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Proceedings of the Newfoundland Regional Advisory Process for 2J3KL Cod and 3LNO Skate

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ABSTRACT

This document records discussions during the Regional Advisory Process for Newfoundland groundfish stocks, specifically 2J3KL cod and 3LNO skate.
Four days of scientific peer review were followed by three days scientific views with views of fish harvesters and other stakeholders.

RÉSUMÉ

Le présent document offre un compte rendu des délibérations du Processus consultatif régional sur les stocks de poisson de fond à Terre-Neuve, notamment la morue - 2J3KL et la raie - 3LNO, soit quatre journées d'examens scientifiques par les pairs suivies de trois jours de présentation des points de vue des scientifiques, des pêcheurs et des autres intervenants.

INTRODUCTION

People with scientific information to contribute met on 22-25 March to examine the relations between different pieces of information, and how they might fit together. The meeting was expanded for 27-29 March to work on coordinating the scientific views and the views of participants in the fishing industry and other stakeholders. A smaller group continued to meet (the whole meeting was invited, but not all were able to be present) 31 March and 3-4 April at NAFC to agree on the wording of the stock status report.

This document will summarize all of the discussions of the meeting. But certain points stood out as especially important and deserving emphasis.

There is compelling evidence that the 2J3KL cod stock cannot be treated as a single entity with rapid mixing (compared with times relevant to management) throughout the stock area. All studies suggest that movement even between adjacent inshore components is very slow on time scales that can be directly observed, and on time scales of relevance to management. Perhaps much of the relevant mixing occurs not at regular slow rates, but in rare and unpredictable large events. Once it is decided to treat separate components of the stock, then understanding and measuring movement and exchange among them becomes crucial. Managers and people advising them face a much more difficult set of problems, which are far from being mastered. For this assessment, it was decided that it is necessary to treat fish spawning inshore and fish spawning offshore as separate components. Although there is strong evidence for further separation of each of these components, there was neither the data nor the conceptual mastery needed to deal with them; therefore finer divisions would be ignored. Pre-recruits (age 3 and younger) to inshore and offshore spawning components can occur together. This prevents us from observing inshore and offshore recruitment separately.

Smith Sound did not contain the only aggregation of cod inshore; but all the other aggregations that had been seen were much smaller.

There used to be a sustainable inshore fishery in excess of 100,000 t per year. The meeting was agreed that this was fed by inshore feeding migrations of cod that spawn offshore, that it could not be sustained by inshore-spawning cod alone. Therefore the recovery of the fishery to anywhere near historical levels depends on the recovery of the offshore. The main questions are (1) when will this happen; (2) can human activities affect when it will happen? And the answers to both questions are: We don’t know. We can perhaps fish the remaining inshore component at a sustainable rate on the rough order of 1000 - 10,000 t per year. If we could say "If you forego such a fishery for, say, 15 years then the offshore will recover." then managers and the industry would know what their choices were and would choose among them. If we could say even "If you forego such a fishery for 15 years then the chances of the offshore recovering are 50%" then people would know the terms of the gamble and could decide whether or not to make it. Alas, all we can say is "If you forego such a fishery for 15 years then the chances of the offshore recovering are unknown. But the payoff to future generations, if it happens, is large." This, one must confess, is rather less useful as a basis for decision and action.

There used to be a cod fishery in 2GH, but it is by now only a distant memory. There was a cod fishery in 2J as recently as the 1980s, but by now we are getting used to there being none to speak of. The meeting was full of comments of the flavour "and of course in 2J there was nothing". Now in 3K we estimate that a catch of a mere 3,000 t imposed an exploitation rate over 30%. If being cautions means anything at all, it must entail backing off 3K and hoping that it's not already too late.

WORKING PAPER SUMMARIES / ABSTRACTS AND RELATED DISCUSSIONS

Smedbol and Wroblewski WP 1.1

ABSTRACT: Using metapopulation concepts we propose a model of subpopulation structure within the northern Atlantic cod (*Gadus morhua*) population inhabiting the coastal and offshore regions of eastern Newfoundland and Labrador. Evidence for subpopulation structure is drawn from studies of spawning time and locations, morphometrics, and genetic differentiation. We identify putative subpopulations associated with spawning areas near offshore banks and in coastal bays. The historical northern cod population is first represented as an unfished metapopulation. We then modify the model to include the influence of fishing harvest on subpopulation extinction. Metapopulation theory predicts that fewer spawning areas are occupied as the population declines. This prediction is validated in that Saglek, Nain, Makkovik and Harrison Banks have had no significant spawning activity since the over exploitation by trawlers during the 1960s. The corollary prediction is that as the population recovers, currently unoccupied spawning areas will be recolonized. The model suggests a continued moratorium on fishing the remaining subpopulations would promote recolonization and accelerate the recovery of the overall metapopulation.

Are density-dependent effects important for recolonization of extinct sites? Probably but since the study covers an era of low density they did not include this effect.

If we know the mixing rates of subpopulations one might set TAC for each subpopulation, or else design a more robust harvesting strategy such as spreading harvesting out.

Don’t use the lack of data as an excuse not to use a precautionary approach.

John Green WP 1.7

Some people suspect that there are other isolated populations similar to Gilbert Bay.

If commercial fishery is allowed to continue on the same scale as it was in the recent 2 years, there will be extinction of these fish. Much of the 2J fishery was on Gilbert Bay fish.

Hence due to colouration, fidelity to spawning areas and the fact that these formed the bulk of the fish removed from 2J he feels that this is a unique population. Fish genetically analysed from Shinneys and sonic tagging both suggest uniqueness. These fish are not land locked and the sonically tagged fish did not leave Gilbert Bay but returned to the Shinneys. Is also uniqueness due to the retention of eggs. It is a gold mine of information for these reasons.

There is stratification, the eggs remain below the depth of mixing hence are retained in the Shinneys, tidal amplitude is 1 m.

DFO manages this stock as part of the northern cod stock. In the UK there is a local set of bylaws to protect the fish. We do not have this level of laws in Canada. Possibly there is a need for local initiative to protect this group of fish.

Could Gilbert Bay fish restock the offshore? Possibly. What is the sustainable harvest from Gilbert Bay so as not to fish stock below sustainability?

Is *Gadus ogac* common? Yes in a neighbouring bay; perhaps in Gilbert Bay they were excluded by *G. morhua*.

John Brattey WP 1.2

ABSTRACT: During 1 April – 3 December 1999, a total 8,825 cod (>45 cm fork length) were tagged with single, double, or high-reward t-bar anchor tags and released in Divs. 3KL at various inshore locations off the east and northeast coasts of insular Newfoundland from Notre Dame Bay southward to St. Mary's Bay. A total of 791 were reported as recaptured during 1999 from recreational, sentinel, directed commercial and by-catch fisheries. Percentage of tagged cod released prior to the fishery and reported as recaptured varied among areas, ranging from 28.6% (n=1420) in 3Ki (Fogo-Twillingate area) to 4.8% (n=1046) in Trinity Bay. Substantial recoveries (~7.2%) of cod tagged in various regions in southern 3L (Conception Bay southward) included many fall recaptures from neighboring Placentia Bay (Subdiv. 3Ps) where there was a substantial directed cod fishery with landings during the last quarter in excess of 7,500 t. The spatial and temporal distribution of recaptures is shown to be consistent with the results of other recent (1995-1998) tagging studies from these areas. The results further support my previous conclusion that the inshore of 3KL is inhabited by at least two groups of cod: (1) a northern resident coastal group that inhabits an area from western Trinity Bay northward through Bonavista Bay, the Fogo-Twillingate area, to western Notre Dame Bay, and (2) a migrant group from inshore and offshore areas of 3Ps that moves into southern 3L during late spring and summer and returns to 3Ps during fall. The timing of movement and northward extent of this migrant group may vary among years and there is some

indication that the northward migration extended further north in 1999 compared to 1998; however, during 1997 to 1999 few migrants appear have moved further north than Trinity Bay.

Would it be useful to plot just those tags recovered 12 (or 24 etc) months after placement, as a possible indication of permanent as opposed to seasonal movement? Possibly, especially for tags placed on spawning aggregations.

The results have not yet been used to estimate rates of mixing between groups of fish.

In all experiments most of the spawning was finished when the tags were placed.

Much of tagging during 80's was on the offshore banks. The fish showed good migration fidelity from offshore to inshore then offshore again. In the inshore most of the fish were recaptured within 60 miles of tagging but in both cases there was some straying.

There is a strong possibility that repported recaptures off Iceland and Norway are in error.

There were reports of fish caught in Trinity Bay that were tagged the day before in Placentia Bay. Maybe false reporting; but John tries to verify tag return locations, and there are legitimate long term movements.

There is a higher reporting rate for high priced tags. They assume 100% reporting for high priced tags and try to estimate the return rate for lower priced tags.

Pierre Pepin – summary of drift and dispersal of larvae.

Can drift patterns be modified by behaviour? Only if there is a lot of shear. If a larva is allowed 5 cm s^{-1} of movement then it could move wherever it wanted. Assuming it knew where to go.

Anent George Rose's theory that if the cod are to recolonize the south they would have to be spawned in the north: it is possible, but we do not understand cross bank movements.

Residence times on banks are variable but perhaps about 60 days.

How do spawn location, drifter, retention and behaviour effects at nursery grounds affect recolonization?
If you tag fish in inshore they disperse offshore, but if you tag fish offshore they move inshore. Because you find an animal in any one location there is no reason to believe that the fish will remain there. We have to learn about fish behaviour to understand where they will be found and when they will be found there.

Cod return to their home areas at 4 years of age.

Transport out of coastal bays is a small effect compared to other sources of mortality.

Why are no offshore 0-group cod found in the northern areas when they are found on the southern banks?

Capelin larvae are found in inshore areas. You can track sizes of capelin. As they move offshore they are older and hence larger.

Terry Beacham WP 1.3

ABSTRACT: The purpose of this study was to describe population structure and to determine the potential for genetic stock identification of Atlantic cod (*Gadus morhua*) in Newfoundland and Labrador using microsatellite loci, synaptophysin (SypI) locus, and a major histcompatibility complex (Mhc) locus. Variation at seven microsatellite loci (Gmo3, Gmo8, Gmol9, Gmo34, Gmo35, Gmo36, and Gmo37) and SypI was surveyed in approximately 5,050 cod from 19 putative populations. Variation at a class I Mhc locus was surveyed in 2,000 fish from the 19 populations. Ten populations were sampled over two or more years, and variation among populations was on average about 18 times greater than annual variation within populations. Regional structuring of the populations was apparent with inshore and offshore spawning populations forming distinct groups. The Flemish Cap population was the most distinctive of the offshore group, and the Gilbert Bay population in Labrador was the most distinctive of the inshore group. In Divisions 2J3KL, no significant genetic differentiation was observed among inshore cod sampling sites in Notre Dame Bay and Bonavista Bay. Some differentiation was observed between sites in Conception Bay and Trinity Bay, and also with other inshore sites, providing some evidence of distinct "bay" stocks of cod along the northeast coast of Newfoundland. All inshore cod sites were genetically distinct from all offshore samples of northern cod. The offshore samples were more heterogeneous, and there may be at least three distinct offshore spawning populations of northern cod. In Subdiv. 3Ps, genetic differentiation was observed between the inshore Placentia Bay and Fortune Bay samples, and the Placentia Bay sample was distinct from offshore samples of northern cod. Simulated mixed-stock fishery samples of northern cod suggested that variation at the seven microsatellite loci, the synaptophysin locus, and Mhc locus C should provide reasonably accurate estimates of stock composition (inshore vs. offshore) when the inshore component comprises at least 50% of the mixture. In Subdiv. 3Ps, bias of estimated stock compositions was marginal when offshore populations (Burgeo Bank, Halibut Channel) comprised the majority of the sample. However, bias in the estimated stock compositions increased when inshore populations comprised the majority of the sample. Increased baseline population sample sizes or additional discriminating markers are likely required to decrease the degree of bias in the estimated stock composition in this application.

How many allele differences are needed to say that two populations are distinct? One would be enough, but you should be conservative and have at least two.

The Gilbert Bay fish had the greatest genetic difference from all other populations. The second greatest difference came from Hawke Channel.

The results indicated three distinct offshore breeding populations.

Among the inshore populations there were some samples - the Notre Dame, Bonavista Bay and Trinity Bay cod - that could not be distinguished from each other. Thus the method does not inevitably find differences.

There was no difference between Trinity and Fortune Bays, however, there was a great difference between Placentia and Fortune Bays. The trend was nearby bays were more similar than distant bays.

What does this analysis tells us about mixing rate? Under the assumption of stable populations at equilibrium, fewer than 5 effective breeders per generation using an evolutionary time scale. Although various technical assumptions are violated in practice, Terry is confident that the number must still be under 1000. This suggests that far fewer than 1% of the strays seen in tagging experiments can become effective breeders in a new location.

The significance levels were corrected for the number of tests that were tried.

Interannual genetic differences, which are the measure of within-group variance, are much less than most between-bay differences. Gilbert Bay had only 1 genetic difference between years.

There is no evidence of movement of fish from the offshore into Placentia Bay.

Terry Beacham WP 1.4

ABSTRACT: Recruitment of O-group Atlantic cod (*Gadus morhua*) to the near shore of coastal Newfoundland occurs in two or more pulses. The two largest of these recruitment pulses occur in August and in late September to October. We investigated the origin (parentage) of these two recruitment pulses appearing in Newman Sound (Bonavista Bay), Newfoundland using genetic variation. Variation at seven microsatellite loci (Gmo3, Gmo8, Gmo19, Gmo34, Gmo35, Gmo36, and Gmo37) and the synaptophysin locus (Sypl) was examined in a sample of 200 0- group cod from each pulse collected by seine during August 16-18 and October 12-13, 1999. Genetic variation within pulses was compared with that of four offshore and five inshore populations of adult cod. For both pulses, variation was most similar to locally spawning adults from within Bonavista Bay in over 50% of O-group cod. Of those individuals most similar to adults outside Bonavista Bay, the majority were most closely related to those collected north (i.e., "upstream") of the bay. Comparatively few O-group were most similar to aggregations of adults sampled to the south, and then only in O-group sampled in October. Offshore

populations (primarily Funk Island Bank) were estimated to contribute 49% of the August pulse, but only 30% of the October pulse.

Should there be a testing sample of a known population and the second sample from an unknown population to test whether the method works? But how would one know whether the first sample was genetically pure or a mixture?

The message is that there is no way of learning about levels of mixing.

This degree of genetic separation indicates that the mixing implied by Pepin's analysis must be reversible. Which seems to imply that individual larvae know where they were spawned, where to return to.

There always is a component of young fish that stays near the inshore and a component that moves offshore. It is not clear what mechanism keeps some fish inshore and some inshore.

The inshore probably always was the nursery area for the offshore stock , and then there was an ontogenetic movement offshore.

If fish normally spawning offshore would move inshore and the stayed inshore, what would they do when spawning time came. Would they skip spawning, rush back offshore or spawn inshore? Possibly there would be a homing mechanism; could this be checked by genetic studies?

John Brattey knows of no studies indicating fish that spawn offshore, move inshore and stay inshore. However, tagging studies in the past found 9 cod that strayed offshore and spawned there rather than spawning inshore.

George Rose WP 1.5

The tags have already outlasted the 2 year guarantee.

Of the 48 fish tagged (av. Fish size 70cm), 12 were caught and returned. This is a substantial exploitation rate.

Of the Placentia Bay spawning grounds, the Bar Haven grounds have the highest fidelity, the other areas are less regularly used.

All areas were covered with the same intensity.

Richard O’Driscoll WP 1.6

SUMMARY: Northern cod (*Gadus morhua*) and capelin (*Mallotus villosus*), their major prey, have both exhibited major changes in spatial distribution during the 1990s. To study the influence of these changes on the predator-prey interaction between cod and capelin, we computed numbers of potential capelin prey surrounding cod predators (potential contact) from spring acoustic survey data. Between 1989 and 1994 the distribution of cod NAFO 2J3KL shifted to the south and east. This distribution shift occurred at the same time as similar changes in capelin spatial distribution, and potential contact between cod and capelin did not decrease in spring surveys from 1991-1994. Since the mid-1990s cod have been concentrated inshore with low densities offshore. Potential contact of remaining cod with capelin is high. High acoustic densities of capelin were observed inshore in spring 1998 and 1999 in Placentia Bay and Trinity Bay close to aggregations of cod. Offshore, acoustic estimates of capelin density and biomass in 1998 and 1999 were low compared to the 1980s. We suggest that cod are unlikely to re-establish offshore areas in 2J3KL unless capelin abundance offshore increases. There have been encouraging signs of a return to a more northerly distribution of capelin off southern Labrador in 1998 and 1999, and we are currently monitoring the response of northern cod to this change.

[Some relevant parts of the discussion of WP 11.1 are also recorded here.]

The paper started by presenting the contemporaneous spatial distributions of cod and capelin. But for recent years it moved to comparing the detailed spatial distribution of cod in one year with capelin in another. The point of this was less clear. The paper didn't appear to demonstrate that the cod-weighted potential contact was greater than the unweighted, although that would seem to be implied by the underlying idea.

Liver condition index is correlated with potential contact with capelin. Hence when talking about cod population dynamics you have to include capelin densities.

It is hard to compare the results here with the capelin SSR, which does not estimate biomass.

Is genetic evidence consistent with the thought that cod will move offshore? There are probably strays and possibly density dependent effects causing cod to move offshore.

There is lots of prey offshore other than capelin, for example shrimp - but possibly not high-quality (e.g. high-calorie) prey. The condition of cod in the recent RV surveys has been at the long-term average.

Timing is important to location of cod, and we have to interpret seasonal distributions of both species.

It might be enlightening to analyse the spring multi species cod catches with this model.

There was some discussion of whether the amount of capelin per cod or per cubic metre was the more relevant quantity. If it is, then in the present conditions of low cod and therefore high capelin per cod, there was less of an issue. Counter to this it was argued that an individual cod cares about capelin per cubic metre, not per cod (although a large cod concentration could reduce the capelin per cubic metre that would be there later).

It is only a hypothesis that cod will not recover offshore until capelin do. (It is also only a hypothesis that capelin abundance offshore is low. Acoustic explorations and reports from inshore fishers suggest this; but comprehensive information is lacking.)

Be careful not to confuse two ideas: the influence of capelin on the distribution of cod, and on their recovery.

Disappearance of capelin and the decrease in condition in cod coincide. As a stock area, at the same time condition decreased in 2J, increased in 3L. But current offshore condition from RV samples is about the long-term average.

Would it help to know the partial fullness index now, to learn what fish in the offshore eat now?
But there may be a difference between what cod are eating now and what is required to maintain a population at the size we would like to see.

The stock status report should reflect the importance of capelin as a prey species as a source of possible impact on rebuilding of cod in the offshore.

General discussion of stock structure issues

If we are to think of 2J3KL cod as consisting of more than one population, with some movement among populations at least at some times of their life, then their dynamics becomes more complicated and so does the interpretation of data and the amount of information required to reach any conclusion. At this stage, even if we decide to deal with more than one component, we do not propose a mathematical model of the dynamics of interacting components, but rather a conceptual model to help us in organizing our thoughts.

We may not need a population dynamics model, with consideration of density dependence and equilibrium states, to assess the state of the stock (numbers at age) today. But to provide advice, especially in a precautionary framework, we do need a model to predict where things might settle as well as where they are now.

What we heard on the first day should allow us to eliminate the one component model on the basis of biological differences, genetics, and tagging studies. Meristics show a sharp break between the Grand banks and the Northeast Newfoundland Shelf. Tagging studies show that fish from different offshore banks migrate differently. But we are not in a position to put numbers on rates of mixing or exchange between components: it is low when we try to measure it directly, but possibly still large enough to have implications for management in terms of what happens in one bay affecting other bays.

The Gilbert Bay population should not be given undue weight: it is a single unique pocket of fish and there is a much larger stock to consider. Though it may be an example of a component with essentially no exchange with anywhere else.

One problem is that the populations are presently low and this may affect current mixing rates (why explore a long difference if there is plenty of room near home). By cropping the inshore to lower levels we are creating a self fulfilling prophecy that the inshore will not recolonize the offshore, hence there are management implications to fishing the inshore. On the other hand, the genetic differences have developed over millenia during which the population were much larger.

There is no evidence of offshore fish moving inshore presently. The fish appear to be stationary. This has mixing and fishing implications. The Fogo Island fishermen noted what they called the "Capelin" fish coming inshore: the fish could be moving between shore and Funk Island Deep.

George Rose noted that he believed in site fidelity but believed that not all animals do the same things at the same time. If there are a lot of capelin that come inshore, some cod follow them offshore then it is plausible that some will stay offshore.

There are two ways for the offshore stock to recover. (1) Resurgence: the intrinsic population growth of the small population that now spawns offshore. (2) Recolonization: some fish that were spawned inshore moving to start spawning offshore. Speculation about things that may be happening or may happen in future, whose possibility it would be cautious to take account of.

1. Some offshore cod may make a feeding migration inshore so that an inshore fishery is taking offshore cod and delaying offshore resurgence.
2. Inshore cod may be more ready to recolonize offshore if inshore concentrations increase over current levels. For example, if the inshore stock were to increase, then it might extend further offshore the range over which it feeds. This might then make it more likely that a group of inshore fish would make the jump to spawning offshore. We do not know how much time this would take, though it *may* happen quickly – perhaps due to a single fortunate event rather than a gradual process – once a threshold level is reached. There has certainly been no sign of it in the 8 years since the moratorium. Given that there are genetic differences, we do not even know that inshore fish are genetically suited to thrive offshore.
3. More of the inshore may be colonized if current inshore concentrations increase. Though we do not know the size of the inshore component of the population in the past.

John Wheeler WP 7.1

ABSTRACT: An acoustic survey was conducted during the fall of 1999 in the coastal waters of Bonavista Bay - Trinity Bay, Newfoundland. The survey area included water depths from the coastline to the 120 m contour. Echo integration along a series of parallel transects within the survey area provided distributional information, densities and a biomass estimate of Atlantic Cod. Comparisons were made with a similar survey conducted in the fall of 1996. The distribution of cod differed between the two surveys. A substantial aggregation of cod was detected in Smith Sound, Trinity Bay during the current survey which was not evident in 1996. It was concluded that there were no other large aggregations of cod, comparable to that in Smith Sound, within the survey area during the fall of 1999.

John is reluctant to read more into this survey than to say that surveys conducted in 1996 and 1999 using identical methods detected similar total amounts of cod inshore, though differently distributed. Compared to other acoustic investigations, the sampling frequencies were different, the bottom detection within the exclusion zone is not good, the sampling depth is restricted to a max of 120 m for herring surveys, and one can not quantify the number of fish within the exclusion zone.

In fall of 99, substantial aggregation of cod in Smith Sound not evident in 96 or 97 indicates seasonal movement during Nov – Dec. It is unlikely that there were other large aggregations of cod, comparable to that in Smith Sound, within the survey area during the fall of 99.

Cod were distributed in shallow water within the survey area with peak densities in water depths of 20m

By January the fish have gone downward therefore the local distribution patterns must have changed greatly. But this would not be a problem during mid November.

It is possible that the fish seen here are resident fish and that the survey missed a large body of fish. But in 97 there were more fish, so they were probably not all resident. It is possible to find separate groups of fish within 2 km horizontally but in very different habitats; one can speculate that they are of different origins.

George Rose WP 7.2

The exploratory survey was designed to search for aggregations of thousands of tons of cod, and then conduct a biomass survey of any aggregations found.

How was it decided which bays to search? (The inside of the Bay of Islands was missed, for example.) George was looking for areas similar to Smith Sound, decisions were based on bathymetry, but also used local knowledge and John Anderson's findings.

Some small cod (<24cm) caught in Bonavista and Trinity bays but not detected on echosounder – 1 year olds and 2 year olds. Fish longer than 32 cm were found only in Smith Sound

George Rose WP 7.3

ABSTRACT: A relatively large spawning aggegation of cod was first observed in Smith Sound, Trinity Bay in the spring of 1995. Since that time several acoustic surveys have been attempted. The original survey results have been reworked here to conform with revised analytical protocols comparable to those used in present surveys. The first 1995 survey indicated the presence of approximately 9.5 million adult cod, primarily of year-classes 1990, 1989, and 1988. A survey in the spring of 1998 indicated the presence of approximately 7 million adult cod primarily of the 1990 and 1992 year-classes. A survey in January 1999 indicated the presence of approximately 11.5 million fish primarily of the 1995, 1994, and 1992 year-classes. Relatively strong recruitment from the 1995 spawning was suggested, as is a difference between the recruitment patterns in offshore (where the 1994 year-class dominates) and Smith Sound. Surveys in June 1999 indicated the presence of less than 1 million adult cod, and it is thought that these surveys caught the end of spawning in 1999, and that most cod had already left the Sound. Surveys in January 2000 found a large abundance of cod of ages 2-15 (ca. 11-12 million fish in total). These fish were size and maturity segregated within the Sound and comprised a total mean biomass of ca. 22,000 t (mean of 2 surveys). The 1990 and 1992 year classes were still present in relatively large numbers and the year-classes of 95, 96, and 97 were relatively well represented.

Used Wade Hiscock's et al. Survey block pattern. Very high densities of cod near bottom during January.

The acoustic abundance estimate is a relative index, because it is calibrated against the Campelen trawl whose catchability is not 1.

Last June there were not a lot of fish in Sound, seems to be an annual outward migration. There is a vertical diel migration, coming up in daytime.

During winter the cod are densely packed near the bottom. These densities are a high as have ever been found: as dense as 10 per square meter, which is about max. packing for the species.

To separate fish from bottom, when editing manually the bottom is expanded and edited out before the backscattering density is found.

Fish are densely packed but do move within the sound. The distributions over the blocks changed greatly over the Sound. The total number of fish did not change but the fish were clearly moving over a small time scale

It is very difficult to sample the fish during the winter. Sets must be only 1 or 2 minutes in duration.

The younger fish showed up very strongly in the 2000 survey. In June there is a completely different length frequency (these are post-spawning fish). During January you get a broader length frequency with more small fish, down to age 2. The overwintering and spawning aggregations therefore appear to be different. The spawning aggregations tend to be the older fish, in lower concentrations. George has never measured a spawning density. Probably not all of the adults found during winter in Smith Sound spawn there; some move out to spawn in unknown locations.

Can follow the cohorts from the samples of cod over the years. The age structure has changed over the years and this is compatible with increasing population numbers; although the total number and biomass of the Smith Sound aggregation has not changed greatly over time. There is a higher number of younger fish in Jan 2000.

The confidence limits on the estimates are broad mainly because the design is evolving and there are still a lot of zeroes. Probably the answers have nothing to do with the subjectivity of the operators.

There is a great deal of confidence in internal consistency of ages between three surveys. The instantaneous mortalities were calculated (~0.5). There did not seem to be high z values for the 1992 and 1991 year-class fish (<0.2) found in 1999 and 2000 surveys.

The plots show annual migration patterns of fish into and out of the Sound. One can make annual predictions of when fish will be in Smith Sound. There may be two or three groups of fish spawning in Smith Sound or other areas. The conclusion that spawning is elsewhere is based on patterns in maturities over 4 or 5 days.

In June the higher proportions of large and mature fish are toward the head of the Sound. Those on the outside may be large fish but appear to be skip spawning.

There appears to have been recruitment to Smith Sound after 1994; it is not clear why.

It is interesting that the largest fish are on the inside. This is similar to John Wheeler's results. The length distribution found in November are similar to George’s January findings, however, the spatial distribution is different. John’s fish were along the slope whereas George’s were on the bottom.

George said that he has only seen the echosounder mistake cod for bottom in one other case (Placentia Bay) because the fish are so dense.

It is interesting to learn about Smith Sound because we end up formulating our ideas of the inshore from the Smith Sound findings. But we have to be careful in our conclusions.

There probably are no other large aggregations in 3L inshore; aside from tagging studies this is probably the only quantitative inshore knowledge we will have.

Noel Cadigan and John Brattey WP 2.1

ABSTRACT: We develop a model for estimating growth of Atlantic cod in NAFO Subdivision 3Ps and Divisions 3KL during 1997-1999. We use tag-returns from the commercial, sentinel, and food fisheries in these regions during 1997-1999. The returns are from numerous tagging experiments conducted in 3Ps and 3KL during 1997-1999. Many of the tags that are returned supply the length of the fish at capture, and we use this information to estimate growth based on known lengths-at-release and times-at-liberty. We use the von Bertalanffy growth model, modified to accommodate seasonal variations in growth. We also incorporate a measurement bias component in our estimation because the tag-return data suggest that there is some bias in the lengths-at-capture reported by fishermen. The basic approach we use has been studied fairly extensively in the fisheries literature, and we present a review of this literature. Important biases in estimating growth from tagging data have been identified in the literature, and we assess the magnitude of the bias in our analysis using a simple simulation. The conclusion from our simulation is that the bias may not be substantial for our population. We use the estimated model to predict cod growth for times at liberty ranging from 0-3 years, and for fish lengths ranging between 40 and 90 cm. For example, we estimate that a 40 cm fish will grow 22 cm during 1997-1999, but in the same period a 90 cm fish will grow only 10 cm.

Results are not obviously at variance with growth rates derived from trawl surveys.

There is possible distortion if the gears select for certain lengths and hence (for a given starting length) for certain growth rates.

Joanne Morgan WP 2.2

ABSTRACT: Estimates of proportion mature at age and age at 50% maturity were produced for Div. 2J3KL cod using a cohort maturation model. For 2J3KL combined, age at 50% maturity has been declining for both males and females since the earliest cohorts that could be estimated (those of the late 1950's). Differences between estimates of maturity from this method and the annual method are generally small but variable. SSB produced using the two methods show similar trends with a maximum difference of 246 000 t and 26%. Estimates of proportion female at age were found to increase with age and were variable across time. Comparison of SSB using fixed and variable proportions female showed that the two methods produced similar trends with the variable sex ratio method generally producing higher estimates with a maximum difference of 31 000 t or 9%. When SSB produced using annual estimates of maturity and a fixed sex ratio was compared to SSB produced using cohort estimates of maturity and a variable sex ratio the maximum difference was 95 000 t and 27%.

Fitting a model to proportions mature at age for each year separately can sometimes lead to the phenomenon of the proportion mature in a *cohort* decreasing from one age to the next. This might be an understandable consequence of fish that skip spawning being misclassified as immature, or of differential mortality for example by extra high fishing pressure on spawning fish. But in any case, the new approach of fitting the model to each cohort separately means that such an anomaly cannot occur.

Is the number of mature females the proper way of estimating egg production? Possibly: there are other factors such as fish condition that are thought to affect egg production but residuals from Beverton-Holt stock-recruit models for two species (four stocks) showed some consistency with proportion of mature population that was female.

John Anderson and Edgar Dalley WP. 7.5

Is this a practical assessment tool considering that it took 3 weeks just to survey Terra Nova Park? But much of this time was in initial design and experimenting and would not be repeated every year.

How can you extrapolate this study to the entire coast? Still needs to be worked out. Once John knows the appropriate scaling he can indicate the cost of a full scale study

Qualitatively John felt that there were fewer cod in 99 than 98.

If they found a concentration of cod, they used feather hooks to confirm. If they got a signal but did not get anything with feather hooks, the fish could have been juvenile or herring.

There was good coherence between sites of cod catches and acoustic signals a few days later.

Dawn Maddock-Parsons WP 8.1

Control sites were selected by the participants at the start of the Sentinel program in 1995, as places where reasonable catches would be expected throughout the year, and were permanent thereafter. were permanent. Experimental locations were set on a daily basis by fishermen. Set at sites where the fishermen thought there would be fish. Most fishers are not using sounders to locate fish and set on them. Most fishers have sites in mind when they set out to fish. Generally set on traditional grounds or someone suggests they might get fish. Some say they use sounders, mostly in the north where catch rates are lower.

There is a bimodal catch rate in 2J3KL with a peak in June and one in the fall. The bimodality shows up in site-by-site plots; so, site is not likely a confounding factor here.

There were 12 trap sites in 3KL and none in 2J.

Edgar Dalley WP 6.1

ABSTRACT: The pelagic juvenile fish survey was carried out from August 23 to September 19, 1999 from southern Labrador to the southern Grand Bank, including the inshore areas of the northeast coast of Newfoundland (NAFO 2J3KLNO). Abundance of pelagic juvenile Atlantic cod (*Gadus morhua*) was high in 1999, compared to surveys carried out 1994-1998, mostly as a result of high catches inshore. However, similar to 1998, higher abundance in 1999 also resulted from relatively high abundance on the Grand Bank (3LNO). Higher abundance was observed on the Northern Grand Bank in 1999, in contrast to 1998, when higher abundance occurred on the Southern Grand Bank.
Abundance on the Northeast Newfoundland Shelf (2J3K) was low offshore while inshore it was the highest of any year. Over the six years of the survey, abundance in 2J3 KL declined from 1994 to 1996 and has increased since then. Catch rate inshore in 1999 was over 3x the previous high in 1994. Highest catch rates occurred in Conception Bay (3L) and Notre Dame Bay (3K) along the northeast coast of Newfoundland. Abundance in Trinity and Bonavista Bays was relatively low and almost no cod were found offshore in 2J3K. On the Grand Banks, juvenile cod were found throughout the surveyed area.
Juvenile cod on the Northern Grand Banks (3L) were not as disparately large than more northern areas (2J3K) as they were in 1998. Cod in 3NO averaged 57.5 mm, while those in 3L (NGB) averaged 53.9 mm in length compared to the north within the inshore bays where cod averaged 38.1 – 49.5 mm in length. Condition factor was relatively high (1.73) in 1999, in contrast to 1998 when the lowest mean value (1.34) of any year was recorded. The relative condition factor was lower for 3NO than in 2J3KL. Preliminary growth rate estimates were higher than those of previous years averaging 0.720 mm/day. Larval hatching dates were earlier and spanned a shorter period of time in 1998, compared to 1994-1998. To the north, in 2J3KL, the production of young fish has increased in inshore areas and also on the Northern Grand Bank in 1999, but continues to be extremely low with no sign of recovery in the offshore areas. A relatively large year-class was measured on the southern Grand Bank for the second year in succession and appears to be a positive response by Atlantic cod to a warmer environment. There has been a general warming of oceanographic conditions throughout the period of the pelagic 0-group surveys (Colbourne 2000).

With the geometric mean computation, if two populations have the same total number, the one that is more even will be given a higher index. Why is this a desirable property? The authors do not know, but use it because the literature does. It does mean that the index is less influenced by one or two enormous catches. The general message of the 1999 survey is about the same if arithmetic mean is used.

The lengths of juveniles appear not to agree with what we know about advection. The mean lengths of Notre Dame Bay and Bonavista Bay fish are larger than in other bays. Possibly the fish were born in different areas. Then on the southern Grand Banks the fish get larger again. The only consistent pattern in size was found in the 1998 data. There was an increase in size from north to south; this is consistent with advection from where we believe fish are spawning.

How do we eliminate the possibility that the 1999 results are an artifact? It seems unlikely. The range had extended to include southern Labrador and the Grand Banks for the first time. The proportion of non-zero catches was much larger. In short, none of the warning signs of a possible anomaly were there. But of course one can never be certain – just wait for next year's look at the year-class.

Is the index sensitive to the division of the survey area into blocks with somewhat arbitrary borders? Tests for robustness will be performed for future years. Removing all borders between blocks gives about the same answer.

Bob Gregory et al. WP 6.2

ABSTRACT: Recruitment of age 0 Atlantic cod to the nearshore in 3KL in 1997 was the highest on record during the annual Fleming survey in the 1990's (Methven et al. 1998). This survey was not conducted in 1998 or 1999. In its absence, we conducted a qualitative assessment of the strength of the 1997-99 year-classes based on abundance of demersal age 0 and age 1 Atlantic cod in the nearshore in Newman Sound, Bonavista Bay in the summer and fall 1998. Four years of data from Newman Sound were available - 1995, 1996, 1998, and 1999. We compared abundances of age 0 and age 1 Atlantic cod in Newman Sound in 1998 to those in previous years and with those reported during the Fleming surveys (1992 -97). The sampling techniques used during both surveys were identical and data were available for the same times in the fall for use in interannual comparisons (early to mid- October). Therefore, abundance data from the two surveys were comparable, albeit at different spatial scales. We observed good correspondence of abundance trends for both age groups between the surveys in 1995 and 1996. In both data sets, 1996 showed the lowest age 0 abundance among years. The Fleming survey results for 1997 predicted that age 1 abundance in 1998 would be higher than in previous years this decade. High age 1 abundance in 1998 compared to previous years in Newman Sound supported this prediction. Age 0 Atlantic cod abundance in 1998 was over double that in 1995 and a full order of magnitude higher than the historical low in 1996. Similarly, age 0 abundance was 40-times higher in 1999 than in the historical low. We suggest that relative to other year classes in the mid- to late 1990s, the 1997-99 year classes should rank comparatively high, especially when compared to 1995-96. Analysis of length frequency data collected from July to November in Newman Sound indicated that age 0 Atlantic cod settled in the nearshore in two distinct recruitment pulses in "good years" -1998 and 1999 -the first arriving in early August, the second in late September. In "bad years" -1995 and 1996- only the second of these pulses was evident. The length frequency data also suggest that the pulse structure may remain intact through the first winter and be detectable in age 1 cod.

The lines drawn to separate year-classes, and spawning pulses within a year, were drawn by eye, not on the basis of any statistical analysis.

The pelagic 0-group survey will miss the first pulse of settling fish.

Will year class strength be affected by the environment? Probably. These pulses have been found in other studies and hence are probably real. The sampling is done independent of tidal cycles, but does not have the frequency of sampling to test effect of lunar cycles.

Small fish were collected in November: possibly there is a third spawning pulse.

George Lilly WP 5.1

The 1994 year-class disappears in the surveys, as it does in other work.

Aging of the 1999 survey has not been completed; so, one cannot tell if the 1999 distribution plots were driven by 0-group cod.

Lilly et al. 1992 working paper on Catches of juvenile cod …..

Shows plots of catches in number by age. The 0-group are near the shelf. As the fish get older, they move offshore. Even with the Engels trawl we see the same pattern that Edgar and John found. The inshore is a preferred nursery area; the stronger year classes have wider distributions.

Juvenile cod small enough to be 0-group or 1 year old fish were not found in predator stomach or in the shrimp research surveys.

George Lilly, E. Murphy and M. Simpson WP 1.8

EXTENDED SUMMARY: The relative abundance and distribution of juvenile cod in Divisions 2J3KLNO in the autumn of 1995-1998 and in Divisions 3LNOP in the spring of 1996-1998 (plus 1999 for 3Ps only) were inferred from catches during resource assessment bottom-trawl surveys. Surveys since autumn 1995 have been conducted with the Campelen 1800 shrimp trawl, which is far more effective at catching small cod than the Engels 145 Hi-rise trawl that it replaced. In Division 2J3KL, there is evidence that cod in some parts of the inshore are faring better (higher densities; larger, older fish) than cod in the offshore. If this is so, then it is of interest to determine if these areas are experiencing different recruitment patterns. In addition, there has been considerable interest in distinguishing and managing individual components within the 2J+3KL cod stock complex, and indeed a quota was established for inshore waters only in 1999. If a management regime with two or more components or substocks is to be considered, then it will be important to create separate recruitment indices for the various components. Previous work on the distribution of juvenile cod in Divisions 2J3KL has revealed that individuals of ages 0 and 1 were found mainly in shallow waters near the coast off southern Labrador and northeastern Newfoundland and on the northern Grand Bank, that individuals of ages 3 and 4 were mainly in those offshore areas occupied by older cod, and that individuals of age 2 were intermediate in distribution. Catches from autumn surveys in 1995-1998 have revealed a similar pattern, with the notable exception that the 1994 year-class, which has been the strongest year-class appearing in the surveys since at least the early 1990s, was already well onto the shelf by age 1. More recent yearclasses have been extremely weak in Division 2J, but have been found to be somewhat more abundant adjacent to the coast in Divisions 3K and 3L. It will be of considerable importance to determine (1) whether these yearclasses are recruiting to the shelf environment from settlement in the offshore or settlement in the adjacent inshore and subsequent migration to the offshore, {2) whether they are derived from spawning on the shelf or spawning inshore and {3) in the case of inshore spawning, whether the parents belonged to a group that is genetically distinct from groups on the shelf. The ontogenetic shift from a coastal distribution at age 0 to a more shelf-wide distribution by age 2 will make it very difficult to create different indices for inshore spawners vs offshore spawners, or inshore substocks vs shelf substocks. Plots of distribution at age from the autumn surveys also revealed a concentration of juveniles on the southern Grand Bank (Divisions 3NO). Presumably, these fish belong to the 3NO cod stock. During the surveys of 1995-1998, very few juvenile fish were found on the northern Grand Bank.

Plots of distribution at age from spring surveys in Divisions 3LNOP revealed the presence of age 1 cod mainly on western St. Pierre Bank and the southern Grand Bank. Very few were caught in Division 3L. Age 2 fish were more broadly distributed from 3Pn across 3Ps and onto the southwestern Grand Bank in Division 30. With increasing age, the cod became more heavily concentrated in northwestern and southeastern Division 3P. Age 2 and older fish were also caught in low numbers on the southernmost end of the Northeast Newfoundland Shelf in northern Division 3L.
The identity of the spawning populations that gave rise to the juveniles caught in the autumn and spring surveys is of considerable interest but difficult to infer from surveys alone because of limited bottom-trawl surveying during spawning periods, the lack of egg surveys and little knowledge of the drift patterns of eggs, larvae and early juveniles. Of special interest is the distribution of the 1989 and 1990 yearclasses, which were not particularly strong at the juvenile stage but were protected during the moratorium years and contributed much of the spawning biomass of 3Ps and 3NO cod in the mid- to late-1990s. These yearclasses were found during spring mainly on the outer slopes of Burgeo Bank and the outer ends of the gullies in southeastern Subdivision 3Ps and southwestern Division 30. These yearclasses also contributed strongly to the cod caught inshore in Divisions 3KL during the 1990s, but they were caught in very low numbers in the offshore of these Divisions.

Each of the year classes is appearing in 3KL from somewhere. There are very few fish in 2J. Because there is little spawning offshore, it is more compelling to think they are coming from inshore.

There are no 0-group fish in Hawke Channel whereas there are 1-group fish. But there are young fish consistently off St. Anthony, which could have moved northward to Hawke Channel.

Could the pattern could be a result of catchability problems? Not likely, because the 0-group are consistently found in certain areas.

In 3P the distribution of age is similar, 0-group in inshore as the fish get older, they move south into the offshore.

The dominant spawners in the 1990's were from the 1989 and 1990 year classes. The dominant year classes in 3NOP are major contributor in the inshore south from 3K. The 1994 year class is dominant in 2J3K.

The broad distribution of age 1 fish in 1995 indicated in this analysis is consistent with that found in the pelagic juvenile survey.

George Lilly and Mark Simpson WP 11.1

ABSTRACT: Changes in the trawlable biomass and distribution of capelin (*Mallotus villosus*), Arctic cod *(Boreogadus* sp.) and sand lance (*Ammodites* sp.) are deduced from their by-catch during standard resource assessment bottom-trawl surveys during the autumn in Divisions 2J3KL and during the spring in Divisions 3LNO. Catches of these species have increased dramatically since the autumn of 1995 when the Engels 145 Hi-rise trawl was replaced by the Campelen 1800 shrimp trawl as the standard gear in these surveys. There has been no attempt to conduct comparative fishing experiments for these three species with these two trawls, so the change in gear represents a break in the time series for each species. In general , Arctic cod were found mainly toward the coast in Divisions 2J3K, capelin were found throughout the area and exhibited seasonal changes in distribution, and sand lance were found only on the plateau of Grand Bank. The most notable changes in distribution involved a move toward the southeast in Division 2J3K by both capelin and Arctic cod in the early to mid-1990s. In the past two years both species appear to be returning to distributions seen in the 1980s. Other notable features include an increase in the trawlable biomass of Arctic cod in the early to mid-1990s and a decline in the trawlable biomass of capelin during the same period.

Yankee survey gear was used on the Cameron in the 1970s. It is effective at catching sand lance. There is no sign of a steady decline in prey from mid-1980s on.

There seems to be lots of prey offshore, capelin is by far the dominant prey in the stock area. Up to 60% of total consumption is capelin. Access to the capelin (ie distributions) is a different question.

These are trawlable biomass estimates, using historic offshore strata, not inshore, but deeper than index strata for cod (down to 1000 m).

Capelin have become closer to bottom since the early 1990s so that there may be a catchability effect.

In one year, large cod catches, in large aggregations, tended to have little food in stomachs, and less dense aggregations seemed to feed at higher levels, or capelin might avoid large aggregations of cod. From these data it is hard to see cod concentrating in areas of high capelin abundance. Capelin could in principle affect the distribution of cod without affecting their abundance. And there are other prey.

Partial and total fullness indices of cod seemed counter intuitive. Average stomach indices for all 2J3KL, declined in 2J when capelin disappeared but not in 3K until years later. There should be a weighting by set and fullness should drop in later years. Problem is looking at a snapshot. May be missing information in distribution and timing of movements which affect the view.

Eugene Murphy WP 3.1

A unit area view is provided because we may be moving toward providing advice on smaller areas.

Numbers for weight at are bumped up where catch weights and sample weights are known.

For most ages in the commercial catch, there are no RV weights; so, the numbers cannot be compared.

Can something be inferred about lack of larger fish in linetrawls? The selectivity in commercial gear seems to be getting narrower.

Shelton & Murphy WP 4.1

There are log books for vessels over 35 ft from statistics branch. But their main duty is monitoring the quotas, catch is the emphasis and effort less important, sometimes not punched.

Most (95% on NE coast) but not all gillnets use 25 meshes deep. The height of gear not asked in the logbook.

Logbooks include sentinel as well as commercial catches; they can be excluded from analyses if desired.

Hard to draw any conclusions about differences between catch rates in only two years, given the differences in the fisheries in each years (for example the allowed areas to fish were different)

Best catches are at the headlands, not in the bays.

If an index fishery is to be informative, it should be spread over time. But we won't get fishers to fish when there are no fish: it costs them money.

A lot of fishers will be able to provide more precise locations. The position field could be labelled, as in the tagging data, as a measurement or an estimate.

Logbooks and dockside monitoring tend to agree on the weights landed.

Buddying up (more gear used than reported) is one of the problems that management regulations create in the catch rate data. This will not be reported in logbooks.

There is more logbook data from before the moratorium, from which effort and catch rate could be inferred. This will be investigated further. Some data were presented in a new working paper, 4.2.

D. Stansbury et al. WP 5.2

The age distribution of sentinel catch rates is dominated by the selectivity of the gear; it is possible to discern that the 1992 year-class is somewhat stronger than its neighbours. Very few fish older than age 10 are being caught.

Why use an offset rather than actual catch rates? It is better way to represent catch rate data. Zero catch with little effort is a different story than zero catch with a lot of effort and this model better represents effort. It is not clear how to model soak time, therefore data was bounded at suggestion of the Rimouski working group. (saturation, fish scrubbing out etc. which may affect catch rates).

The catch rate pattern is as one would expect, similar to sentinel results, overall 1999 down compared to 1998, but at least for gillnet, not very different from 1997. The 1992 year-class tracks, 1993 doesn't, suggesting it's weak, 1994 doesn't appear to be much.

Should the output be put through a year-class model? It is difficult when selectivity confuses results so much and SPA is not available as a smoother for the data.

Control and Experimental gear catching same age-classes so should be grouped.

It is good to see a lot of information being derived from Sentinel data.

Gear-age interaction difficult to pull out of the data. Maybe it is better to do it by gear type for time being, run two models without removing gear effect. Can one implement a gear specific soak time unit as a covariate?

Catch rates are down, but is that an influence of a year-class? Is it a year effect? For example, water was warm inshore this year; how did that affect catches? One comment heard a lot was that fish weren't as close to the shore, were more spread out, water temps talked about a lot, trap catches were very poor.

Given fixed sites, is there any indication that fish changed depth distribution? It is difficult to say.

Noel Cadigan and John Brattey WP 9.1

ABSTRACT: In this paper we develop a model to estimate exploitation rates using tags returned from the commercial fishery for Atlantic cod in 3Ps and 3KL during 1997-1999. The tag-returns are from numerous tagging experiments conducted in 3Ps and 3KL during 1997-1999. Many of these experiments are described in Brattey et al. (1999) and Brattey (2000). Our model involves length- and gear-based exploitation rates for the commercial fishery and incorporates methods to estimate tagging mortality, tag loss, tag reporting rates, and growth. We use this model to estimate weekly exploitation rates for the cod fisheries off the south coast of Newfoundland (NAFO Subdivision 3Ps) and off the north coast of Newfoundland (inshore regions of NAFO Divisions 3L and 3K). However, we focus our inferences about exploitation in 3KL during the two time periods in which most of the commercial catch was taken: July and Sep 5-Oct 10.

Migration of cod causes problems when analyzing tag-returns, and usually leads to underestimation of exploitation rates. At present we have not been able to accommodate migration in our tagging model, so our estimates of exploitation rates will be too low. Our estimates are based only on tags returned from fish caught in the same geographic area in which they were tagged and released. Our estimates represent the fraction of the tagged population exploited by the fishery if there was no migration. With migration, the size of the tagged population in a region is less than the number of tagged fish released (even after discounting for tag loss, natural mortality, and previous fishing mortality), so the actual exploitation of the fishery is higher than the quantity we estimate. Nonetheless, we feel our results are reasonably accurate for 3K and the northern part of 3L where migration was low. Our results suggest that the exploitation rates in 3K and northern 3L during July were 19.4% and 2.3%, respectively. During Sep 5-Oct 10 the exploitation rate estimated in 3K was 13.5%, and in northern 3L the estimate was 3.8%. Using the reported landings during these two periods, we estimate that the biomass of fish in 3K was 9 000 and 11 000 tonnes in July and Sep 5-Oct 10, respectively. Biomass in northern 3L was estimated to be 50 000 and 42 000 tonnes in the same two time periods. Our results for southern 3L were considered too unreliable because of the smaller numbers of fish tagged and relatively extensive movements of fish between this region and 3Ps.

It might be informative to present a sequence of ever-more-complex models to help build intuition.

Tagging mortality is determined from caging experiments. It assumes tagging mortality occurs within a few weeks after tagging and doesn't include later mortality. In general, mortality was never worse than 20%. These fish had been held on surface in warm water. Most mortality from tagging is very low. There is less information on survival from trawl tagging experiments. Caging experiments could not account for tagged fish being more attractive to predators.

The model assumes exploitation is constant everywhere; clearly that may not be the case.

Was the caged mortality controlled? Untagged fish were included in some studies, but it is difficult to account for the effect of caging on mortality.

The tag reporting rate from 4R3Pn, where the programme is not well advertised, is estimated to be only 35%.

There is an assumption made that poorly placed tags will disappear within the first few weeks. There are plenty of data available for these estimates (40% of tagged fish contain double tags) and there have been 4000 returns with up to 3 years at liberty.

The large difference in exploitation rates in 3L is probably a migration effect or the result of mixing.

Breaking the timing of the season into smaller units has enabled a better estimation of exploitation. Last year's model tended to assume that fish released after/during the fishery were exploited. Breaking out the time series probably gives a better view of movements.

Natural mortality is fixed at 0.2 for the purposes of the model.

Captures in 3Ps may be affected by larger gillnet mesh size there.

It could be useful to take account of effort (e.g. from logbooks) in analysing tag returns.

Large changes in estimated biomass over a short time within a season may reflect fish movement.

Precision of estimates may be affected by lack of fishing (or only sentinel activity).

The model presented here considers only tags that are recaptured in areas that they were tagged in, movement out of the area is not considered. The biomass estimates are upper bounds of what was in the area.

Areas won't be managed on a weekly basis, so what is F for 1999? What proportion of fish were removed from the area by fishing? There is not enough information to answer that question.

Can one say all fish in 3KL are a population (inshore), so that if it goes up one place, it must go down in another, and thence estimate F for the whole population? What would you lose if you aggregated the areas to get a total exploitation? If you think that the same exploitations occur in the areas, you could get a total exploitation. If this data is to be used as an indication of exploitation, how would you use the overall exploitation.? One must use the rates in context of the time they are applicable. Movement of fish between areas creates problems.

What is a "least distorted view" of exploitation for the time period in question? If you assume these are closed populations, just add the exploitations. But when the size of the population being exploited changes over time, it is not easy to define either an exploitation rate or a population that represents the whole year without distortion. But it is desirable to do so – especially if the point is to convey information that will help non-specialists to make better decisions.

Some residual patterns helped to identify anomalous experiments that were removed; other curiosities are as yet unexplained.

There may be an influx of returns after the TACs are set, if people are afraid that returns will reduce TAC. But such a pattern is not observed in the return rates.

Subsequent re-analysis and discussion

Deeper analysis of caging experiments yields a tagging mortality of about 14%.

In July and September the biomass was below 10,000 tonnes in 3K; below 50,000 tonnes in 3L north and below 140,000 tonnes in 3L south. How much below cannot be determined from the analysis that does not consider outward migration. For southern 3L the outward migration was probably large, and hence the population was probably much below the 140,000 tonne limit; it may be misleading to report this number. Other indices did not corroborate it. There are separate biomass estimates for the two periods because the cod are migrating between different inshore areas. They are estimates of the population that remained in the area of tagging, not that part that left.

Even in 3Ps the commercial catch was 1700 tonnes, however, the number of tag returns was low. The low returns will affect the exploitable biomass estimate.

The southern 3L exploitable biomass upper limit was much larger than would be inferred from logbook data.

The population bounds could also be checked by looking at sentinel gillnet catch rates for the relevant peiods.

A re-analysis of the sentinel data showed that 3L south had very good catch rates in the first pulse of the 1999 fishery compared to other year; in the second pulse the 3L catch rates were lower than in other years. Sample size was low for these times.

John Pope : mark-recapture model including mixing

The new answer does not change the outcome.

Takes the 3K to 3Ps inshore and offshore tagging results for 1997 – 1999 with a correction for the immediate tagging mortalities, treating the two pulses of fishing as one. Estimates the raw exploitation percentage in 1999 by tagging group for each of the three years of tagging experiments. There are discrepancies in 3K in the earlier years, these may have been due to differential mortalities. Possibly the 1997 tagged fish in 3K were no longer susceptible to the gillnets. He did not correct for this as assumed a differential mortality by area. Each tagged fish has a characteristic amount of time in each area.

A fish tagged in 3K and caught in 3L would be a 3L mortality.

High mortalities in 3K high and 3Pn, lower in other areas. The proportion of time fish spend in each area was plotted within a pentagon. 3Ln fish spend all their time in 3L, 3K fish spend some time in 3Ln, the 3Ls fish spend a1/2 of their time in 3Ln rest of time in 3Ps offshore and inshore as well as 3Ln. 3Ps offshore fish spend a great deal of time in other areas, in particularly 3Ls and 3Ps inshore. The exploitation rates were similar to WP 9.1: high mortality in the north and low mortalities in 3Ls and 3Ps offshore. A sum of squares of residuals was plotted, there seemed to be a few high residuals and even though not thoroughly examined, there appeared to be more positives than negatives. He thought that the results indicated that the 3Ls fish were spending ½ their time in 3Ls and part in 3Ps and 3Ln. The problem in determining stock size components is that fish are immigrating into and emigrating from. Residence time was estimated for each area ie 17% of time the 3K fish are in 3K the rest of the time they are in 3L where mortalities were lower. Therefore, we have a problem by setting a TAC for individual areas, as fish may be double or triple counted. This model indicates higher mortalities in the north. This is corroborated by other studies.

These are quick calculations, based on reasonable assumptions but the behaviour of the model (e.g. its susceptibility to different kinds of error) has not been investigated in great detail.

If one analyses the two fishing pulses separately, would the confidence limits decrease? Probably it would not make a great deal of difference. The assumptions used would underestimate mortality.

George Rose WP 7.4

One can summarize this paper as: there was a noticeable 1994 spawning aggregation; none that large has been seen since. From that aggregation there was an initially promising year-class; but it disappeared.
There was almost no spawning in 1996.

D. Kulka- Skate

ABSTRACT: Countries of the European Union, particularly Spain have prosecuted a directed fishery for skate (*Raja* sp.) since the mid-1980's outside 200 miles on the Grand Banks. However, Canada established a limited directed fishery for skate only after the decline of traditional groundfish resources. Prior to 1994, skate in Canadian waters were taken only as bycatch, most of which were discarded. Since the establishment of a directed fishery for skate on the south-western Grand Bank and southern St. Pierre Bank Canadian landings have fluctuated around 3,000 t annually until 1999 when they fell below 1,000 t. This evaluation represents the fourth assessment of this relatively new fishery. It shows that biomass indices, following a decline to their lowest historic level in the early 1990's have stabilized and may have increased in recent years. However, a change in survey gear (in the fall of 1995) with different catch characteristics has created a discontinuity in the survey time series preventing a comparison between two periods, 1986-1994 and 1995 to date. The spring survey, previously used to estimate biomass and abundance may not have included a substantial portion of the population. On average, fall survey estimates of biomass for the comparable area (NAFO Divisions 3LNO) were 43% higher although only 7% in 1999. In addition, differences in spring and fall distributions and seasonal change in bycatch rates of skate from Grand Bank slope fisheries suggests that thorny skate migrate toward the shelf edge in the winter, a portion of the population out of the surveyed area. They disperse onto the bank in midsummer and throughout the fall. Analysis of lengths taken during the research surveys have covered a consistent range since 1985 with main modes occurring at 15 to 32 cm and 65 to 83 cm in both spring and fall, the latter mode comprising mature fish. Since 1996, a single mode in the 30-60 cm range (a mix of mature and immature fish) has been observed. A recent increase in proportion of larger skates in the catches is noted. A spatial analysis of skate size shows a degree of segregation by size, the largest fish tending to aggregate along the southwestern shelf edge with a mix of sizes in the bank aggregation to the north. A comparison of distribution of skate from surveys data with commercial grounds shows that the Canadian fleet fishes about 1/3rd of the area of high concentration of skate in the spring, primarily along the shelf edge where the skate are largest. The Spanish fleet fishes a separate area on the Southeast Shoal outside 200 miles in the fall taking smaller fish. A relationship of both skate density and size with temperature and depth was observed, denser concentrations, in particular larger fish, were associated with warmer, deeper locations.

Some of the statistics from Statistics Branch are product (e.g. wing) rather than species codes.

How reliable are the quota reports? They are supposed to be dockside monitored…
Are low values in early data (1985) are due to unreported catch? People didn't report skate catches then. If yellowtail, for example, is misreported as skate, then distributions may be misreported. But distributions shown are based on boardings.

Are Canadian catches on the edge because they are trying to avoid bycatch?

There appears to be a jump in smaller fish in 96 (first year of Campelen) which may indicate a gear effect.

There was a 3000 tonne quota and 9000 tonnes taken last year. The argument can be made that even so, the amount taken can be sustained. The relevance of the is to control Canadian catches.

Foreign catches are more likely to be over- than under-reported (because they actually caught yellowtail for example).

Discards are worked into the numbers.

What constitutes assessments on the status of the stock, and how does one present it to the FRCC?
The information presented here is used, and seems to suggest that the status quo is sustainable.

There need not be a high survival rate from discards given the depths they are taken from.

Lilly et al. WP 2.3

Although the evidence is compelling that condition in 3K decreased from 1988 to 1992, the trend in the early part of the time series is not as clear. The early time series was noisier due to less sampling.

At the time condition went down, liver condition went down in 2J but up in 3L.

There is no indication that inshore fish are in poor condition.

Is the increase in proportion mature in 1997 reason to be optimistic? Or is it noise or variation?

If one analyses the data by cohort, there is still variability, but not as much.

Shelton and Stansbury WP 6.3

Most of the recruitment indices have been collected only recently, for year-classes for which there is no accepted (e.g. SPA) measure of actual recruitment. It may be worth examining those indices that were measured for year-classes whose strengths we think we know. The Fleming survey used to be such an index, but is not available now. If there were a trusted index of recruitment, and if it received a low weighting, this might be reason to distrust the model and how it judges the indices.

Some indices are counting fish that are now mixed together, but which would later have separated into inshore and offshore recruits. So there is a danger that measures at different ages are measuring different things. All one can do for now is include them in the model and see how they fall out: if they are different, this may show up. If there is a difference between inshore and offshore recruitment, it should come out as a pattern in the residuals.

There are many pairs of indices that were never measured for a common year-class; they are calibrated only through a sequence of weakly-overlapping intermediaries. Such is life.

In previous looks at the Fleming survey, age 0 numbers appeared to predict age 1 numbers well. Why do they get such different weights here? Is the difference driven by one year?

It is fair to say our understanding of rankings versus reality is tenuous at best. The weighting measures only how well the indices agree among themselves. We don't understand what it means in terms of the link between the first principal component and reality. If you have five good indices and more noisy indices, how does it look?

The year-class signal doesn't change when one index is removed.

The 1992 doesn't show up as strong in the compromise index, although it has been prominent in the fishery. Perhaps the 1992 fish came from good recruitment in 3Ps.

Although the error bars around the 1999 year-class are large, they do not overlap with earlier ones. So the increase appears to be real, at least at age 0.

Later re-analysis and discussion

Looked separately at the 10 top-ranked indices and the other 18, to see if perhaps they were telling different but almost equally coherent stories (akin to the first and second principal components). There was no great difference among them. Nor does anything jump out when the individual scaled indices are plotted. Most showed a dip in 1996; but there is a general problem of having no really strong signal for the indices to track. Nor did disaggregating by age show much.

Part of the problem is that so many of the series are short and have little overlap.

Part of the value of age 0 indices is that their predictions are for recruitment many years in the future – the first look. The same feature would also make it the least reliable: indices at other ages can already take account of random events later in life.

Comments on individual indices: subjective indications of their quality:
Squid traps may have collected the same fish twice if there were along shore migrations. By tagging we would learn about migration of fish.

No recruitment index could pick up the 1992 year-class that has been strong in the sentinel catches. This tends to moderate our enthusiasm for the recruitment indices. Of course, that year-class may be 3Ps fish that would not be seen in 3KL as pre-recruits.

This work should be seen partly in the light of what the FRCC has said about recruitment . It talks about trends in pelagic and beach surveys showing that there is a slight increase in recruitment subsequent to 96.

There is no recruitment index going substantially further back in time unless one is willing to believe the Engls-to-Campelen conversions for fish younger than age 3.

Intrinsic reasons to like one index over another:
The 0-group survey has a 150 samples over a broad area from southern Labrador to the southern Grand Banks from the bays to the offshore. But it is many years (many potential accidents) prior to actual recruitment.

There was some reluctance to use low-weight indices at all; but in general it was accepted that they could do no harm if properly downweighted.

A modified capelin net would be good for sampling the youngest cod. If there were the money to add that to the sentinel program, that would provide good spatial coverage.

The analysis was performed without preconceived notions about accuracy. If we had prior variances to include then we could modify the analysis to take them into account; however, this exercise has the goal of agreement without a standard, if we do not use the model then we are left with indices without anything to pin raw data to.

In the end the meeting felt that, although the method could be criticized, it was an objective way of arriving at a compromise among several indices that ought to be related to year-class strength. But we are not yet totally confident of what it means. The 1999 year-class, in its first year, was definitely out of the ordinary for the 1990s (even with its large error bars). Need to examine more closely those data sets that are continuing; and perhaps explore the behaviour of the model with simulations. Modelling without understanding is a recipe for disaster.

Harvey Jarvis and Rick Stead WP 8.3

The comparison of sentinel and commercial CPUE came only from sentinel participants.

Sentinel and commercial activity were conducted in the same area. The seasonal catch rates were based on the entire season. The sentinel activity was broader and may not have been at the same time. Hence the peaks were not the same. A three day fishery might have sufficed to catch the commercial quota; this may account for some of the difference.

The low catch rate in sentinel traps in 1995 may have been due to the late start.

The 1999 season was short compared to traditional commercial seasons; it is not clear that the catch rates experienced during these short periods were sustainable. Perhaps the harvesters' questionnaire could invite comments on the reasons for catch rates. Inshore catch rates are higher now than the rates that caused concern in the late 1980s.

C. Howse-Best WP 8.4

ABSTRACT: The 1999 Inshore Sentinel Survey in the Fogo Island area collected data from four sites around the Island established in 1995, Seldom, Fogo/Deep Bay/Island Harbour, Joe Batt's Arm, and Tilting and a fifth site established in 1998, Shoal Bay. Cutbacks to the cod trap portion of the program enabled an increased amount of time and effort to be focused on the gillnet and trawl portion of the program. Having an effective cod trap fishery in the three remaining areas for such a reduced amount of fishing time (17 days total, half of previous years' time), proved to be difficult as both catches and lengths were low. Decreases in both average length and estimated cod trap catches were noted.

The small mesh gillnets (3.25") continued to be effective at catching smaller fish in the cod population around Fogo Island as last year and provided a more well rounded sample for analysis. Overall, the average length of cod caught by 5.5" gill nets showed a small increase over last years' values while the proportion of females in the catch decreased in all areas except Seldom.
Trends and patterns in the trawl fishery were not observed as fishing activity throughout all survey years was sporadic.

The most complete biological data currently being collected comes from the Fogo Island co-op sentinel fishery.

Todd Inkpen WP 3.2

The catch length frequencies and the landed length frequencies were similar. This indicated a low rate of highgrading. The gillnet and handline data indicate a decrease in mean size from July to Sept. There is a trend of increasing mean size from north to south.

Todd Inkpen WP 3.3

Looked at the periods 97 – 99 within the shrimp fishery. Under 65' vessels ~ 3 – 5 % observer coverage. The rates of discarding in terms of shrimp landings is very low. Roughly 2.3 tonnes of cod were discarded in 97. The bulk of the fish lost were from 2J.

The only way that there could be a problem with the shrimp fishery is if the unobserved boats are doing something different from the observed boats.

Some fish were discarded from gillnets if they had been in the water too long (could not be retrieved within 24 hours because of bad weather); but this was unlikely to be more than about 1%.

Peter said that the SSR should show that highgrading and bycatch is very low. Last year the FRCC used a factor of >25% for all unreported deaths due to human activity.

Logbook Catch Data

Tagging data estimate of biomass for 3K inshore north – south, 3L inshore south and north 2 pulses. There were not many fish in the north (3K inshore first and second pulses) hence lower catch rates and higher exploitation rates.

Peter wondered why, if the biomass was high in 3L, the catch rates were low. Answer the fishermen moved from 3L south to 3L north where biomass and catches were higher.

If there are fish that spend substantial amounts of time in each of 3Ps and southern 3L, there is a certain logic to allowing people to catch them in each area. But this is not an assessment issue. The question of whether the current stock boundaries are the most useful for management purposes could be an assessment issue and is worth some thought.

Mortalities from RV Surveys

Total mortalities (z) as indicated by numbers-at-age in RV surveys appear to be high in the offshore, about 0.77 (averaging over years and age groups) for the most recent years. Could z values applied to the offshore be applied to the inshore? Do we understand the dynamics of fish mortality? Can we extrapolate mortalities from the offshore?

Do we know that the offshore fish were dying and not simply moving inshore? The latter effect would appear to drive z upwards offshore and downwards inshore. But genetics do not indicate that this is the case.

What do we feel is causing the cod mortalities? Possible reasons include environment, seals etc.

There were very few reports of mortality due to seals in winter 1999-2000. There were some reports of cod possibly being trapped in cold water and dying.

60 million fish in the offshore sounds like a lot, far from "barren". But this must be compared with billions in the 1980s and earlier.

Raw mortalities, derived from the ration of numbers at successive ages in successive years, are easy to interpret but susceptible to noise. There is no clearly best method for smoothing them.

In the North Sea the catchability decreases hence the z values are decreasing. He feels that the behaviour could be changing. Since we have been cropping down the older fish, the younger ones could be changing behaviour earlier, hence our RV z values are over-estimated. Maybe we should set gill nets near rocks.
Gill nets were set in the Virgin Rocks and found pockets of fish.

Logbook Catch Rates Revisited

Median logbook catches for 3KL, agreed with what some fishermen reported last year. The 1998 and 1999 catches were greater than in 1988. Why, then, did 3L south fisherman move to 3L north? Because not all areas had high catch rates. Catch rates were high in 3L north and parts of 3L south (St. Mary's Bay) but low in other areas. It would be useful to indicate the amount of effort in terms of number of nets. The comparison is confounded by effort, present use of fish finders, use of individual quotas etc.

Fishermen felt that the technology has changed, possibly we should make use of no technology for the Sentinel Fishery data. But the Sentinel fishery makes use of a control site in fixed positions. Sounders are not used in setting a control net. Fishermen have indicated that sounders were not used when determining where to set experimental gear.

The present catch rates are not comparable with past catch rates. The SSR should indicate this lack of comparability and indicate why this is true.

The Possibility of Recovery

Although this meeting is to assess the stock and not to make management (e.g. quota) recommendations, it is taking place in a context of precautionary management. For example NAFO and instructions from the ADM Science. If we have valid scientific grounds for evaluating what would be cautious, it would be remiss of the meeting not to convey them to people charged with making decisions.

At the same time, there are no firm rules under which we make decisions about what is cautious.

We do not know if the inshore stocks are now in a rebuilding stage, or if they are as high as they are going to get. Or if they can go higher but only as long as there is no interference from offshore fish migrating inshore. We can say that there has been an increase in exploitable inshore biomass recently. The stock without an offshore component has not existed for long enough to know what rules it plays by. But we at least suspect that there is room for more than one inshore aggregation – that there is potential for inshore expansion. It would be cautious to assume that such expansion is possible (targets are often set in terms of MSY so we wish to determine what that is); but if we are wrong how long are people prepared to wait for an expansion that is not going to happen?

Perhaps we should talk about recovery on the whole, and not recovery of the offshore and inshore separately.

All stakeholders are looking for signs of recovery; is there a clear agreement about what would constitute such signs? It would be useful for all parties to know what would be counted as evidence.

If inshore cod are growing out of the size range where the gear can catch them, then (at least in the absence of a change in gear or in fishing practices) this would afford some protection for the inshore spawning component.

Given our lack of ability to predict recruitment, we may never be predict rebuilding of the stock.

The inshore data do not show trends in any reliable abundance index.

There may be weak recruitment in the future because there are no strong year classes subsequent to the 92 year class.

The feeling of many fishermen that the stocks are improving should be recorded in the industry perspectives section of the SSR.

We cannot do a meaningful sequential population analysis (“analytical assessment”) with the data currently available: there are not enough years of information to make a calibration possible.

If old fish are not appearing in Sentinel catches, then either: 1) they are beyond the catchability of the gear and even though they are still abundant we are not catching them, or 2) they are now dead or, 3) they have moved out of the area.

Informal Risk Assessment and Advice-Like Statements

The FRCC would like to see evidence suggesting that the TAC should increase, decrease or stay the same. The options whose risks we might assess include increasing the TAC, leaving it at the current 9000 t level (which is felt to be the minimum that is worth the trouble of gearing up for), reducing it to an index fishery for information, or a complete moratorium. The bad events whose risks under these options we wish to know might include the spawning stock declining; the stock being below B_{lim} (except that we do not know what that is); exceeding a 20% exploitation rate; wiping out small subpopulations; reducing the chance of offshore recovery.

There is a small risk that the SSB will decrease even if there is no fishing, and the risk increases as fishing increases. Decision-makers may, of course, decide to accept such risks.

A good starting point for the discussion is what happened last year with the 9000 t TAC and 8600 t catch.

Probably the SSB will not increase with any of the proposed fishing options.

If catch, regulated by individual quota, is not distributed as the population is, then there is added risk of disrupting populations outside areas of major aggregation.

It is possible that the migration out of 3Ps into southern 3L might not happen in some year. In this case a fixed quota for southern 3L would remove more of the resident stock than was intended. The risk of having a >20% exploitation rate increases in southern 3L with decreasing migration. Levels of migration from 3Ps can not be predicted.

We have no evidence of signs of hope in the offshore, which is where the 100,000 tons in the inshore fishery came from. The potential risk is that the offshore recovery will be due to expansion of the inshore fishery. There is the possibility that there could be no fishery in the inshore and there would be no rebuilding in the offshore. Alternatively, we could destroy the inshore and the offshore will recover anyway. The 150,000 t within the inshore fishery came from the offshore migrations. We will never get a 150,000 t fishery from the inshore fishery alone.

We have not exhausted possible ways to learn more from the Sentinel data.

The Smith Sound acoustic biomass has remained at roughly 22,000 t over the last few years. That might indicate stability of the whole stock; equally well, it might mean that Smith Sound is a preferred area and the first 20,000 t that can get in there will, perhaps leaving the rest of the inshore depleted. Though the fish in Smith Sound do not appear to be packed as tightly as possible.

We have no idea of how many fish are taken by the recreational fishery.

The limit of 20% exploitation could be applied to the whole inshore stock; or, more restrictively, it could be applied to each subcomponent. Though this might have undesirable consequences if it led to effort concentrating on the largest components – which might be the best hope for recovery or expansion.

The analysis of tagging data is an ongoing research project. This means that the message being presented differs from one year to the next, which is unsettling for people who have not followed the details of the developing project.

Tagging analysis models can also be used to determine the most informative places to tag.

Though historic biomasses may be used as a guide, the history of northern cod has been relatively short. We do not have knowledge of acceptable levels of biomass, therefore we should have only small fisheries in the inshore.

A fisherman noted that when the fishery was healthy they would capture fish in 3K that were tagged in 3Ps. So during healthy times there is mixing, or at least a higher number of captures to collect statistics on rare events.

Abundance – tagging estimates of exploitation rate (i.e., something constructed to be an underestimate of exploitation and overestimate of numbers, but by an unknown amount – this is nothing like a confidence limit) and biomass – pulse 1 3K exploitation 19%, biomass 8,900 t; pulse 2 exploitation 13%, biomass 11,000 t. these are two estimates of the same thing and therefore are not additive 3L inshore north, pulse 1 exploitation rate 2.3%, biomass 49,000 t, pulse 2 3.8%, biomass 42,000 t

Acoustic Estimates

3 Rose, Wheeler and Anderson estimates. All have different adjustment factors that make them not directly comparable with each other.

January 2000 estimates from Smith Sound 22,000 t (Rose et al.) - no other aggregations anywhere near this magnitude exist in the area surveyed (most deep water inlets from southern Bonavista Bay to Notre Dame Bay)– the 1990 and 1992 year classes were still present in relatively large numbers and the yearclasses of 95, 96 and 97 yearclasses were well represented.

Trinity Bay/Bonavista Bay acoustic estimate November (Wheeler) 5,100 t (not corrected for detectability) – 2,700 t in Smith Sound – 3,350 t in Trinity Bay compared with 1,530 t in 1996, 1,750 t in Bonavista Bay compared with 4,040 t in 1996. Size range peaked at 62 – 72 cm when fishing with feathered hooks and jiggers which would underestimate smaller fish. Due to fishing methods we will have biased length distributions. Wheeler's study it biased toward large fish whereas Anderson's was biased toward juveniles.

Southern Bonavista Nov/Dec. 1999 (Anderson et al.) at much higher resolution – did not encounter any large concentrations of cod in the area surveyed.

If we could get ages of fish from these aggregations what we do with them? Determine if there is a broad age structure or are we looking at only one year-class?

Indices

Sentinel age-disaggregated index: the 1992 year-class is clearly seen but all subsequent year classes are substantially smaller.

It would be useful to present variances. There are covariances so the variances are not independent (age within year and site) and are therefore not additive. Thus variances would overestimate error bars.

Age aggregated estimates showed increases from 95 to 98 for most gear types – but the 1999 catch rate is lower than the 1998 estimates in all gear types.

Spatial patterns from the logbooks in 2 years. The gillnet catch rates indicate lower catch rates in 3K, higher in 3L north, low again in 3Ls and higher in St. Mary's Bay.

Catch rates from White Bay north were low in 1998 and 1999, they increase in Notre Dame Bay eastward to Fogo island and then higher catch rates from Cape Freels, Bonavista Bay and the western side of Trinity Bay. Low in the south, but increasing again in Cape St. Mary's.

There was a concentration of effort in the lower portion of 3K because the of fishing regulation restrictions. The fishermen were not allowed to move into 3L north. The fishermen had to fish hard to get their IQ. There were low catch rates, as well as a high number of fishermen in the area.

We can not draw inferences about trends from two years of data.

Commercial Catch

No evidence of substantial unreported deaths due to fishing.

Given the problems introduced by the selectivity of the gear, only the 1992 year class can be distinguished as stronger than average.

The inshore fishery catches few fish older than age 10 (1989/90 and previous year classes). The 1990 year class is getting weaker.

Recruitment

0-group is substantially higher in 1999 but the relationship of 0-group surveys with subsequent recruitment to the spawner population remains uncertain (the 1994 year class was not relatively strong at older ages in the inshore but was seen in Hawke and RV offshore and is now gone).

Combined indices from 11 surveys were modelled and the result showed that the 1998 and 1999 year classes were higher than those from 1990 to 1997 but with considerable uncertainty. The 1996 year class appeared to be particularly weak.

Alternative Options for 2000 and Associated Risk to Inshore and Offshore

Short-term outlook for the SSB is downward if you look at the recruitment subsequent to 1992 – longer term depends on recent recruitment (1999 and 1998 year classes)

Exploitation rate went up in 1999 compared with 1998 with the doubling of the TAC

Exploitable biomass in the inshore from Trinity Bay north is less tha the average of the pulses for the two boxes.

It is more informative to put boundaries between subregions at the heads of bays, where historically there was little fishery, rather than at the headlands.

The tagging estimate for South of Trinity Bay was not accepted (because catch rates were low in the sentinel and commercial catches) – we know that there are seasonal movements of 3Ps fish into this area.

Based on the low estimates of biomass inshore and the lack of any recovery of the spawner biomass maintaining the current catch is likely to lead to an increase in fishing mortality.

In an index fishery, a wide spread of catches would be useful for recovering information from tagging studies; but this goal conflicts with the desire to reduce risk of high exploitation in 3K.

Offshore

All we have is the RV and Rose – Hawke Channel (nothing substantial in Hawke since 1994)
RV increase 1999 compared with 1998 across many ages in 3K and 3L but remains historically low compared with historic levels.
RV catches low in 2J in 1999 compared with 1998

no large catches in the RV offshore since 1993
60 million fish (29,000 t) – swept area estimate – extremely low but biological extinction not about to occur.

There were very recent reports of seals eating cod in Bonavista Bay. Much reduced obvious predation in relation to 98.

Sentinel catch rates that were low compared to others in 1999 would still have been considered moderate in the 1980s. But it is not clear how comparable the two series are, considering the low level of Sentinel effort and changed technology.

How can we determine the seasonal migratory contribution of 3Ps fish into southern Division 3L so that biomass estimates could be determined? Look at tagging returns and complete a migration model, but it would take a lot of time for peer review.

There were changes in water temperature in 1999. This may have modified behaviour and had an effect on catch rates.

Because the most abundant year-classes are outgrowing the gear, age-aggregated catch rates are a less useful indicator of abundance than age-disaggregated ones.

Does an index fishery have to be regulated by quota? Would other regulations yield as much scientific information with less risk?

“However, it would be sensible to maintain low levels of fishing mortality in all areas of this stock and to avoid fishing concentration on any component of the stock.”

APPENDIX 1: LIST OF WORKING PAPERS

1.1 R. Kent Smedbol and J.S. Wroblewski. Metapopulation theory and northern cod population structure: interdependency of subpopulations in recovery of a groundfish population.

1.2 John Brattey. Stock structure and seasonal movements of Atlantic cod in NAFO Divs 3KL inferred from recent tagging experiments.

1.3 Terry D. Beacham, John Brattey et al. Population structure of Atlantic cod in the Newfoundland and Labrador area determined from genetic variation.

1.4 Terry Beacham, Robert S. Gregory and John Brattey. Origins of two recruitment pulses of 0-group Atlantic cod in Bonavista Bay during 1999, determined from genetic variation.

1.5 George A. Rose and D.R. Robichaud. Spawning site fidelity in Atlantic cod.

1.6 Richard L. O'Driscoll, George Rose, John Anderson and Fran Mowbray. Spatial association between cod and capelin: a perspective on the inshore-offshore dichotomy.

1.7 John M. Green, The resident population of Atlantic cod in Gilbert Bay, Labrador.

1.8 George Lilly, Eugene Murphy and Mark Simpson. Distribution and abundance of demersal juvenile cod in 2J3KLNOP: implications for stock identity and monitoring.

2.1 Noel Cadigan and John Brattey. Estimation of cod growth in 3Ps and 3KL in 1997-1999 from tagging experiments.

2.2 M. Joanne Morgan. Estimating SSB in 2J3KL cod using a cohort maturation model and variable sex ratio

2.3 G.R. Lilly et al. RV survey data: size-at-age, condition and maturity

3.1 Eugene Murphy. Catch of 2J3KL cod from 1959 to 1999.

3.2 Todd Inkpen and David W. Kulka. A description of the 1999 commercial cod fishery in 2J3KL from port sampling and fishery observer records.

3.3 Todd Inkpen and David Kulka. Discarding of cod in the northern shrimp and directed cod fisheries in 2J3KL during 1997-1999.

4.1 Peter A. Shelton and Eugene F. Murphy. Catch rate data from logbooks for the less-than-35ft sector.

4.2 R. Stead. A sample of historic catch rates calculated from port sampling records.

5.1 G.R. Lilly et al. Some research vessel survey information (unaged)

6.1 Edgar L. Dalley, John T. Anderson and Denise J. Davies. Year-class strength of northern cod (2J3KL) and southern Grand Banks cod (3NO) estimated from the pelagic juvenile fish survey in 1999.

6.2 Robert S. Gregory, David A. Methven et al. Relative strength of the 1999 year-class from nearshore surveys of demersal age 0 Atlantic cod in 3KL and in Newman Sound, Bonavista Bay.

6.3 Peter A. Shelton and Don E. Stansbury. Northern cod recruitment before, during and after the collapse.

7.1 John P. Wheeler. Distribution and abundance of Atlantic cod from an acoustic survey of Bonavista Bay - Trinity Bay during the fall of 1999.

7.2 G.A. Rose. Two-stage acoustic survey of NE Newfoundland coast from Trinity Bay to Green Bay, January 2000.

7.3 G.A. Rose. Update on the Smith Sound cod.

7.4 G.A. Rose. Update on the Hawke Channel cod.

7.5 John Anderson and Edgar Dalley. Trawl and acoustic survey in southern Bonavista Bay - observations on demersal distribution of Atlantic cod.

8.1 D. Maddock Parsons, R. Stead, D. Stansbury. 1999 sentinel survey in 2J3KL.

8.2 D. E. Stansbury, D. Maddock Parsons and P.A. Shelton. An age-disaggregated index from the sentinel program for cod in 2J3KL.

8.3 H. Jarvis and R. Stead. Results of the 1999 fish harvesters' committees questionnaires and a comparison of sentinel and commercial catch rates in 2J3KL.

8.4 Fogo Island Co-operative Society. Inshore sentinel survey: Fogo Island, 1999.

8.5 D. Maddock Parsons. Sentinel catch rates during two pulses of commercial fishing in 1999.

9.1 Noel Cadigan and John Brattey. Lower bounds on the exploitation of cod in 3Ps and 3KL in 1997-1999 from tagging experiments.

11.1 George Lilly and Mark Simpson. Distribution and abundance of capelin, Arctic cod and sand lance on the Northeast Newfoundland Shelf and Grand Bank as deduced from bottom-trawl surveys.

Newfoundland Region

Proceedings of the Newfoundland Regional Advisory Process for 2J3KL Cod and 3LNO Skate March 2000

Sign-off Checklist

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