloor or sub-bottom pipeline systems and manifold systems, docking facilities and the like. As offshore ice encroaches on land, through water current or wind forces, and the ice becomes grounded and is actually forced inland under some conditions, design engineers have different problems to contend with than those for offshore structures, for instance.

I thank these authors for their efforts in this regard.
The authors are to be complimented for their idea of determining the frequency of ice override with respect to space and time from the examination of low-level air photos of the northern coastline of Alaska. The effort expended in gathering the photographic data base was probably not trivial.

Air photo interpretation can provide an assessment of the frequency of ice ride-up, but one must heed the authors' comment regarding the underestimation of override frequency due to 1) non-detection of the events on the air photo and 2) reworking of ice rideup scars beyond recognition. For example, the data base did not pick up the interesting ice ride-up event which took place during the winter of 1973-74 at the abandoned Bullen Pt. Dew Line Station. This ride-up extended inland about 20 meters. The ice pressed against a steel building, caving in a portion of the side wall. Two views of the site are shown below. The conclusions reached in this paper...
on the frequency of ride-up should be evaluated against this and other ride-up omissions.

It would have been most informative if the authors had presented some photos showing the more severe ice ride-up events, particularly those in excess of 100 meters. Could the authors supply these photos in their reply? The authors should define "severe" override events. Perhaps we missed their definition. Without doubt these omissions were covered in reference 3, on which this paper is based, but this reference is proprietary and therefore not available to the general reader.
Discussion of "Analysis of Ice-Override Potential Along the Beaufort Sea Coast of Alaska"

W. M. Sackinger
Geophysical Institute
University of Alaska

The authors have taken a significant first step towards identifying regions subject to ice override. The inventory of ice override events can be quite useful. From air-photo interpretation alone, it is not possible to determine those site conditions conducive to ice override, yet this kind of information would be very useful in the design of the shoreline contours of man-made islands, for example. Are there any studies planned or in progress to look at site conditions at the override locations?

A randomly-chosen length of shoreline obviously has a low probability of ice override, as the authors have shown. A consequence is that the siting of shore facilities and pipeline landfalls should be possible in most regions. However, site-specific studies will still be required to make final designs, and areas with frequent override recurrences should be avoided.
REPLY TO DISCUSSION OF
ANALYSIS OF ICE-OVERRIDE POTENTIAL ALONG
THE BEAUFORT SEA COAST OF ALASKA
by
John R. Harper and E.H. Owens

Kovacs and Sodhi's comments on the Bullen Point override, which took place during the winter of 1973-74, provide a focus for further discussion on the methodology and reliability of our approach to the estimation of frequency of occurrence of ice override. The omission of the Bullen Point override from our analysis is useful to illustrate the limitations of the data base (i.e., the distribution of aerial photograph coverage in both time and space) and limitations associated with the interpretation of the data.

No aerial photographs of the Bullen Point shoreline were analyzed for the years 1974 to date, thus the omission of the Bullen Point override does not reflect an inadequacy of the technique, but rather the limitations of the data base. The distribution of air photo coverage is not uniform in time or space (Harper and Owens, 1981; Tables 1 and 2) and consequently not all sections of the coast could be surveyed with the same frequency. In general, the offshore barrier islands, which are exposed to greater ice forces, received greater emphasis in the analysis (Harper and Owens, 1981; Table 1), and, if anything, that emphasis may cause an overestimate of the true frequency of override occurrence along the entire coast.

An additional limitation of the technique includes the possibility that the Bullen Point override may have been unnoticed due to (a) scale limitations of the aerial photographs, or to (b) reworking of the ride-up scars by marine, aeolian or terrestrial processes, even if the appropriate aerial photographs had been available. Unfortunately, there is no appropriate ground-truth data available to verify the aerial photograph interpretations; however, we do consider that the technique would record most of the "severe" override events (see discussion below for a definition of a severe event). Qualitatively, our conclusion that ice override is a rare event is supported by three independent information sources. These include:

(1) personal observations of all of the North Slope shorelines and the Canadian Beaufort Sea shorelines over a number of seasons, along which only two override events have been directly observed by the authors. Our cumulative observations cover tens of thousands of kilometers of shoreline, suggesting an overall frequency in the same order as the estimates obtained from the aerial photograph analysis;
Observations by Leffingwell (1919), who lived for 5 years in the Flaxman Island region, and who noted that, whereas ice push along beaches was common, evidence of override events "was infrequently seen and in no place did such evidence extend more than 100 feet from the ocean." He also noted that ice blocks may override low banked shores, but this was "very exceptional." Significantly, he did not observe ice override on the barrier islands around Flaxman Island. Leffingwell's observations qualitatively support our conclusion that override is a rare phenomena along the Beaufort Sea coast;

Observations of local Inupiat elders from Barrow, Alaska (Shapiro and Metzner, 1979). Of eight elders interviewed, only one recalled witnessing an override event (near Cape Halkett), and none could recall an override event which completely overtopped the barrier islands. The elders' cumulative experience represents informal observations along thousands of kilometers of coastline; the fact that no direct observations of override were made by the elders also suggests that override is a rare event along this segment of coast.

Kovacs and Sodhi also raise a point concerning our definition of "severe" override. Severe override events are those which we consider to have potential to damage structures near the shore zone. The estimate on severity of an event in the study was based on an assessment of penetration distance, override width, ice thickness and maximum elevation attained by the override, where the severity index (see Tables 3 and 4, last column) increases as:

1. penetration distance increases
2. override width increases
3. ice thickness increases
4. override elevation increases

The severity index for any one event then represents an average of each of these components, and for purposes of discussion, an event is considered severe if the index is equal to or greater than 3.0.

The Bullen Point override has a computed severity index of 2.6, yet caused damage to shore structures, and suggests that perhaps our limit for severe events could be lowered to 2.5. Although the Bullen Point override did cause damage to shore structures, the structure itself was apparently sufficient to cause failure of the ride-up.
It is hoped that the results of this study can be updated as additional data become available. Low-level aerial videotape recording (VTR) surveys (most of which are proprietary) of the Alaskan and Canadian Beaufort Sea coasts, were flown during the summer of 1981 and should provide an excellent information source for updating the data base. Observations during these overflights support our original analysis of the Beaufort Sea coast of Alaska. During VTR surveys of the Amundsen Gulf in 1981 and the Northwest Passage during 1980, however, localized areas of intensive override reworking of the backshore were noted. In one case, ice was noted on the top of a 5-m cliff on Ramsay Island at the southern end of Prince of Wales Strait. The preliminary results of these surveys suggest that the frequency of ice-override occurrence varies on both a regional and localized basis, and that similar surveys of this type will be required in other areas as part of the site selection process for coastal-zone structures.

REFERENCES


DISCUSSION
By:

G.R. Pilkington, Dome Petroleum, Calgary, Alberta, Canada

1. Did the ice foot go vertically downward from water level to seabed at that location?

2. During my talk on Hans Island (C4-1) someone suggested that we put an accelerometer on the island to measure its response to ice loads. It would seem to me that Fairway Rock is a better candidate. Have you by any chance considered the feasibility of this? Have you considered using ice floe impacts to calibrate such a system?

AUTHORS' REPLY
By:

A. Kovacs and D.S. Sodhi

1. One of our field objectives in April 1981 was to access the subsurface relief of the icefoot and the submarine topography of Fairway Rock. This work was to be undertaken off the U.S. Coast Guard Icebreaker Polar Sea. Since this ship became
disabled and was adrift in the Chukchi Sea ice, and therefore was unavailable to us, our field program at Fairway Rock was reduced to a short inspection using a helicopter. The observations of Gordon Cox and the first author revealed the icefoot face to be near vertical on the seaward side (see photographs). Past studies of first-year ridges which have lost mass due to slumping or calving revealed that the keels of these ridges are quite steep. We believe that the keel of the icefoot at Fairway Rock, having also lost mass to slumping and calving, should also be quite steep, and that the keel rests upon the submarine rock talus. Given the distance the icefoot extends from the rock, and making an estimate of the angle of repose of the submarine talus, we would not anticipate that the keel of the existing ice foot contacts the seabed.

2. Yes, we have considered installing an inclinometer, accelerometer, magnetic field sensors (as the rock moves, variations in the magnetic field occur), and other devices on Fairway Rock to measure the island’s response to ice impact forces. To calibrate these devices we considered such methods as explosives, cable loading by ship (where a load shackle fails at a known tension) and the use of ice floes as you have at Hans Island. The last method was ruled out because a) one cannot be certain which floe, north or south of the island, would impact the rock, and b) when major ice movements do occur it is generally during severe wind conditions. At these times transportation to the island is out of the question, and to try to man ranging equipment as you have at Hans Island would not be possible.

As related to the use of accelerometers and inclinometers, we have made rough calculations which indicate that the deformations and accelerations due to ice impact forces will be very small, and for all intents and purposes not measurable. The most sensitive accelerometers and tilt meters currently under research evaluation would not appear suitable, as these devices are currently sensitive to temperature fluctuations and drift with time.

Instrumentation to measure acoustic activity due to ice impact and failure is feasible. However, it will be most difficult to calibrate such a system.
DISCUSSION
By:
P.R. Kry, Research Dept., ESSO Resources, Canada, Limited, Calgary, Alberta T2G 2B3

Table II of the authors' paper presents the spacing $\lambda$ of the points where buckling occurs as an independent parameter. The given stress presented in the table thus decreases significantly as $\lambda$ increases because it assumes there is more area over which the driving stress is applied ($\lambda r_0$), whereas the failure load is a constant, being restricted to single areas separated by increasing distance.

This is questionable on the grounds that the spacing $\lambda$ is, in general, determined by the failure process. For buckling failures the cusp, giving rise to individual subsequent failures, will have a spacing of only a few action radii ($\lambda < 10$). $\lambda$ values higher than this may not be physically reasonable.

AUTHORS' REPLY
By:
A. Kovacs and D.S. Sodhi

The authors agree that the spacing of contact areas is determined by the size of cusps or the area of the ice sheet affected by buckling failure. The spacing parameter was arbitrarily assumed to demonstrate that low environmental forces can cause buckling failure in an ice sheet. It should also be pointed out that the above situation refers to the case when all buckling failures occur simultaneously. If non-simultaneous failure zones are assumed to occur, the values given in Table II may not be so unreasonable.
SESSION B7

ICE CONDITIONS

GENERAL DISCUSSION BY:

S.S. Lazier, Queen's University, Kingston, Ontario, Canada

INTRODUCTION

If we know the ice conditions which may be encountered in the field it may be possible to cope with them or at least avoid unsuitable conditions.

This session deals with a wide variety of ice conditions for sea ice growth to effect of electric correct on the mechanical properties of ice.

CLOSURE

We have learned today of the relationship between ice growth and the relevant climatological factors which affect. As well we have been exposed to the use of laser to determine sea ice conditions and to plat changes in them over a season. There are macro scale field events.

At the micro scale we have discussed a numerical model which predicts the accretion of ice against structures which seems to hold some promise for use in the field. Also we have learned of the relation between an electric field and the properties of ice, which indeed may be useful technique to generate ice with specified physical characteristics for models and other experiments.
"A SENSITIVITY ANALYSIS OF A SIMPLE MODEL OF SEASONAL SEA ICE GROWTH"

DISCUSSION by:
W.B. Tucker III, U.S. Army Cold Regions Research and Engineering Laboratories - U.S.A.

Was the ocean temperature always specified to be the freezing point of seawater in the model?

REPLY by:
J.D. Miller, Petro-Canada Exploration Inc. - Calgary, Canada

Yes, the water beneath the ice was assumed to be both isohaline and isothermal. The temperature used was the salinity dependant freezing point temperature.

DISCUSSION by:
E.B. Bennett, Dept. of Fisheries and Oceans - Burlington, Canada

Was the snow density value of 310 kg/m$^3$ chosen to give the best fit of the model to the data?

REPLY by:
J.D. Miller, Petro-Canada Exploration Inc. - Calgary, Canada

No, it was selected in advance on the basis of Bilello's work on snow density.
This paper is an excellent illustration of the value of laser profiling of the sea ice surface. Leppäranta has confirmed for thin first-year ice in the Baltic the validity of the negative exponential distribution for sail height and for sail spacing (demonstrated hitherto for the Arctic Ocean), and has derived a method for estimating mean ice thickness from the laser-derived ridging intensity. The technique of operation from an icebreaker promises an operational application of the laser profilometer as a tool in assessing ice conditions during icebreaker operation. It is clear that the system works well only if the ship can maintain a steady speed in the ice, but it may be possible to design a concurrent photography or videotaping system that would enable the spacing of ridges to be measured despite variations in ship speed.

Leppäranta states that the ridge frequency and mean sail height are unrelated, contrasting this with findings for the Arctic Ocean. The 1976 co-operative submarine/laser experiment in the Arctic Ocean (Wadhams, 1981) confirmed a strong positive correlation between sail frequency and mean sail height for Eurasian Basin ice. It is interesting that Baltic Sea ice confirms the negative exponential distribution for sail heights yet does not have a mean sail height that is related to sail frequency. In the Baltic Sea the undeformed ice is all first-year, and of similar modest thickness. There is a chal-
lenge to theoreticians here to explain how the mechanics of the
ridge-building process lead to such results.

Wadhams, P. (1981). Sea-ice topography of the Arctic Ocean in the

AUTHORS’ REPLY:
We would like to thank Peter Wadhams for his discussion of the
paper and for his suggestion for a method to remove the measurement
errors in ridge spacings due to variations in ship speed.

We feel that in his comments about the independence of the ridge
frquency and mean sail height Peter Wadhams pays attention on a
key thing in noting that the undeformed ice in the Baltic Sea is
all first-year ice and of similar modest thickness. According to
our experince, although the mean ice thickness grows to 50 to 80 cm
in the course of the winter, there is generally present a lot of
very thin ice, 10 to 30 cm, which is used in building up new ridges.
We are very interested to see whether our future data will support
the observed independence and does that exist through the whole
season

DISCUSSION BY S.S. LAZIER, Queen's University, Kingston, Ont.

1. Does a laser operate satisfactorily when exposed to cold
weather?

AUTHORS’ REPLY:
The manufactorer guarantees the laser to operate at temperatures
above 0 °C. We have, however, used the instrument at temperatures
down to -20 °C without any difficulties. In cold weather an insula­
ting cover is placed on the laser and it is not kept outside for
more than one hour at a time.

2. Does blowing snow affect its resolution?

AUTHORS’ REPLY:
The laser beam reflects back from snowflakes blowing in the air
and consequently the results are not reliable in such conditions.
In light and moderate snowfall we have had no difficulties.
DISCUSSION

By:

E.P. Lozowski, University of Alberta, Canada

As co-developer of a previous ice accretion model [Lozowski et al., 1979-McComber Ref. 2], I would compliment the author on his novel technique for taking into account the feedback between the growing ice accretion and the droplet trajectories. It would be very useful if the thermodynamic aspects of Lozowski et al., could be combined with the feedback aspects of McComber's model, to yield a time-dependent model capable of predicting both dry and wet ice accretions.

By way of criticism, it should be pointed out that McComber's approach is not the only one to the problem of shape-trajectory feedback in dry growth. Both Lozowski and Oleskiw [1] and Bragg and Gregorek [2], have outlined a different method in which the droplet trajectories are derived from a Lagragian point of view. A comparison between this approach and that of McComber should be made. It may be that one or the other is either more accurate, more efficient, or possibly both.

It is unfortunate that space limitations prevented McComber from providing more details of his model. Figure 2, suggests that the spatial resolution of the accretion shape is rather coarse. Consequently, I agree with his suggestion that a better shape interpolation would improve the model. However, what is the effect of this coarse spatial resolution on the droplet trajectories and the collection efficiencies? Unhappily, some of the symbols used in the paper are undefined. Consequently, I found Equations (7) and (9) rather confusing. Another source of confusion is the statement on page 6 that the model used a liquid water content of 2.5 gm\(^{-3}\), followed by a statement on page 7 which contradicts this. McComber suggests that the model liquid water content was 'adjusted' to fit the model predictions to the initial observed accretion rate. This does not entirely invalidate the model-experiment com-
parison made in figure 3. However, it begs the question as to why this needs to be done in order to obtain a good fit. What are the problems with the experimental determination of liquid water content and drop size distribution?


AUTHOR'S REPLY

By:

P. McComber, Université du Québec à Chicoutimi, Canada

The author would like to thank Dr. Lozowski for his discussion of the paper. As he points out in his discussion there are other models using a feedback approach between the shape of the obstacle and the droplet velocities (Lozowski, Ref. 1 and 2). These models, however, were not considering an arbitrary shape but rather approximating the accretion shape by either an ellipse [McComber, Ref. 3] or an airfoil [Lozowski, Ref. 1]. A comparison of the numerical results of the two approaches is difficult since each method is at the moment fairly complex to implement. The inertia parameter $K$ is used as a loading parameter in the non-linear solution presented in the paper. This represents a significant difference in that new iterations are not required for the solution to be applied to a droplet diameter spectrum instead of constant diameter droplets.

Results using a finer mesh are presently being obtained. The increased accuracy may influence the collection efficiency only insofar as it affects the accretion shape significantly.

The extension of the present model to wet growth is presently being considered. A local heat balance on the surface of the accretion must be considered as well as the effect of gravity on the liquid water film. This makes the model fairly complex since the effect of the wind on the liquid film in the form of water runbacks is also important. In view of this, a numerical model for dry growth only was developed and tested first. Wet growth will eventually be combined at a latter stage of the project.
Concerning the liquid water content \( w \), the results shown on Fig. 2 did use \( w = 2.5 \text{ gm}^{-3} \) as stated on page 6; the comparison, however, between numerical and experimental results was based on an experimental value of \( \frac{wV}{\rho_1} \), in Equation 8, as obtained from the accretion on the 3.49 cm cylinder. The liquid water content measurement is presently obtained through the measurement of an accretion thickness on a small rotating cylinder [McComber, Ref. 8]. It appears more accurate to use an initial accretion thickness on the cylinder itself as a basis for the liquid water content measurement. It should be mentioned that the accuracy of liquid water content measurements is still under investigation especially in the lower velocity range (for lower \( K \)).
DISCUSSION
By:

R. Stallabrass, National Research Council, Ottawa, Canada

The procedure outlined in this paper of determining the droplet velocity field at the finite element grid points instead of tracing individual droplet trajectories is an interesting and useful innovation and the author is to be congratulated.

The necessary brevity of the paper results in some ambiguities and lack of clarity. For instance, the variables \( r_e \) and \( r_i \) in equation (9) have not been defined, making it difficult to understand what the author intends. Is it implied that the contour of the ice growth face is assumed to be cylindrical? If so, this model suffers the same disadvantage as that of Ackley and Templeton [McComber, Ref. 3], in that the ice shape is arbitrarily assigned an assumed profile -- elliptical in Ackley's case, cylindrical here. The reason perhaps that this assumption is necessary is presumably the very coarse angular increment used (i.e. \( \Delta \theta = 11.25^\circ \)) which does not permit an accurate definition of the ice shape.

Inspection of Figs. 2 and 3 suggests that the computed ice growth increments at the various angular increments have been applied in a radial direction rather than in the direction of droplet impingement (i.e. generally forward growth, but with some inward component). Correcting the growth direction would rectify the error, evident in the two figures, of the ice accretion having a greater width than the cylinder diameter. It would also result in an ice profile that becomes progressively narrower and more pointed with forward growth. A great improvement in predicted shape results even if forward growth instead of radial growth is assumed, as is demonstrated by comparing the predictions for dry growth illustrated in [1] below with those shown in [2] of the paper.
To account for the shoulders evident on the experimental accretions (Fig. 3) and which are the result of a feathery type of rime of low density on the flanks of the accretion, it would be necessary to take the variation of density of the accreting ice at various points into account. Macklin [2] has shown that, for a rotating cylinder, the ice density is a function of \( r \, v_0/\theta_s \), where \( r \) is the droplet radius, \( v_0 \) its impact velocity and \( \theta_s \) the ice surface temperature. It would seem that for a non-rotating iced cylinder, the angle of droplet impact with the surface must also be introduced into the dependence, since it is on the flanks of the accretion where the impact angle is small that this low density rime occurs.

It would be interesting to compare the results of this finite element approach with those of the more conventional approach of tracing droplet trajectories such as described in [3] below.


The author would like to thank Dr. Stallabrass for his discussion of the paper. For the calculation of the ice growth in the numerical model, no assumption is made for the accretion shape, the change is obtained at each time increment by calculation of a radius increment \( r_e - r_i \) on equally spaced radii, so as to modify the locations of the surface nodes. It is felt that this approach does not impose a direction of ice growth insofar as it may be seen as using polar instead of cartesian coordinates. As the finite element mesh is made finer, and the time increment reduced the accuracy can be improved if necessary.

It is true that the local variation of density was not considered in the model. Density varies as a function of the surface temperature \([\text{Stallabrass, Ref. 2}]\) and the surface temperature depends in turn on the heat balance at the surface, which then requires the calculation of the convection heat transfer coefficient. Accuracy in the calculation of the surface temperature is therefore difficult to obtain in which case an average value of density gives comparable results in the case of dry growth. Finally, as far as comparing the present numerical model with those using the trajectories approach, this has been discussed in the author's reply to Dr. Lozowski's discussion.
A LABORATORY STUDY OF HEAT TRANSFER TO
AN ICE COVER FROM A WARM WATER DISCHARGE
by: I.K. Hill; A.B. Cammaert and D.R. Miller
Vol. II p. 1094

DISCUSSION BY:
K. Häggkvist, WREL, University of Luleå, Sweden

When designing ice reduction installations in arctic harbours, it is important to have access to a correct description of the heat transfer process between the water and the ice undersurface. This paper is interesting and important from this point of view.

The subject of the paper is to investigate if a commonly used empirical expression correctly describes the heat transfer, when both forced and free convection simultaneously are important. The empirical expression is based on heat transfer experiments between water and a plane plate. A relation best fitted for forced convection heat transfer. The mixed type of convection mentioned above is so far poorly treated in the literature. It is therefore interesting when the results of this investigation indicate that the heat transfer coefficient should be about twice the "empirical" coefficient, the latter based on the plane plate experiments (see Figure 3 in the paper).

Careful use of the result curve, the "Test-data" curve in Figure 3, is however recommended. The curve is based on a limited number of experimental runs, with different water temperatures (i.e. different Gr-numbers), which have been treated as a single data set without considering the dependence of the Gr-number. The scatter of the individual measurements around the "test-data" curve is therefore considerable. The Nusselt number for instance lies, for Re = 10^5, in the interval 900 ≤ Nu ≤ 2000.
An interpretation of the test data with a functional relationship of the type:

\[ \text{Nu} = f(\text{Re}, \text{Gr}); \text{Pr} \approx \text{konst.} \]

would have been interesting.

The results in this work do accentuate the importance of further research in order to obtain correct heat transfer coefficients in flow situations where both forced and free convection processes are important.
The authors suggest that low explosives or propellants may be more efficient than high explosives. We did some tests (Mellor and Kovacs, 1972) to compare compressed gas blasting devices (Airdox and Cardox shells) with dynamite, and found that crater radius was about the same for the gas shell and for an explosive charge with approximately the same amount of energy. However, the breakage processes were very different, with the gas devices producing large crater fragments in a neat dartboard pattern. At a different scaled thickness, the efficiencies of the two energy sources might have been significantly different, with the compressed gas doing better in "thin" ice. In one of the dynamite shots, photographs indicated that the ice in the crater was actually sucked down after the shot vented, perhaps because the gas bubble was collapsing. Incidentally, this report gives a few data sets for dynamite that have not been used yet in regression analyses.

Maybe the next step in this area of research should be tests under very thin ice, with the investigators hanging from skyhooks, followed by the placement of giant bombs under thick polar ice shelves.

References
Mellor, M. and A. Kovacs (1972) Breakage of floating ice by compressed gas blasting, Special Report 184, CRREL.
The Explosive Demolition of Floating Ice Sheets

by: G. D. Fonstad
R. Gerard
B. Stimpson

Discussion by: G. Frankenstein, U.S. Army Cold Regions Research and Engineering Laboratory, Hanover, NH

The authors have conducted an excellent study, both theoretically and experimentally on the use of explosives for removing ice. Their work will be very useful for a planned operation where everything can be measured and/or determined very accurately. However, their results will be difficult to use in ice jam situations where local contractors usually conduct the blasting operation. The U.S. Army Corps of Engineers use a curve developed by Frankenstein that is very easy to use. It gives the charge weight required for a desired crater hole diameter. The curve when converted is almost identical to the authors equation 9.

Had the authors presented their field results in tabled form, it would have been useful. This would give other engineers the opportunity to readily make use of the data.

The author's last sentence should be emphasized as a maxim for all blasting operations.
"THE EXPLOSIVE DEMOLITION OF FLOATING ICE SHEETS"
by G.D. Fonstad, R. Gerard and B. Stimpson

Authors' reply to discussions by M. Mellor and G. Frankenstein

The authors would like to thank both Dr. Mellor and Mr. Frankenstein for their thoughtful discussions of our paper. They are much appreciated.

Dr. Mellor's comment about the possible 'hidden' effects of various combinations of charge size and ice thickness is well taken. It would appear that these effects are related to the two features of the ice sheet that influence crater size - its inertia and its 'strength'. The traditional list of parameters used in the dimensional analysis of the situation - and those used in this preliminary paper - is based on the anticipated dominance of inertia and shock wave effects. To include a possible effect of the ice sheet 'strength', material properties such as the material stiffness $E_t$, Poisson's ratio $\nu$ and some failure stress $\sigma$ would have to be included in the list of parameters. The influence of $E_t$ and $\nu$ are probably adequately represented by the characteristic length of the ice sheet $\lambda$, so that only $\lambda$ and $\sigma$ need be considered. However, $\sigma$ is more or less constant, and $\lambda = f(t_i)$, hence these effects should be covered by the parameters considered in the paper.

Perhaps a more fundamental concern is that use of the shock wave celerity $c$ implies that the shock wave is important in the creation of the crater, or represents a variable that is important. However there is some doubt about this. For instance, Barash [7] noted that as the shock wave pulse reached the ice sheet it caused the ice to initially rise in the shape of a dome. Yet Kurtz, et. al. [2] noting a similar phenomenon with the ice dome rising "from one to three feet", ascribed the motion to the expansion of the gas bubble. Their analysis of the high speed photography taken during their tests did not yield any indication of spalling of the ice surface by a shock wave, and for certain shots indicated that the ice sheet 'rebounded downward under tension'. The crater, therefore, is probably more a function of the large scale displacement of the fluid by the bubble expansion.
If the celerity \( c \) is not important then another parameter must be found to provide a time scale, and this should presumably be related to the rate of bubble expansion. In addition to the energy released by the explosion the latter should be a function of the ambient pressure, which in turn depends on the depth of placement and gravity. Hence the corresponding time scale parameter would seem to be gravity \( g \). It is interesting to note that if \( g \) is used instead of \( c \) in the dimensional analysis, then a 'fourth root' scaling of the charge weight is required rather than the more familiar 'cube root' scaling used in the literature and our paper. This different scaling should make a difference to the regression, but we have yet to determine whether it will help to remove the bias.

Dr. Mellor's second comment on 'energy equivalence' is pertinent. In fact we did conduct such an analysis [9], after writing the paper, based on the energy equivalence of the various explosives reported in the ice demolition literature, including, incidentally, those data sets for dynamite mentioned by Dr. Mellor in his discussion. The parameter we used to determine energy-equivalent amounts of the various explosives was the 'effectiveness as an external charge' used by the U.S. Army Corps of Engineers [4], thereby converting charge weights to equivalent weights of a common explosive - TNT. We chose the 'effectiveness as an external charge' as we found that it approximated the ratio of the specific energy of a given explosive to the specific energy of TNT. We found that the energy equivalence approach did make a difference to our findings, the results of which we hope to publish in the future.

We were remiss in not referring in our paper to the simple relationship developed by Frankenstein and Smith [10] that is referred to in Mr. Frankenstein's discussion. This expression seems also to be based on tests on solid ice sheets, notably those of 'Operation Breakup' [2], with limited data from Mr. Frankenstein's work in releasing an ice jam on the Mississippi River near Elk River, Minnesota. (It is not clear from the reference whether, in this latter investigation, charges were placed beneath the ice jam, or beneath the thermally weakened ice downstream of the jam). The relation should presumably be used in conjunction with the depth of placement equation given in [10].

As indicated by Mr. Frankenstein the solution of our equations 7 and 9 for crater radius \( R = f(W^{1/3}) \) will yield a single curve similar to his, but with a significantly smaller slope. The use of such a curve, however, would require that both the charge weight and placement depth be already optimal for a given ice thickness. The implication in using the curve mentioned by Mr. Frankenstein is that the depth of placement is optimal for a given charge weight, but that the ice thickness does not influence the efficiency of the charges. As is evident from Fig. 1
of the paper, over the usual range of scaled ice thickness, the ice thickness has a considerable influence on the efficiency of the charge, at least for solid ice. To allow for the effect of ice thickness, equations 7-9 could be plotted on one graph, with the ice thickness along the abscissa and charge weight, placement depth and expected crater radius along the ordinate. Such a plot would be simple for a blasting contractor to use. However it should be noted that the use of equations 7-9 can lead to some recommendations that, while they are the most efficient from a quantity of explosive point of view, are downright inefficient in man hours. This brings up another interesting aspect of the problem of ice demolition which we hope to pursue.

Notwithstanding the above, with regard to ice jam demolition, the question remains whether a thickness of fragmented ice is equivalent to an equal thickness of solid ice. If the phenomenon is dominated by inertial effects this may well be so, within reasonable limits.

Finally, the limitations on paper length imposed for the conference precluded inclusion of the data obtained in the Chilcotin tests. At the prompting of Mr. Frankenstein, it is given in the following Table 1.

Additional References


### TABLE 1

THE CHILCOTIN DATA

<table>
<thead>
<tr>
<th>Shot No.</th>
<th>Placement Hole Diameter (mm)</th>
<th>Charge Weight (kg)</th>
<th>Ice Thickness (m)</th>
<th>Placement Depth (m)</th>
<th>Water Depth (m)</th>
<th>Crater Radius (m)</th>
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<td>-0.29</td>
<td>-</td>
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Unless otherwise specified, the explosive used was DM-12, a German PETN-based explosive with oil and grease plasticiser.

All placement depths and depths of water are measured from the ice/water interface.

NOTES:

1. In shallow (3") hole at ice surface, tamped with snow.

2. Ice bordering crater bent up, crater radius large, suspect measurement error.

3. Explosive for this series was CIL 40% Forcite.

4. Explosive for this series was CIL Amex II.

5. Explosive for this series was CIL Hydromex.

6. Crater radius small, suspected partial detonation of explosive due to water seepage into the charges.
DISCUSSION
By:

M. Mellor, U. S. Army Cold Regions Research and Engineering Laboratory, U.S.A.

The paper describes a major series of tests by a group which has an outstanding reputation for equipment development and jet cutting experiments covering a wide range of applications. The ice test data, which presumably are given in the NRC reports cited by the author, should be of great value in further evaluating the potential of water jets.

The paper itself does not give actual test data. Instead, it gives the result of a regression analysis which shows jet penetration depth to be a function of jet power, ice temperature, nozzle standoff, and nozzle traverse speed. The final empirical equation (5) is a little troubling, in that it contains no explicit dependence on nozzle diameter, while the boundary conditions for power, traverse speed and standoff seem unrealistic.

An important thing to remember is that a liquid jet issuing from a nozzle into air is subject to resistances and instabilities, so that it has a finite length which is a function of nozzle diameter. Cutting jets typically have a total length in air of about 3000 nozzle diameters, and an effective length of about 2000 nozzle diameters. The depth of penetration into target material, plus the standoff distance, cannot possibly exceed the jet length. The width of the slot cut by a traversing jet, which is a factor in assessment of specific energy, is also a function of the nozzle diameter (about 2.5 times the nozzle diameter for high speed cutting).

Eq.(5) says that penetration can increase indefinitely in direct proportion to the power. It also says that there will always be some penetration, no matter how great the standoff distance. Furthermore, it says that zero standoff will give infinite penetration. The equation also implies that penetration tends to infinity as traverse speed drops towards zero, and that penetration will never be less than 7 mm, even if the traverse speed tends to infinity. None of these things can
really be true.

So far, jet cutting tests on ice have not been particularly encouraging, but sooner or later there will be some problem in ice engineering that can be tackled to advantage with water jets. The results of earlier studies showed that the specific energy for deep jet cutting in ice was comparable to, or greater than, the specific energy for melting. This leads to the suspicion that jets, like lasers, penetrate ice by a process that is energetically equivalent to melting. It would be interesting to know whether the new results bear out this hunch, since the total energy content of a water jet could be increased significantly (and cheaply) by heating the water.
AUTHOR'S REPLY

By:

D. B. Coveney, Division of Mechanical Engineering, National Research Council, Canada.

I would like to thank Dr. Mellor for his comments. Owing to space limitations in the paper, the test data could not be included. However, a more comprehensive report with the complete test data has now been published.

It must be realized that equation (5) is a good least squares fit to experimental data and not a scientifically exact representation of the phenomena involved in cutting ice. As such, it is limited in direct applicability to the test conditions from which it was derived. While it does yield unrealistic results at or near the boundary conditions of zero and infinite power, traverse speed and standoff, these boundary conditions are themselves beyond the limits of practical reality. Equation (5) does provide a simple highly significant relationship relating the depth of cut to the jet parameters over a portion of the range of practical interest in cutting slots in ice. While the effect on the depth of cut of each parameter of equation (5) may not be scientifically exact, it is a close approximation over the range of test conditions. In addition, since the test results to date have not shown a tendency for the depth of cut to level off at the higher power levels, the relationships of equation (5) may remain a close enough approximation within reasonable, practical limits to permit a rough estimation of the capabilities of a practical larger size system.

For practical purposes,
a) the lowest pressure that will cut adequately is most desirable - high pressure equipment is expensive. In our tests 40 MPa was adequate to cut a kerf while spalled trenches could be produced with pressures as low as 10 MPa;
b) the power to be applied to the jet is dependent on economics and the availability of equipment;
c) the nozzle diameter should be the largest that is compatible with the pressure
selected and the power available;
d) the nozzle standoff should be as large as possible to prevent nozzle contact with the ice due to nozzle motion and/or ice irregularities, while at the same time remaining within the range of efficient jet cutting. For efficiency in cutting, 50 nozzle diameters standoff has been found desirable, while beyond about 100 nozzle diameters standoff jet degradation became excessive;
e) in cutting slots zero traverse speed is not possible and exceedingly slow traverse speeds are of no practical interest. Also jet cutting at speeds above a few km/h would produce too shallow a slot to be useful.

Within the accuracy of the regression, the 0.7 cm constant of equation (5) is not significantly different from zero. However, the constant has been retained in equation (5) to permit calculation of the mean depth of cut.

The kerf width when measured ranged from about 2 to 4 nozzle diameters with an average of 3 nozzle diameters. Assuming for all tests a kerf width of 3 nozzle diameters (even for those tests that spalled a much wider trench) the specific energy to cut ice was generally high, ranging from less than 10% to more than 1300% of the specific energy required to melt ice from and at 0°C. However, most of the tests with specific energy above the melting energy were at traverse speeds below 1 km/hr. At the higher, more practical traverse speeds the specific energy dropped to about 50% of the melting energy, and in some cases even lower. There was a general tendency for the specific energy to vary inversely with the square root of traverse speed.

While the specific energy for cutting ice with water jets is high, water jet cutting does provide a means which could be developed into a practical non-contact system for reducing the overall power requirements of ice breaking.

The addition of heat to the jet as suggested by Dr. Mellor could enhance the cutting ability of a water jet if significant rates of heat transfer could be achieved. From a practical stand-point such heat in the water would reduce the chances of system freeze-up if the piping system were insulated, and this could provide a good use for waste heat.
CONFERENCE SUMMARY

P. BRUUN, Secretary General, POAC, Norway

During the recent 6th International Conference on Port and Ocean Engineering under Arctic Conditions held in Québec, Canada, attended by almost 400 professionnels about 125 papers were presented. Authors from Canada, Japan, Scandinavia and the United States delivered a wealth of new results of tracking and mapping of ice, ice mechanics, ice forces and forces by ice on structures, including breakwaters, platforms for ice conditions, fill islands, the sea bottom and shores. Results of field as well as model research, whether physical or mathematical modelling, were presented.

Considerable progress has been achieved on forecasting and tracking of ice as mentioned in several papers including papers by Leavitt et al., (Canada) on "A Sea Ice Model Developed for Use in a Real Time Forecast System" on the prediction of ice velocities, areas of convergence and ice deformation and by Lowry et al. (Canada) who developed the techniques for extracting ice classification and ice motion data from radar imagery in a form suitable for input to an ice dynamics model. Agerton's (USA) paper on "Large Winter Ice Movements in the Nearshore Alaskan Beaufort Sea" describes observations of a mid-winter storm in the Alaskan Beaufort Sea and the associated ice movements and deformations, which are compared with simple mathematical force models. It reviews Landsat Imagery and Weather Records from previous years for indications of similar ice movements. Satellite imagery was also used by ARCO (USA) to document historical ice breakup and ice freeze up periods (Wolfson and Evans). This was done for Norton Sound, Alaska, which is covered with ice for about six months. A model of the Sound under ice cover is described by Liu and Leendertse (USA) who illustrate the effect of tide, depth, boundary and ice/ice interaction on ice drift. Tucker and Hibler (USA) describe a sea ice model for East Greenland area, which employs a viscous-plastic
constitutive law. The model provides reasonable thicknesses and velocities over a 60-day study period including thermodynamic, ice interactions and stresses. Zorn and Valeur (DHI, Denmark) mention "Pack Ice Drift and Weather Impact", an environmental research project off East Greenland using drift buoys, reconnaissance flights and satellite images for comparison with weather maps and climate current data. It is shown, how ice drift may serve to verify historical weather charts, thus providing a tool to improve weather analysis. Another ice transport model developed for application to the Great Lakes to aid in forecasting ice condition is described by Rumer et al. (USA).

Ice growth is dealt with by Kovacs et al. (USA) in paper on "Pooling of Oil Under Sea Ice". Ice thickness profiles were constructed for six fast ice locations in the vicinity of Prudhoe Bay in Alaska using a radar echo sounding system, which revealed in detail the undulating relief of the bottom of the sea ice, in which oil could pool up, if released under the ice. Storage capacities for such pools are given for a variety of ice conditions, 10 000 m$^3$ to 60 000 m$^3$ per pool. Malcolm and Cammaert (Canada) in paper on "Movement of Oil and Gas Spills Under Sea Ice" mention that in the event of a well blow-out under Arctic Ice, there is evidence that the blow-out products will be spread out under the ice rather than fracture it. At a greater distance from the blow-out, the gas bubble will be fragmented and oil and gas will become trapped in the ice cover undulations. Paper by Reimer et al. (USA) mentions ice motion in the Chukchi Sea in response to ocean currents to determine whether oiled ice could be transported to Alaskan Coasts.

Another environmental concern is the influence of heated water discharged from coastal power stations. This topic is dealt with by Horikawa and Mimura (Japan). Most of the steam nuclear power stations in Japan are located on the coast and discharge huge amounts of heated water in the sea. The observed data indicate that the pattern of temperature distribution is strongly influenced by long period flow oscillation with a period of two to three days mainly excited by a meteorological front migrating from the Asian Continent to the Pacific Ocean. A laboratory study on heat transfer to an ice cover from a warm water discharge proposed to limit ice build up around an Arctic liquid gas terminal is mentioned by Hill, Cammaert and Miller (Canada). Combination of a sinking warm water discharge and air bubble curtains for ice reduction is dealt with by Häggkvist (Sweden). A laboratory experiment and theoretical analysis are presented and example on application in Luleå Harbour, Sweden, Baltic, is given.
Prof. Palosuo from Finland gives a general review of the biological productivity and its significance to the Arctic Basin ecosystem using as examples the shelf between Spitzbergen and Franz Joseph Land, the Mackenzie Estuary and the Stefansson Sound.

Ice ridges, ice pile ups and ice over-ride present severe problems for arctic industrial activities.

Ice ridges are mentioned in several papers on forces by ice. Leppäranta and Palosuo (Finland) in paper on "Studies of Sea Ice Ridging with a Ship-Borne Laser Profilometer" study the distributions of the height and spacing of ridge sails. The Profilometer was used aboard the icebreaker YMER in July 1980 in the area between Svalbard (Spitzbergen) and Franz Joseph Land.

Sisodiya and Vandry (USA) studied first-year ice features in the Alaskan Beaufort Sea during March-April 1979 to determine ice feature geometry and internal characteristics as well as assess winter ice conditions in the lease sale area offshore Prudhoe Bay, including sail and keel profiling of ridges. The ridge project by Dickins Engineering and Suncor Inc., Canada, described by Dickins and Wetzel had as its primary objective the obtaining of fundamental data on multi-year pressure ridges in the Queen Elisabeth Islands off the Canadian Arctic. The mean sail/keel ratio found was 1 to 5.6 ± 2.2. The mean total ice thickness of the ridges was 25.3 m with a maximum thickness of 46 m.

Kreider and Thro (USA) describe "Statistical Techniques for the Analysis of Sea Ice Pressure Ridge Distributions" using stereo-aerial photography from the Beaufort Sea. A first-order negative exponential distribution was found to fit the sail height data.

Ice pile ups were studied in the field by Kovacs and Sodhi (USA) who collected information on sea ice conditions in the Bering Strait and the icefoot formation around Fairway Rock located in the Bering Strait. It is shown that the ice cover most likely fails in flexure as opposed to crushing or buckling, as the former requires less force. Several other papers on forces by ridging and pile-ups are mentioned below.

Ice overrides may cause severe damages. Harper and Owens (Canada) carried out study to estimate the frequency of overrides along the Beaufort and Chukchi Sea coasts of Alaska and thereby to delineate areas of high override potentials and their return periods. Such overrides are capable of penetrating substantial distances inland from the coast, up to 150 m in some cases.
An ice growth model, developed by Petro Canada, is mentioned by Miller. It serves to obtain an appreciation of the magnitude and timing of the response of an ice cover to specified variations in environmental conditions. "Comparison of Sea Ice Features in the Beaufort and Bering Seas using SLAR and LANDSAT Data" is mentioned in paper by Bowley and Barnes (USA) favoring SLAR imagery.

Smith and Banke of the Bedford Institute of Oceanography, Nova Scotia, presented paper on "A Numerical Model of Iceberg Drift". The movement of icebergs under the influence of winds and currents are hindcast using a numerical model, by which air and water drag coefficients were adjusted to give a best fit to the observed drift. Many hundreds of iceberg tracks were observed from drill ships and rigs off the coast of Labrador.

Icing is a difficult problem as explained in paper by McLeod titled "Atmospheric Superstructure Ice Accumulation". Ice accumulations were measured on Middleton Island in the Gulf of Alaska. The icing events were compared with meteorological conditions prevalent before, during and after icing to establish preliminary methods for design recommendations for offshore atmospheric icing on derricks, flare booms, antennas, quarters and superstructures on an offshore platform. The analysis of 10 years of recorded climatological data proved useful to provide preliminary surface meteorological criteria for atmospheric icing and hindcasting of same. Icing on fishing vessels are mentioned later.

Demolition of floating ice sheets is mentioned in paper by Fonstad et al. (Canada). Demolition of floating ice sheets is a common technique used to clear shipping lanes, to construct temporary port facilities in the Arctic and Antarctic environments and to mitigate ice jams in waterways. A series of tests were conducted to determine the optimum spacing of charges in a row. Appropriate dimensionless terms were derived.

Another method of demolishing ice is the use of high pressure water jets to cut a slot into or through a sheet of ice, thereby weakening the ice sheet. Tests run by the National Research Council of Canada are described in paper by Coveney. Scale tests were run with fresh-water ice. One result was that as fresh water ice temperature dropped substantially below freezing a considerable reduction in penetration capability occurred - apparently due to an increase in ice strength. Saeki et al. (Japan) presented paper on "Experimental Study in Flexural Strength and Elastic Modules of Sea Ice", attempting to clarify differences in flexural strength and elastic modules of sea ice, which is produced by differences in testing methods and scales.
Ice Forces were dealt with in a great number of papers. A very remarkable paper by the Dome Petroleum Co. was presented by Metge et al. (Canada). For many years attempts have been made to evaluate the effective ice pressure from laboratory and theoretical studies and relatively small (up to 3 m²) field measurements of indentation strength of ice. The extrapolation of such data to large scale failure of thick multi-year ice against a real structure, such as a production platform, however, is likely to be conservative. Direct field measurements of large scale ice forces, therefore, are important to the design of offshore structures. Such field study of ice forces was made in August 1980 during the collision of multi-year ice floes with Hans Island located in the Kennedy Strait on the NW part of Greenland. During impact, large multi-year ice floes may fail by crushing, although ridging, rafting and splitting may reduce the effective ice pressure. The importance of sideways impacts of rotating ice is discussed. The size effect on effective ice pressure against structures has been separated in a physically meaningful way, into the effect of the diameter and the effect of thickness.

"Probability Distributions for Structure Loading by Multiyear Ice Floes" were investigated by Wheeler of EXXON (USA). This paper describes procedures for calculating probability distributions for multiyear ice loads on a structure during openpack conditions. Information on floe diameters and thickness, areal ice coverage, ice movement and ice strength are combined in a Monte Carlo calculation to develop the probability excursions. Rojanky and Gerwick of the University of California (USA) describe "Failure Modes and Forces of Pressure Ridges acting on Cylindrical Towers". Their study was directed at the potential failure modes and sequences of failure of a typical sea ice pressure ridge impinging against the vertical cylindrical shaft of an offshore gravity platform. From the analysis the maximum forces on the structure can be bounded. In analyzing the interaction forces, the minimum required energy to failure concept may be beneficially adopted. Sackinger of the University of Alaska delivered a comprehensive review of "Technology for Alaskan Offshore Petroleum Recovery" including all of the environmental hazards which relate to the design for conditions in the Beaufort, Chukchi and Bering Seas.

Michel of the Université Laval (Canada) in his paper on "Advances in Ice Mechanics" reviews recent work in ice mechanics to provide a broader view of the properties of ice that could be simplified for engineering application. He concludes "It can be seen that, although ice is a very complex rheological material, it can be presented for engineers, in the first approximation only and for high strain rates, by simple rheological models. It is either an elastic or elastic-plastic model and the long accepted practice of taking the maximum shear stress failure criterion can be continued, if the values are qualified, and taken differently either for tension or compression or for non-isotropous ice".  

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"Plastic Limit Analysis of Ice Splitting Failure" was investigated by Ralston of EXXON (USA), who provided useful information on the failure modes and forces for ice floe splitting and ice plate splitting in laboratory indentation tests.

Other papers on ice mechanics crushing strength of ice broken ice, ice breaking, brash ice, etc. were presented by Sinha. Frederking and Timco, McKindra and Lutton, Sandkvist, Taylor, Wang, Urabe and Yoshitake, Kollé, Chen and Vinson and Chaichanavong. Paper by Carter et al. (Canada) discusses "Fracture of a Solid Ice Cover by Wind-Induced or Shipgenerated Waves".

The sessions on Ice versus Structures probably drew most attention and included more than twenty papers.

Basic aspects on the subject were reviewed in paper by Bruun and Moe (Norway) on "Design Criteria for Nearshore and Offshore Structures under Arctic Conditions", which reviews the probability-based reliability analyses for design of offshore structures and discuss a method which utilizes characteristic values and partial safety factors.

New design criteria for breakwaters, by which the old "formulae technique", which has proven to be inadequate and fallacious, is replaced by a hydrodynamic approach recommended by the Permanent International Association of Navigation Congresses (PIANC) in report by the 2nd Waves Commission (1976). This subject is also dealt with by Ploeg (Canada) in paper on "The Importance of Defining Wave Climate", in which Ploeg draws similar conclusions as those by PIANC and by Bruun and Moe.

Paper by Brooks of CHEVRON (USA) on "Ice Resistance Equation for Fixed Conical Structures" discusses the adaption of ice resistance equation for an ice-breaking ship to fixed, upbreaking, conical structures for Arctic Conditions.

Reddy et al. (Canada) presented paper on "Response of Offshore Towers to Non-Stationary Ice Forces" and Eranti et al. (USA) paper on "Dynamic Ice-Structure Interaction Analysis for Narrow Vertical Structures", which comprises a method for analyzing dynamic ice-structure interaction for narrow structures. Theoretical results are compared to experimental findings. Urabe and Yoshitake (Japan) discuss "Steel Selection and Reliability Analysis of Structures in Cold Regions", regarding the selection of steels, which should be used for ice breakers and structures in cold sea areas as well as for safety assessment of the structure as well as economy.
Määtänen (Finland) presented paper on "Experiences with Vibration of Isolated Lighthouses". To avoid adverse ice-induced vibration effects in lighthouses superstructures a special vibration isolation system was developed. Two test lighthouses have been built in the Botton Sea. Vibration insolation appears to be efficient in suppressing superstructure vibrations. Paper by Xu and Leira (China and Norway) also deal with vibrations in paper on "Dynamic Response of a Jacket Platform Subjected to Ice Loads".

Cederwall (Sweden) in his paper on "Behaviour of a Reinforced Ice-Cover in Regard to Creep" reports on, how different kinds of reinforcing materials have been tested in order to find the possible increase in the bearing capacity and ductility of an ice-cover. Fifty ice beams were tested and it was found that e.g. wooden reinforcement increased the bearing capacity three times and the ductility up to twenty times. The ultimate load in the case of steel reinforcement was found to be 400 MPa.

Johnson (USA) made tests on "The Relation of a Floating Ice Sheet to Simple Loads and Certain Classes of Vehicles and Machines". Two factors: the weight of the load and its pattern of loading, control the magnitude of stresses in the stresses in the ice.

McLean et al. (Canada) investigated ice platforms with Urethane foam cells in the neutral axis zone and their application in arctic offshore drilling. The tests demonstrated that the weight reduction of the ice platform through the use of foam could be assumed to be replaced by an equal amount of rig load as far as long-term deflections are concerned.

Wasilewski and Bruce (Canada) addressed the topic of "Conceptual Design for a Mobile Arctic Gravity Platform". The purpose of this study was to arrive at a solution for a year round, mobile platform for operation in water depths ranging from 30 to 50 meters. A form of monocone is proposed for further study.

One of the most interesting and demanding papers of the conference was presented by Holand (Norway) titled "Risk Assessment of Offshore Structures, Experience and Principles". Risk assessment of offshore structures in northern waters is essential because of the severe environmental conditions and the size of the installations. The paper summarizes fatal accidents in Norwegian water including the failure of the semi-submersible platform "Alexander Kielland" in the spring of 1980 (paper on this subject presented by Holand in special session). The failure statistics shows that structural failures in floating structures and helicopters dominate. World wide statistics show that the accident rate for mobile platforms is much greater than for fixed platforms. Methods of risk analysis and the application of the results are outlined. In the design the notions of accidental loads and barriers are essential. The paper also discuss safety management and the role of government control.
Several papers on shore based structures were presented including the already mentioned breakwater design papers by Brunn and Moe and by Ploeg. Wortley's (USA) paper on "Marine Piling and Boat Harbor Structure Design for Ice Conditions", also discussed the use of a compressed air melting system to prohibit the lifting and breaking of piles. Danys (Canada) delivered paper on "Offshore Structures on Weak Foundations Exposed to Large Ice Forces". He mentions particularly the problems in the St. Lawrence Waterway, where soils conditions are difficult. Special elements, such as sloped surfaces, cylindrical and conical shapes have been incorporated in the structures to reduce ice forces. Special pile clusters, floating pile and raft foundation types have been used to overcome stability problems caused by weak foundations. 40 lightpiers have been built successfully on weak foundations.

Déry (Canada) presented paper on the "Design of Wharves for Winter Navigation in the St. Lawrence River". This paper reviews ice and climate conditions prevailing in the St. Lawrence Valley during the winter season and gives a short history of wharf design. Ice problems encountered in wharf design are identified and solutions are given.

"Ice Defense for Natural Barrier Islands during Freezeup" is mentioned by Vaudry and Potter (USA). For wide structures with small freeboard, such as artificial islands, there is a risk during freezeup that the ice sheet may ride up the beach and on to the island. The ice defense system considered for Challenge or Alaska Islands, in the Alaskan Beaufort Sea, consisting of Concrete "Tank Traps" is discussed, and active defence procedures are described.

Yamaguchi et al. (Japan) presented paper on "Field Test Study of Pack Ice Barrier". Based on model and field tests of various ice resistant structures, an offshore protective structure composed on inclined and horizontal steel tubes was developed and tested in the ice-covered sea of Okhotsk. The design was subjected to forces by 3.6 m waves and to pack-ice. It was effective in keeping the pack-ice away. Wortley (Univ. of Wisconsin, USA) delivered paper on "Dock Floats Subjected to Ice". Results of full scale field tests of floating docks placed year-round in a boat harbor subjected to severe ice conditions were presented.

An interesting paper on the influence of ice on storm surge amplitudes was presented by Murty et al. (Canada). It was shown that the amplitude of the surge is directly proportional to the rate of growth of the storm, whereas the time of occurrence of the maximum surge is inversely proportional to the rate of growth of the storm.
The influence of ice on the bottom when grounding causing scours was dealt with by Pilkington and Marcellus (Canada) in paper on "Methods of Determining Pipeline Trench Depths in the Canadian Beaufort Sea". This paper describes the origin and subsequent disappearance of sea floor ice scores, then presents a discussion of the various methods that can be used to calculate the return period of ice scores and the so-called TOP (top of pipe) depth for sea bed installations.

Paper on "Model Tests of Sea Bottom Scouring" was delivered by Abdelnour et al. (Canada). The paper describes a series of model tests of sea bottom scouring, which were conducted by ARCTEC for the Arctic Petroleum Operator's Association. These tests included investigations of scouring resistance forces, pressure distribution in the soil, pressure distribution on the model front face and shape characteristics of the scour profile. Scales of 1 in 25 and 1 in 50 were applied in sand, sandy silt and silty clay.

Chari and Abdel-Gawad (Canada) presented another soils paper on the "Static Penetration Resistance of Soils". A free-fall impact penetrometer with a capability of testing the surficial ocean sediments up to a depth of 10 m was developed by the Memorial University of the St. John's and tested in the laboratory and in the field. The paper explains details of laboratory and field tests and compare the results with theoretical computations.

Ice may cause severe erosion of shores by pile-ups and ride-ups. Dionne (Canada) in paper on "L'Action des glaces sur les littoraux" explains that drift may be an important agent of erosion, transportation, sedimentation and protection. Drift ice erodes shores in unconsolidated deposits, and picks up large quantities of sediments, which are dispersed and scattered over various distances according to the melting rate of floes. The material eroded may be transferred by the ice to harbor channels and entrances.

Another paper on scours by icebergs was presented by Chari and Green (Canada). This paper titled "Iceberg Scour Studies in Medium Dense Sands" is concerned about iceberg scourings on the Canadian East Coast and its threats to the extraction process of hydrocarbon resources. The mechanics of gouging is explained together with the results of model tests in soft clayey materials as well as frictional soils. The soil movement below the scour depth is a phenomenon to be contended with both in cohesive and cohesionless soils.

Ice Navigation is an important factor in the industrial development of mineral resources in the Arctic.
Lewis (USA) in his thematic paper "On the State of Commercial Arctic Marine Transportation" claims that "decision makers in the United States do not believe arctic marine transportation is technically or economically feasible". He argues and recommends that a 100,000 to 200,000 DWT, Class 10, arctic icebreaker tanker be constructed and operated year-round from Valdez, Alaska, through the Northwest Passage to ports on the North American east coast.

Ice Navigation on the St. Lawrence was explained by McCallister and Argiroff (USA) in paper on "Extension of the Navigation Season on the Great Lakes and St. Lawrence Seaway System".

The US Government authorized a Winter Navigation program in 1970. After 8 years of study the findings of the demonstration program was that "the traditional navigation season on the Great Lakes - St. Lawrence Seaway system has been successfully extended".

Skarborn (Canada) presented paper on "Marine Transportation in Arctic Waters", which comprises a detailed study on the marine transportation of oil and LNG from the Arctic Islands to southern Canadian markets. Key elements in the study were evaluation of the required horsepower of the ships, the rate of progress through various ice conditions, the ships' fuel consumption, and other related costs. The conclusion is that a predominantly marine transportation system for oil and LNG from the Arctic Islands is feasible.

Transport by barges was investigated by Takekuma et al. (Japan and Canada) in paper on "Transit Analysis for Delivery of Large Barges to Arctic Destinations". They examined transit performance of large barges in arctic waters from Lancaster Sound to Bridport Inlet. Six barge bow types were designed and investigated through model experiments in both broken and level ice. Comparative studies on the transit performance of six candidate towing systems were made. Two towing systems are considered feasible for summer season transit. The performance of a very large, Swedish icebreaker YMER (7900 ts displacement) in the Arctic was explained by Liljestrom and Lindberg (Sweden).

During the summer of 1980 YMER spent 100 days in the Arctic from Spitzbergen to the North East tip of Greenland and back to Franz Joseph Land. Ice conditions were recorded by TV on video-tape. In addition, shaft torque thrust and rpm were recorded. In the forebody of the vessel 27 strain gauges were installed for the measurement of stresses in the hull plating, frames, stringers and web frames. Representative stress levels for various ice conditions and speeds were recorded. The paper states that "the performance of YMER (built in a Swedish shipyard) was above expectations".
One difficult problem on ice navigation is the performance of propellers during ice-breaking. Paper by Okamoto et al. (Japan) mention model tests on propeller-ice impacts and milling as well as strength tests of large-diameter model propeller. Another propeller experiment is mentioned by Sasajima et al. (Japan) and comprises the study of two different propeller designs with respect to their hydrodynamic characteristics and ice milling capability. Tests were made in a towing tank as well as in a cavitation tunnel.

Fishing is an important industry in the Arctic, but fishing is often hampered severely by accretion of ice on the vessel endangering its stability. Paper by Carlson et al. (USA) describes "Engineering for Vessel Ice Accretion with Particular Reference to the Alaskan Fishing Fleet". Five types of devices that appear promising and are being considered for further research include adaptation of inflatable membranes, improved high-pressure sea water jetting, mechanical surface vibrations, and waste engine heat supplied thermosiphons.

Ice research in the laboratory, of course, would not be possible without proper model basins. "A New Model Basin for the Testing of Ice-Structure Interactions" is described by Pratte and Timco (Canada). The Hydraulic Laboratory of the National Research Council of Canada has constructed a facility for model testing the dynamic interaction between a structure and ice cover. The facility has a refrigerated chamber, 29 x 10 x 1.2 m, in which carbamide (urea)-doped model-ice is grown. Cold air is blown in the chamber, which is instrumented for all kinds of recordings of arctic environmental features and parameters.

All in all the Proceedings of the POAC-81, which includes 3 volumes and about 1800 pages (available from l'Université Laval in Québec), covers a large range of subjects concentrating on ice subjects like ice mechanics, ice observations, ice vs structures, including shores and bottom, and ice navigation. And wave subjects like load factors and risk analysis under wave action. These subjects are well described and dealt with by competent authors. Interests are to a high extent turned towards field research and full scale tests. The Proceedings therefore serve their purpose of being a guide to Industries operating in the Arctic. The same services are supposed to be provided by two "task forces", one on ice forces, the other on penetration of environmental forces in the structure, established by the POAC International Committee. The former is headed by Dr. W.M. Sackinger, the University of Alaska, the latter by Dr. I. Holand of the Norwegian Institute of Technology. These task forces will review the work by others, fill in the gaps and report to POAC-83 in Finland.
BUSINESS SESSIONS

Two business sessions were held during the Conference and a plenary session on the last day.

A short constitution was adopted at the plenary session proposed by P. Bruun and seconded by W.M. Sackinger.

CONSTITUTION

Objectives of POAC

To improve knowledge on Arctic and Antarctic problems by having scientists, technologists, design and development engineers discuss and exchange ideas on relevant topics.

In addition to personal communications, one major objective is to have other national, international organizations, industries and research institutes engaged in work on the Arctic and Antarctic of interest to POAC, to report their work at the POAC Conference.

This is done by the organization of biennial conferences or seminars on Port and Ocean Engineering under Arctic Conditions.

This conference is left in the hands of the national sponsors who will defray all costs of the conference, charge registration fees and print and distribute proceedings of the conference to all participants and others.

Membership

POAC has no official membership and consequently no membership fees.

POAC is steered by an International Committee with memberships from many countries, which are activity engaged in engineering exploration and research activities in the Arctic and Antarctic. The committee may establish groups to perform special tasks. The committee's general business and coordination with other groups, etc. is handled by the secretary general.
President Chairman of the next conference
Vice-President Chairman of the succeeding conference
General secretariat Permanent
Committee Of 12 members
Geographical distribution: 5 Europe
5 North America
2 Others
Professional distribution:
Loose rule of 50% members from design and construction and 50% scientists and research engineers.

The Past-President stays another conference period to insure continuity.

Term of members of the International Committee

Two conference periods only. Half of the members are replaced at each conference.

Elections

A nominating committee is named by the POAC International Committee at the beginning of each conference, and reports to the International Committee.

The new Committee members shall be approved by the International Committee and finally by the plenary meeting of the conferees.

MEETING OF THE INTERNATIONAL COMMITTEE

During our meeting in Québec it was decided to elect the following new members of the International Committee:
John Kreider, USA
Kenneth Croasdale, Canada
Eero Mäkinen, Finland
Jizu T. Xu, China

P. Jumppanen, Finland, is President of POAC on a 2-year term, W.M. Sackinger, USA, is Vice-President of POAC, on a 2-year term, next President on a 2-year term. Secretary general is P. Bruun. Meeting in 83 will be in Finland. Meeting in 85 in Alaska/Greenland. W.M. Sackinger and P. Tryde will look into the possibility of having the conference at the Thule Air Force Base.
A task Force on Environmental Forces versus Structures is established.

Task Force A, headed by W.M. Sackinger will review existing information and try to fill in gaps, whenever needed.

Task Force B, headed by I. Holand, will attempt to establish criteria for and methods of analyses on the action of forces by wind, waves and ice into structures.

Both task forces will report to POAC-83.

INTERNATIONAL COMMITTEE 1983

The International Committee for 1983 is the following one:

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<tr>
<th>Name</th>
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<tr>
<td>Prof. P. Jumppanen</td>
<td>Technical Research Center of Finland, President</td>
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<td>Prof. W.M. Sackinger</td>
<td>University of Alaska at Fairbanks, Vice-President</td>
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<tr>
<td>Prof. B. Michel</td>
<td>Université Laval, Canada, Past-President</td>
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<tr>
<td>Prof. P. Bruun</td>
<td>Retired chairman, Port and Ocean Engineering, Technical University of Norway, General Secretary</td>
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<td>Dr. T. Carstens</td>
<td>River and Harbor Laboratory, Trondheim, Norway</td>
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<td>Prof. R. Dempster</td>
<td>Memorial University of Newfoundland, Canada</td>
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<td>Swedish Board of Maritime Works, Sweden</td>
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<td>University of Oulu, Finland</td>
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<td>Prof. P. Tryde</td>
<td>Denmark's Technical University, Lyngby, Denmark</td>
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<td>Mr. J. Kreider</td>
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<td>Mr. K, Croasdale</td>
<td>Dome Petroleum Ltd., Calgary, Alberta</td>
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<td>Oy Wärtsilä ab, Helsinki Shipyard, Finland</td>
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<td>Prof. J. Xu</td>
<td>Tianjin University, Tianjin, China</td>
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