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# Proceedings of the Annual Meeting of the National Marine Mammal Peer Review Committee (NMMPRC)

**October 20-24, 2014**
**Ottawa, ON**

**Chairperson: Don Bowen**
**Editor: Christine Abraham**

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## Foreword

The purpose of these Proceedings is to document the activities and key discussions of the meeting. The Proceedings may include research recommendations, uncertainties, and the rationale for decisions made during the meeting. Proceedings may also document when data, analyses or interpretations were reviewed and rejected on scientific grounds, including the reason(s) for rejection. As such, interpretations and opinions presented in this report individually may be factually incorrect or misleading, but are included to record as faithfully as possible what was considered at the meeting. No statements are to be taken as reflecting the conclusions of the meeting unless they are clearly identified as such. Moreover, further review may result in a change of conclusions where additional information was identified as relevant to the topics being considered, but not available in the timeframe of the meeting. In the rare case when there are formal dissenting views, these are also archived as Annexes to the Proceedings.

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## SUMMARY

The National Marine Mammal Peer Review Committee (NMMPRC) holds at least one annual meeting to conduct scientific peer-review of marine mammal issues. This approach provides the opportunity to bring together experts on marine mammals from Fisheries and Oceans Canada (DFO) with specific contributions from non-DFO experts to ensure high quality review of the scientific results and to provide sound scientific advice as the basis for the management and conservation of marine mammals in Canada. When time permits, this annual meeting is also an opportunity to review ongoing research projects and provide feedback or guidance to the scientists involved. In addition to these Proceedings, several Research Documents and Science Advisory Reports will be published as a result of the meeting.

The meeting was held at the Delta Hotel (Ottawa, ON) from October 20 – 24, 2014. The participants invited to this meeting included individuals from DFO (Ecosystems and Oceans Science, Ecosystems and Fisheries Management), Greenland Institute of Natural Resources, St. Andrews University, Scotland, National Oceanic and Atmospheric Administration (NOAA), U.S. Geological Survey (USGS), Nunavut Wildlife Management Board, Nunavik Marine Region Wildlife Board, Makivik Corporation and Nunavut Tunngavik Inc.

## SOMMAIRE

Chaque année, le Comité national d'examen par les pairs sur les mammifères marins (CNEPMM) organise au moins une réunion où l'on procède à un examen scientifique entre pairs de questions touchant les mammifères marins. Cette approche donne l'occasion de réunir des experts sur les mammifères marins de Pêches et Océans Canada (MPO) ainsi que des experts de l'extérieur du MPO, ce qui permet d'assurer un examen de qualité supérieure des résultats scientifiques et d'offrir un fondement scientifique solide en vue de la gestion et de la conservation des mammifères marins au Canada. Lorsque le temps le permet, les participants à cette réunion en profitent également pour se pencher sur les projets de recherche en cours et formuler des commentaires ou des conseils aux scientifiques qui y prennent part. En plus de ce compte rendu, plusieurs documents de recherche et avis scientifiques seront publiés à la suite de la réunion.

La réunion a eu lieu à l'hôtel Delta d'Ottawa (Ontario), du 20 au 24 octobre 2014. Les participants invités à cette réunion comprenaient des personnes du MPO (Sciences des écosystèmes et des océans, Gestion des écosystèmes et des pêches), du Greenland Institute of Natural Resources (St. Andrews University, Écosse), du National Oceanic and Atmospheric Administration (NOAA), du United States Geological Survey (USGS), du Conseil de gestion des ressources fauniques du Nunavut, du Conseil de gestion des ressources fauniques de la région marine du Nunavik, de la société Makivik et de Nunavut Tunngavik Inc.

## WORKING PAPER 1A: INSTANTANEOUS AVAILABILITY BIAS ESTIMATION FOR CALCULATING AERIAL SURVEY ABUNDANCE ESTIMATES FOR NARWHAL (*MONODON MONOCEROS*) IN THE CANADIAN HIGH ARCTIC

*Cortney A. Watt, Natalie C. Asselin, Jack R. Orr, and Steven H. Ferguson*

*Rapporteur: Jack Lawson*

Discussion: The Committee asked whether the presence of predators, such as killer whales, were the source for the apparent narwhal dive behaviour variability (e.g., did the authors do an analysis of killer whale tagging data in 2013 when there were also tagged narwhal in the study area)? The authors felt that the presence of killer whales might influence narwhal distribution (which seemed different than normal in 2013) and perhaps swimming behaviour.

In light of the author's decisions to analyze dive data in 0-1 and 0-2 metre depth bins, the Committee also inquired about the precision of the vertical depth data collected by the satellite tags. The authors responded that this depends on the tag model and that the tag software allows you to scale this precision; the authors agreed to add a description of the tag dive data precision in the paper (Wildlife Computers has indicated that precision of these tags is 0.5 m within the 0 to 1,000 metre depth range).

The Committee concluded that the average time spent in the 0-2 metre depth bin (31.3%) is not based only on data from the daylight period, and this is not explicitly stated in this paper (although it is in the equivalent bias estimation paper for the bowheads), and should be. The comparison of day and night diving data - when the survey was only conducted during the day - inflated the sample size but for a non-applicable sampling period. The Committee recommended that during analyses the authors use only satellite telemetry data that corresponds to the 2013 HACS survey period (e.g., no night dive information) since it would not change the results much, but would be more logical.

The Committee asked the authors why they used weighted averages in the analyses as this would artificially reduce inter-animal variation; the Committee felt that it would be better to use non-weighted averages to not underestimate individual variability of diving behaviour. The authors responded that they did not want narwhal that had contributed a large proportion of the telemetry data to overwhelm the analyses, and that their analysis was more likely to overestimate individual variability.

The Committee felt that the paper's modeling of dive bin proportions is not best done with ANOVAs, but rather with a beta regression approach; this latter approach would likely capture variability better. Further, the Committee recommended that the authors investigate the distribution of the errors rather than the depth mean data. Since this approach will affect the results, the Committee recommended that the authors include this in these analyses.

The Committee suggested that although the paper reported using 1,194 narwhal "samples" in the analyses, these data should instead be considered as just 24 narwhal samples as there is autocorrelation amongst the narwhal providing the data – in effect, the observations are not independent. Because of this, the Committee felt that the standard error estimates should be based on an N of 24 independent samples (not 1,194). The authors responded that the covariate analysis employed the weighted means with an N of 24, and using an averaging of the 24 mean values only will likely give a similar correction factor to what is presented based on the 1,194-sample average. This is captured in analyses by including the random variable "whale".

The Committee suggested that the analysis of a variety of environmental variable effects on time spent in the depth bins seems to be limited in that the authors used only one correction that does not account for all of these effects. The authors responded that there were no significant effects for most of the variables, but would re-visit this conclusion by way of including more detail in the Methods section on how the analysis was done (e.g., how were the location data used for each time bin?). The Committee suggested that the authors could simplify the paper by just looking at environmentally-related sources of variation, and leave the behavioural analyses to another paper. The authors will consider this.

The Committee asked that, since the size of the narwhal did turn out to be a significant covariate in the analyses, why didn't the authors use this in the analysis if there were some photogrammetry information available? The authors responded was that they have not yet read the photographs to obtain narwhal size measurements. This would add at least a month to the time for the analysis. Further, the authors suggested that the behaviour of smaller whales are only different in the 0-1m bin but not the 0-2m bin, but could not say why. The Committee recommended that the authors consider a third analysis group of "mothers with calves".

The Committee suggested that the study may not have sampled sufficiently to determine the age structure of the narwhal population so this may have an effect on the availability bias; they recommended that the authors add a better description of the sample.

The Committee asked the authors why they assume that the narwhals' behaviour was not affected by the survey aircraft overflying the whales at 1,000 feet survey altitude. The authors responded that to assess this possible response would require an interesting, but separate, study. It is unlikely that survey aircraft overflew tagged narwhal in 2013 so it is unlikely the satellite tag data could be used to assess this.

The Committee recommended that the SAR only look at the availability correction factor. They further recommended that the authors change the title of this paper to "Factors affecting near surface behaviour and estimation of an availability bias correction factor".

The Committee recommended that the authors express results with one decimal place for precision, not two or three as in the current version of the paper.

The Committee asked the authors if their single correction factor approach to the abundance estimation correction could be supplanted by a survey analysis that employs the known influential covariates (such as depth and distance to shore) to post-stratify the study area (e.g., fiords versus non-fiords)? The authors responded that that this type of post-stratification was possible and might result in lower CVs. The Committee recommended that the authors also present more detail on how the location of dive data samples was incorporated in the analyses (e.g., slope and depth).

The Committee would like to see the analysis incorporate a correction factor that relates to day and survey window and appropriate depths, whereas the current analysis may or may not have provided that (with the aforementioned suggestions for better methodology descriptions this will help); an analysis that looks at factors that more broadly affect the surface behaviour of this species could be done. In response, the authors felt that unless the Committee could suggest a good reason to stratify (which they did not), the authors think the correction factor as currently calculated is representative of what was affecting the availability of narwhal.

## WORKING PAPER 1B: IDENTIFICATION OF DUPLICATE SIGHTINGS FROM THE 2013 DOUBLE-PLATFORM HIGH ARCTIC CETACEAN SURVEY

*D. Pike, T. Doniol-Valcroze*

*Rapporteur: Nell den Heyer*

Discussion: Discussions explored the impacts of whale aggregation, ice, and weather conditions on the identification of duplicate sightings between observers and between observers and photographs. Illustrations helped to demonstrate the difficulty in identifying pairs of replicate sightings when there are many potential pairs (e.g. aggregation of whales in clusters of similar sized groups). The defining of pairs is exacerbated by degraded data when there are a lot of whales. It was noted that the problem of identifying duplicates is common to other surveys, and that some other surveys use video, not photographs as an alternate. The possibility of using photographs instead of the observer counts was discussed, but photographs have other problems including more limited area swept and being instantaneous, thus may have lower detection.

It was noted that not incorporating the mark-recapture data would underestimate the population, and this bias was considered unappealing because we know there are animals missed by the observers.

The proposed logistic regression method is data driven and informs the weight given to the characteristics of the sightings that are associated with duplicates. The advantages were reviewed: First, where Southwell (chosen as candidate method from literature review) saw inflections in the data that indicated what thresholds defined duplicates, unfortunately the HACS data was not so prescriptive. Second, the Southwell method weighted all criteria equally. The new method allows the data from this survey to define weighting. The proposed method is also more likely to assign duplicates (i.e., be more conservative) when data are missing.

It was noted that the observer bubbles on the plane may have contributed to the reduced sightings close to the track line. It was suggested that it may be worth exploring estimated distance instead of (or in addition to) declination as the variance will be different. However, a case was made that the error should be in the value observed and recorded.

A number of questions focused on assessing the impact on the estimate of abundance of the new method of estimating duplicate sightings. It was suggested that the estimate be compared to an estimate that used the same method as the previous (Richard) approach and/or that a sensitivity analysis be done. The previous analysis had subjective identification of duplicate pairs and was hard to replicate; it also had a type of adaptive approach, where photographs were used at high densities. As the adaptive approach was not part of the survey design, it is difficult to do this in a statistical framework post hoc. The Committee agreed that sensitivity analysis would be valuable to assess the impact on the abundance estimate of changing the threshold for the identification of duplicate pairs.

## WORKING PAPER 1C: SPATIAL MODELLING OF NARWHAL DENSITY IN BAFFIN BAY FIORDS DURING THE 2013 HIGH ARCTIC CETACEN SURVEY (HACS)

*T. Doniol-Valcroze, J.-F. Gosselin, D. Pike, J. Lawson*

*Rapporteur: Nell den Heyer*

Discussion: It was noted that in some surveys, turbidity can help improve detections if the background colour provides contrast. For the HACS, the observers (many of whom were at the peer-review) felt that the turbidity reduced detection and did not add contrast.

There was a question about post-stratification; specifically dividing the Baffin Island strata into south and north because the south had few whales, while the north had many. It was pointed out that the low counts in the south are in the estimate of mean, and in theory the mean should not change much with post-stratification (although here, because southern fiords have smaller area, it might), but post-stratification would reduce variance estimate. Best practice is to consider post stratification only based on additional information, not on the counts. Notably, traditional knowledge would suggest that there are concentrations of narwhal in the south (Home bay #16 on the map), suggesting that the stratification scheme used was appropriate. This will be included in discussions for the design of the next survey.

A question was asked about the impact of the choice of the stratification scheme - weighting based on area would under-sample small fiords. What are the implications of this? For example, are whales as likely to be in small fiords as large fiords? A number of covariates, fiord depth/width/shoreline length/complexity could be explored. The authors replied that there may not be enough survey data to tackle this question, but telemetry data could also be used to explore if narwhals spend more time in fiords with different characteristics, although at this point it too may be lacking.

Discussion also explored observer bias and detection in fiords. It was noted that in fiords there was high aggregation, some high turbidity, and the detection function may be mis-identified because of the vertical walls of the fiords. The idea of using photographs for counts was discussed. Notably, because of truncated detection function (by fiord walls), the photographs provide less data loss relative to effective sampling width of observers.

In conclusion, the spatial density approach was accepted by the Committee, and the value of the variance estimate was acknowledged. The use of photographs was seen as promising method based on the spatial characteristics of the fiords, and it was suggested that the results of both methods be compared.

## WORKING PAPER 1D: ABUNDANCE ESTIMATES OF BAFFIN BAY NARWHAL STOCKS IN CANADIAN WATERS BASED ON THE 2013 HIGH ARCTIC CETACEAN SURVEY

*T. Doniol-Valcroze, J.-F. Gosselin, D. Pike, J. Lawson*

*Rapporteur: Linda Nichol*

Discussion: There was some discussion regarding cluster sizes. The authors tested for the significance of a linear relationship between the log of the cluster size against the probability of detection (i.e., can larger groups be detected farther away than small ones?). If significant, they use the group size predicted by the linear regression, if not they use the mean cluster size for that stratum (only one stratum had a significant relationship).

There was also a question regarding the point-independent method and the apex of the gamma detection function. Standard point independent methods assume that the detection curves of the two observers are only independent at the trackline where the probability of detection is usually highest. However, because the authors used a gamma curve, where the highest probability of detection was not at the trackline (distance zero) but rather at the apex of the gamma curve (about 200 m from the plane), and that is where the point independence is assumed.

It was suggested that the authors add a table of the detection models that were tested and the AICs. It was also suggested to limit extrapolation of density in the Smith Sound stratum (which was not covered completely) to the area that was surveyed and revise the analysis accordingly.

The Committee also asked for further explanation for why the function did not left truncate, and to include additional explanation regarding clumping (e.g. clumping is often incorrectly considered to be associated with lower estimates). Clumping creates more variation among transect lines, and therefore more variable encounter rates. The resulting variance is higher, and is captured in the CV. The authors justified why no left truncation – there were many sightings near the trackline (~100m), so truncation would have been an arbitrary decision resulting in loss of data. Newer modeling tools support using a better model than just the ones in the Distance software (i.e. gamma function).

The Committee requested adding a table showing the values for each of the stocks for each of the PBR input values. Due to large uncertainty about stock structure and movement, there was a suggestion for Jones and Smith Sounds to use a smaller $F_R$ because there is uncertainty that is not captured in $N_{min}$ – rather than conservation concern per se. The authors note that a similar justification was used in the past on a bowhead whale PBR. There was a suggestion to provide both PBR results (with $F_R$ 1 and $F_R$ 0.5) as advice to management. However, the authors provided justification for using $F_R$ 0.5 – it is not arbitrary. Wade's paper, where he tests the $F_R$ values, shows that Fr of 0.5 is robust to various sources of uncertainty other than around the abundance estimate (e.g. stock structure). The Wade paper makes a good case for the $F_R = 0.5$ value. The question then is why use $F_R = 1$ for the other stocks? The authors replied that the main sources of uncertainty (for the rest of the stocks) come from the abundance estimate and thus are captured in the CVs (of the estimates) and then in the PBR (by using the $N_{min}$). Whereas for Smith and Jones Sounds the uncertainty is related to stock structure and movement, so these two elements are not captured by the CVs of estimates or the $N_{min}$.

There was some discussion related to the final abundance estimates (see Table 3 in document) and CVs. As presented one cannot see the contribution of variance from the different steps, so it was suggested to include a sensitivity analysis to illustrate the contribution of these variances to each stratum estimate. The authors agreed to do a sensitivity analysis on the thresholds used to identify duplicate sightings based on yesterday's discussion, and further noted that most of the variance comes from the encounter rates.

There was a request to address the stratum in Lancaster Sound – was it dropped from the analysis, and if so, why? The authors replied that observations were made during a training flight, and were not concerned that they did not include these in the analysis because these are animals that are likely transiting and satellite tag data suggest that the vast majority were captured in the surveyed strata on either side of Somerset Island. The authors will clarify this in the discussion.

There was another suggestion to add the Detection Functions for each observer to the paper. The authors replied that they were very similar – however, observers differed in the way they recorded data on large groups. But it is the average Detection Function that really matters. Pooling across 15 observers with different experience did have an effect, not at the Detection Function level but rather in the way groups were counted or recorded, making identification of duplicates difficult. Text will be added to clarify this in the paper.

It was noted that the numbers in this paper are not the final numbers, as there will be some additional analysis (e.g., availability bias estimates, sensitivity analysis and also a recalculation for Smith Sound).  So for now we are reviewing the methods framework – not the final number.

## WORKING PAPER 2A: INSTANTANEOUS AVAILABILITY BIAS CORRECTION FOR CALCULATING AERIAL SURVEY ABUNDANCE ESTIMATES FOR BOWHEAD WHALES (*BALAENA MYSTICETUS*) IN THE CANADIAN HIGH ARCTIC

*Cortney A. Watt, Bernard Leblanc, and Steven H. Ferguson*

*Rapporteur: Linda Nichol*

Discussion: There was a question regarding the uncertainty in the estimates for the depth bin included.  Because there is uncertainty in the 4m bin as to whether this is the best estimate of maximum depth, it was suggested the authors should use the variance of the 3m and 6m bins to account for the uncertainty. Photos could be used to estimate the actual depth below surface that whales are seen. The authors replied that in the satellite tagging data in the area where animals were diving deeply there seemed to be no difference between depth bins, but this was not so in other areas so they may require different corrections for different areas.

The percent of time in various bins was about 25%.  Because they are relatively equal, the authors could give them a number (0 to 4) combining all August estimates and all depths and then address concerns about the variance. Location appears to be a very significant factor. The authors have used to the two-date range estimates as corrections in the population abundance estimate (e.g. mid versus late August) and applied those to the survey data accordingly. However is true that area differences were bigger than the difference by dates. So they could certainly recalculate the availability correction by area.

If the authors are not sure which bin to use, then this should be reflected in the correction factor (the CV of the correction factor). So perhaps the authors should include uncertainty about the choice of bins. Choosing the 4 metre versus the 8 metre bin would make a big difference in the population estimate. There is no data on observations of bowheads at known depths. Therefore, the authors should use a range and determine how sensitive it is.  Another suggestion was to add to your variance (variance amongst the corrections, and the inverse of them – so averaging the correction factors). This seems to be an ad hoc approach but may be the best option to put some value to that important uncertainty. The Committee must agree on how many bins to use and how to weight them.

A reviewer asked for the rationale for using the 0-4m bin. The authors replied that it is based largely on previous work including surveys and tagging.

It was suggested that an ad hoc approach might work – the authors could try a 1/3, 1/3, 1/3 at 0-2, 0-3 and 0-4 metres. They would have to invert the estimates so the variance stays where it belongs.  There are two ways of looking at the issue: either the uncertainty is built in the correction factor itself, or we provide several correction factors and let the abundance paper calculate the uncertainty arising from not knowing which one to use.

The Committee agrees that there are two key issues here. One is uncertainty about the depth bins as discussed above. The second issue the area-specific differences. The difference between time periods seems less important than the uncertainty about depth bins and about locations. The statistical design (as mentioned yesterday) used (spearman’s rank correlation) is insensitive and is not really the right design – rather, the authors should use a GLM with an assumed error difference.

There was also some discussion regarding how any methodological changes suggested here may also influence the narwhal availability bias paper. It was noted that the narwhal tags cover only two stocks, so we are assuming these data are representative of narwhals in all the other areas. Even if we did find a significant difference between the two stocks (Admiralty Inlet and Eclipse Sound), we would not know which factor to apply to the other areas. So instead, we are pooling the data and therefore these concerns about locations do not apply to the narwhal papers.

Statistical analysis needs to be done to decide which coefficient is most appropriate; we also need to deal with the uncertainty related to bins as well as the issue about the dive cycle which tracks the availability (primarily for bowhead, possibly for narwhal as well).

Since we have an estimate of availability bias but don't know which bin to use, it was suggested the authors run through all the bins and see how that influences the availability bias.

Additional analysis and results will be reviewed again after the revisions are made following the meeting.

## WORKING PAPER 2B: ABUNDANCE ESTIMATE OF THE EASTERN CANADA – WEST GREENLAND BOWHEAD WHALE POPULATION BASED ON THE 2013 HIGH ARCTIC CETACEAN SURVEY

*T. Doniol-Valcroze, J.-F. Gosselin, D. Pike, J. Lawson*

*Rapporteur: Sheena Majewski*

Discussion: Regarding the high CV range in Table 3, there was a request to include the details of the calculation for the total CV of 22%. The authors replied that the Fewster systematic variance estimator was used for this analysis. This estimator is useful when there is a lot of aggregation among transect lines, and applies a post-stratification scheme to estimate variance in assessments where there is a systematic survey design. The strata with higher CV usually had relatively low N, and therefore have lower influence on overall CV and in most cases are not representative of the real range of CVs. The authors will provide further information to clarify this in the document.

It was suggested that there needs to be further examination of ice graphs to validate comments regarding a late ice break in the summer of 2013.

The Committee also recommended that details regarding perpendicular distance (shorter for fiord strata) be included in the document.

There was some discussion regarding the recommendation of a recovery factor (used for PBR calculation). This was previously calculated with a recovery factor of 0.1 (precautionary approach); the results of this study, however, are more precise than previous estimates. The authors feel the recovery factor should be changed to 0.5 due to the species status as Special Concern. Due to increased confidence in this survey, the authors could use 0.5 and 1 in calculations and present both to management to decide. The authors should highlight the reduced uncertainty in this analysis.

It was noted that the PBR could have large implications to the RFMP currently in development and management would appreciate a recommendation from science regarding which PBR is most applicable. There was a question regarding whether the survey should be considered a partial or minimum population estimate and whether this estimate is for the Canadian portion of this trans-boundary population or the entire population (i.e. including Greenland). The authors replied that this survey was not a complete coverage, but the results are fully corrected and should not be called a "partial estimate" as this is the first time the entire recognized population was surveyed in the same year. Since there is a CV around the estimate it should not be considered a “minimum” estimate either. Tag data from Greenland indicates that in summer the entire population is found in Canadian waters, so it could be clarified in the document that the current estimate is for the entire population.

There is a need to justify the recommended PBR; despite the current recognition as a “population”, it is believed that there is strong spatial segregation within the population by sex, age (etc.), and therefore a risk that some groups are targeted for harvest more heavily than others. If this population is still considered “depleted” from whaling (accounting for the Basque hunt) there could be an argument for a more conservative PBR.

If the population is increasing, there is a rationale for using a recovery factor of 1.0. It is difficult to be certain of this due to the limitations of past surveys. Also, there may be some indication that the population is not increasing; since this is only the second estimate of the population and population dynamics are uncertain it might be difficult to justify using a recovery factor of 1.0. Rather, using 0.5 provides a safety factor which could account for potential impacts of climate variability (etc).

The Committee recommended that the authors use a recovery factor of 0.5 based on uncertainties at this point, and this should be revisited after another survey is completed.

## WORKING PAPER 2C: ABUNDANCE ESTIMATES OF THE EASTERN CANADA-WEST GREENLAND BOWHEAD WHALE (*BALAENA MYSTICETUS*) POPULATION BASED ON GENETIC MARK-RECAPTURE ANALYSES

*Timothy R. Frasier, Stephen D. Petersen, Lianne Postma, Lucy Johnson, Mads Peter Heide-Jørgensen, and Steven H. Ferguson*

*Rapporteur: Sheena Majewski*

Discussion: It was noted that exclusion of in-year resightings may have helped with location dependent analysis. A reviewer was hesitant to combine probability of recapture within year and between locations and between years.

The Committee asked, if we looked at the table without the results of the survey, what rationale would we use to say that the location independent estimate is the best? There is not enough data to infer unsampled locations. Also, the assumption of a stable population over 19 years is not correct, as dataset declines over the time period. The estimate with shortest time frame most accurate. Another suggestion was to do the other time series analysis to look at potential intercept values for a true estimate (e.g. could add 10 and 15 years). Another suggestion to include a table showing what years samples were collected, as there be some years with sufficient samples for this analysis. Another suggestion was to use 20$^{th}$ percentile of the posterior distribution (vs. log-normal usually used) to calculate PBR to compare with the values obtained from the HACS estimates. Another suggestion was made to cut off the data analysis for the last 6 years since this is the majority of data.

There was some discussion regarding how to use the two estimates from the bowhead papers. It was suggested that the authors could stick to the aerial survey data for this review, then incorporate genetic approach in the next review. Because the two estimates are so close together, there is a relatively low risk in choosing one or the other, but the authors could also average both estimates weighted by their CVs to increase the certainty in the final recommendation.

Would the MR analysis, with assumptions involved, be considered as a complete population assessment? It was noted that this survey has data from Greenland; the weight of evidence from different approaches suggests the whole population is being represented in both estimates. The authors acknowledge the assessment is incomplete due to some missed locations; both methods have uncertainties, but the higher MR vs the survey estimate is consistent with the assumption that the aerial survey missed ~10% of the animals. A combined estimate may be the strongest approach.

It was noted that the assumption of a closed population may be violated, as the population may be changing. We need more information on individuals – for example, what are their characteristics with respect to how well they represent the population (e.g. age, sex and size distribution?). The authors agreed to add this information to the document.

It seems that the aerial survey assumptions have been more thoroughly tested than for the genetic data. Without being able to test the assumptions of the genetic data, we cannot say it isn't a coincidence that the estimates from the two methods are similar. As the estimates and methods become more robust, we can weight this genetic data more heavily. The biopsy sampling program can be done more frequently due to smaller expense, and will contribute to this dataset.

It was notes that there is uncertainty regarding the availability bias; the new analyses of that data will help to clarify the CV and PBR.

There is strength to the Bayesian approach; use the posterior distribution as a prior for the estimate, then evaluate weightings for each method to come up with a posterior. It was suggested to generate a PBR from each estimate.

There was some discussion regarding how to use the estimates. Are they comparable and do we have equal confidence in the two methods? We need to ignore the fact that the numbers are similar and not combine the estimates to reduce CV. We need to step back and look at whether the assumptions have been violated.

It was asked if there are inherent limitations to our confidence in the two methods themselves vs a violation of assumptions? The methods are likely similar, but not enough information is available for us to be confident that the assumptions were met with the genetic data (could be on equal footing after more years of data are collected).

In considering sampling sites for genetics data collection, do we need to increase sampling efforts in other areas? To consider equal probability of capture, the timing of sample collection for biopsies varies. The authors were asked to provide more detail regarding the assumptions and uncertainties in the discussion and possible recommendations for the future.

The assumptions may still not be met in the future, but we will have a better opportunity for testing them with as more data is collected and different mark-recapture models are applied (e.g. could use models that don't make these assumptions; stronger data set and design of genetic survey to address current uncertainties). It was asked if we should expand the design to incorporate other sites, and could we resolve some of the uncertainties by modifying sampling design? We could look at open population models for comparison.

There is less concern with uncertainty than in the possibility of bias due to inability to evaluate violation of assumptions in the genetic estimate. Therefore, we should use the estimate we have more confidence in and present the genetic estimate with the provision that, due to the small number of recaptures, there is insufficient data to test whether the appropriate assumptions were met.

It was noted that recommendations can be made by this Committee regarding testing assumptions and addressing uncertainties in future, and this should be considered for the next survey.

The Committee agreed to use the aerial survey estimate as the basis for the PBR calculation and report the genetic estimate but state that it is not being used due to a need for more fulsome testing of assumptions

It was noted that we should consider applying this method for other species. For example, there is application to walrus data currently being examined. There is support and opportunity for community based program initiatives and the use of harvested specimens for genetic data (although no recapture is possible in this case). There is also community based support to increase samples and recaptures.

It was suggested that the authors include information in the paper regarding why bowhead is a good candidate for these techniques and why they can't always be applied to narwhal. There are issues regarding heterogeneity of sampling, and understanding how narwhal stocks may be mixing, make sampling design difficult.

The Committee asked how we could improve the data in the future? The coverage of this study was good and the three areas are representative of the population, they just need more recaptures. Multi-year tagging data could inform the sampling strategy in the future.

It was asked if the areas where we don't get many samples due to whales moving through quickly, or due to low sampling effort. It is hard to access areas like Isabella Bay where whales spend lots of time. The authors noted that the three sites currently being sampled are likely the best sites but it is always useful to go to other areas. Each samples a different time of year and different point in the seasonal movement of whales.

The Committee recommended improving the design for future surveys by collecting more samples in general, and adding Isabella Bay as a concentration which may be male dominated to capture heterogeneity in the population

## ADDITIONAL DISCUSSIONS REGARDING NARWHAL AND BOWHEAD WORKING PAPERS ABOVE (WP1A-1D, WP2A-2C)

Following the in-person peer-review meeting, there were three additional conference calls of the Committee to address and resolve various analytical issues identified and discussed during the in-person meeting. Many discussions were focused on analytical methodology for calculating availability bias and therefore had implications for both narwhal and bowhead abundance estimates. The Committee made several recommendations to revise analyses in relevant papers.

There were two main issues to resolve; first, to identify the appropriate correction factor, and second, to explore diving behaviour. There was a priority to provide correction factors for the narwhal and bowhead abundance papers. There were discussions on December 5 (2014) and January 9 (2015) to address these issues and review re-analyses. On January 27 (2015) there was a final conference call to review the revisions suggested in the previous calls. These discussions were highly technical in nature, and a summary of the changes made to the relevant working papers (based on Committee consensus) are presented below.

**Narwhal Discussion**: The paper is evaluating diving behaviour based on modeling effort and recommends a correction factor for surveys in general and for 2013 HACS in particular.

When evaluating choice of models, environmental factors like depth provide a better fit for both species. The authors need to explain why they are not using this model to calculate the correction factor, and need to clarify the uncertainty associated with depth and location.

There was some discussion on why the backwards step-wise approach was used; the committee agreed that this is the correct model.

There was some discussion regarding why samples were grouped from August 25th onward. The authors replied that division was chosen because it is the period when narwhals begin making migration movements, and also it was identified as the last week of August (consistent with previous tagging studies). The authors will include an explanation in the paper.

There was some discussion on the correction factors for 0-1 and 0-2m bins for early and late August. It was suggested that the error factors should show uncertainty with depths; if the error is 0.5m and using bins 1m deep, then half of 1m bin data could have been in adjacent bins and been miss-assigned. The authors will include in the paper a discussion of information on error associated with depth and that estimate for variance is underestimated.

There was some discussion on the second objective for describing diving behaviours. The Committee questioned whether the log transformed model is appropriate to use with proportional data. The authors think this is the best approach. Looking at the literature, this is a model that has been used in other similar studies. The Committee agreed that the authors should provide the models, and provide the reasons why this method was chosen.

**Bowhead Discussion:** The depth at which bowhead can be seen needs to incorporate an estimate of variance; the authors were not certain which bin should be used and the method developed was to incorporate Figure 3 and Table 4 with an extra column for combined bin with combined variance. The conclusion paragraph recommended the correction factor be based on the new column in Table 4. This was an ad-hoc method, and poses some difficulty because the 0-6m bin included the 0-4m and 0-2m bins, and these results are not independent of one another. Based on the same individuals, the authors could adopt a probablistic approach, and give all approaches equal weight. The advantage to this is that the average is the same as averaging all results for bins, but variance is larger.

There was some discussion on model fit and appropriateness of model (carried over from narwhal paper to bowhead paper). The Committee agreed that this method is acceptable, provides a better indication of uncertainty, and reflects broader uncertainty. The Committee agreed to combine 2, 4 and 6m bins. The Committee suggested splitting the result table to make the method more obvious, and pull out 0-8m and 0-3m bins.

The authors will clarify the correction factors recommended and make the recommendation of using combined bins for the abundance papers. The authors will include more information about the best model, residuals of best model and plot of model fit. The selection of modeling approach (log transformation) and residuals for the selected model will be included.

The Committee agrees with the correction factor presented for availability bias for both narwhal and bowhead.

The Committee came to consensus on the way forward to address analytical issues for narwhal and bowhead papers. Once revised, all of the narwhal and bowhead working papers will be redistributed to the Committee for final comments. The following represents a summary of analytical changes made to the relevant narwhal and bowhead papers following the face-face meeting:

**WP1a**

- Weighted averages were used to calculate the averages for the different depth bins
- Beta regressions were conducted to determine if that was a better method for deciding which factors (period in August, sex, location) were significant – after running of this model compared to linear mixed effects models on log-transformed data it was decided that we would go with linear mixed effects models.

- Residual plots and a figure showing the relationship between water depth and time in the different depth bins were added to the paper.
- Statistic summary tables were added to the document.
- Corrections were made to the averages to use only one decimal point.
- Subheadings were added to the discussion to clarify specific recommendations for the 2013 survey versus future surveys.
- We added a sentence to discuss how there is uncertainty in the measurement of depth in the tags (0.5 m) and this would increase the variance around the bins – this should be considered in the future.
- We made recommendations that future surveys may need to consider using link or beta regression functions.

**WP2a**

- Weighted averages were used to calculate the averages for the different depth bins
- Beta regressions were conducted to determine if that was a better method for deciding which factors (period in August, sex, location) were significant – after running of this model compared to linear mixed effects models on log-transformed data it was decided that we would go with linear mixed effects models.
- Residual plots and a figure showing the relationship between water depth and time in the different depth bins were added to the paper.
- Statistic summary tables were added to the document.
- Corrections were made to the averages to use only one decimal point.
- Subheadings were added to the discussion to clarify specific recommendations for the 2013 survey versus future surveys.
- We added a sentence to discuss how there is uncertainty in the measurement of depth in the tags (0.5 m) and this would increase the variance around the bins – this should be considered in the future.
- We made recommendations that future surveys may need to consider using link or beta regression functions.
- Estimates with variance based on a combination of the 0-2, 0-4 and 0-6 m bins were added to the document, and a figure describing the process was added. In addition, it was decided to give all bins equal weight since we have no experimental evidence to support giving one more weight than another.

**WP1b**

**First step:**

- the procedure to identify duplicate sightings was scripted and automated so that sensitivity analyses could be performed
- this allowed us to include data recovered from photographs in the case of missing or uncertain measurements from visual observers
- we investigated whether it was better to use perpendicular distances instead of declination angles

- we simplified the paper by removing aspects that had little bearing on the HACS estimates
- we examined how sensitive the results were to choices of threshold values for covariates (time, angle, etc.). The resulting datasets of unique sightings will be injected into the abundance estimates to examine impact on N/Nmin/PBR

**Second step:**

- examination of the differences between visual and photo measurements (Fig. 1)
- new plots to better justify selection of thresholds (Fig. 4) + better explanation in text
- more emphasis on hard caps (Southwell method) vs. soft caps (logistic method)
- updated sensitivity analysis and conclusions

**WP1c**

- all the models have been rerun with the new set of unique sightings identified from the new version of the duplicate paper. add a detailed example of model fitting, selection and checking
- add “naïve” estimates of abundance (i.e., without spatial modelling) to compare the results for each fiord
- breakdown variance (CV) into the GAM + detection function components
- discuss perception bias
- discuss potential biases due to fiord area and complexity
- introduce the idea of a different availability correction factor for fiords with murky waters

**WP1d**

- all the estimates have been rerun with the new set of unique sightings identified from the new version of the duplicate sightings paper.
- includes the results of the sensitivity analysis to thresholds used in duplicate identification
- each component of the final variance/CV are shown (i.e., CV around detection function, group size, encounter rate, availability correction, duplicate identification)
- includes a new table with Nmin, PBR and TALC, plus justification for recommending the recovery factors for each stock
- the Smith Sound stratum has been restricted further to the small area that was actually surveyed
- inclusion of a different correction factor for fiords with murky or opaque waters

## WORKING PAPER 3: TRENDS IN THE ABUNDANCE AND DISTRIBUTION OF SEA OTTERS (*ENHYDRA LUTRIS*) IN BRITISH COLUMBIA UPDATED WITH 2013 SURVEY RESULTS

*Linda M. Nichol*

*Rapporteur: Lianne Postma*

Discussion: The Committee noted that sea otters tend to modify their environment and a question was raised asking is this a possibility as to why they have not reached carrying capacity? This discussion continued to add that carrying capacity may still be changing at the same time as the otters modify their environment (i.e. the otters tend to take out the highly profitable prey and diversity of prey, so they are not actually improving the environment in relation to carrying capacity for themselves). They may encourage the growth of large kelp beds, but does it follow that this also increases the productivity of kelp beds and therefore improve the quality of their environment? The authors noted that the numbers of large prey do not necessarily increase with larger kelp beds, as there are complicated feedback effects. There is evidence that the feedback effects on kelp beds effects primary productivity, and work is being done to investigate the potential of what this effect has in the long term. This may provide some information about how kelp beds might influence sea otter population size.

A question was raised about the selection of locations to survey for sea otters: the discussion focused on the central West Vancouver Island section and the area where there are three big inlets, one of which was surveyed and had many sea otters. Was there evidence to suggest that otters are not in the other two inlets, or have those inlets not been surveyed? The authors replied that these areas were surveyed in 2014 and there were low numbers of otters found in these additional inlets, but no rafts of otters as were observed in the main location included in the regular surveys. The survey each year is designed around reports of otters in certain areas and this information influences what inlets should be included in the survey. Along the W Vancouver coastline with suitable habitat for sea otters, the whole area (for the most part) is reasonably well used (by people), so there is likely information as to whether or not there are otters there. The Committee recommended that this explanation should be included (or made more explicit) in the document, since the survey was designed to cover areas of expansion but it wasn't clear as to why all inlets were not surveyed.

There was some discussion regarding the large variance in the counts (7 -12%), and a question was raised asking why that was not presented for the estimates? The authors replied that because there are not replicate counts it was not possible to calculate CVs around the counts. The use of replicate counts involved counts from different years compared to demonstrate precision.

The Committee then asked for clarification of how the counts are obtained. The authors explained that for any segment there are two observers, sometimes three, looking forward in the boat and they all count all otters as they slowly move forward through the area. When large rafts of otters are encountered they stop the boat and count the whole raft. For the final number for each raft, the highest observer count (not the average of all observer counts) is used because counts are not really comparable due to difficult conditions associated with counting rafts of sea otters from small boats (e.g. boat movement and other environmental conditions). If observers are experienced, then the counts are likely not that different. The Committee suggested that it would be useful to know which observer counts were used for the analyses and to include this in the document.

A question was then asked about the effect of using a different platform for counts on different segments of the survey; specifically, what was the influence of using the Coast Guard vessel for the northern end of Vancouver Island? Also, what is the platform height from that vessel and how might the change in platform height influence the counts? The authors acknowledged that the platform height was substantially higher than standing in a small boat and more stable and that it was likely the observers could see further away from the trackline than they could from a small boat. As it turned out, no rafts were observed during the offshore survey of this area as the animals were scattered, so the point being made was that difference between a count from a small boat and the coast guard ship would have be related to the number of scattered individuals visible to the observer because of platform height creating a wider strip, rather than the effect of having a better view to look down on a large raft. It was also pointed out again that only boats are used for the sea otter surveys, and helicopters were not used in 2011 or 2013 (in fact not since 2004).

Following the discussion about adding a new platform in a new survey area, it was noted that the working paper emphasized that the same route was used for the survey every year. The Committee recommended that the authors clarify this explanation so that it describes how the routes and platforms are chosen for the new additional areas that are surveyed each year.

The Committee asked if there is enough data to begin to estimate carrying capacity. If yes, then could this be done for at least the two main sea otter areas on the central part of the W. Vancouver Island coast? In response, the authors stated that it is very difficult to do this for the whole population because the population is still expanding so *K* keeps moving. But, yes, for the stable segments it could be done and an interesting comparison could be made to other estimates and methods. A reviewer suggested that, since the otters have the choice of staying in or dispersing from a given area, they would not actually be estimating the carrying capacity of that area but may instead get an asymptote of the quality of that area. However another reviewer pointed out that this would make sense if otters were logical, but animals in a local area will stay in that area even after carrying capacity is reached, especially females with pups.

There was some discussion regarding the frequency of the survey. Given that the survey takes a lot of effort and the otters tend to stay in one area once they are established (it is expected that the rafts don't get bigger, but the number of rafts can increase in a given area), would it be possible to not have to survey all areas each year? A subset of segments could be covered and the only areas that would need to be surveyed every year would be the segments at the edges of the range to monitor for expansion. The advantage of this approach is that there would then be the possibility to be able to do replicates of counts in each area surveyed within a given year. The replicates would allow confidence limits to be put around the estimates and increase the precision of the survey. Furthermore, if it is shown that if some of the points for some areas are removed, and the same estimates result, you strengthen the confidence in the surveys. This has been done in Alaska with good results.

A reviewer asked why a linear regression for growth rate is not being used and instead it is being fit to a piecewise curve? It was suggested that it should be removed and instead look at the overall regressions.  Additionally, why not try to fit more of a logistic curve? The authors replied that there doesn't seem to be enough information yet to do this. Overall can you fit something that is density-dependent related, but again, perhaps there is not quite enough information available. Another suggestion was to look at a whole habitat approach to try and develop arguments as to where carrying capacity may be and include use this information to inform decisions about appropriate models. USGS uses alternative models in California and will make the code available to the authors to use for the central W. Vancouver Is. segments.

A concern was raised that at this point what has been done in the working paper does not make any assumption about *K* (although it may be possible to estimate this for a couple of survey segments). The Committee suggested that is it probably better for now to stay away from any model that requires assumptions about *K*. If the work presented at the meeting provides the appropriate information to respond to the request for Science Advice, then the document should be left as is. However, if the request for Science Advice needs information about *K,* then should alternative models be included in this document? From a management perspective, it was noted that the issues are broader and the carrying capacity issue does not need to be addressed here, and the information presented in the document does provide the information needed to provide the advice requested. However, it would be worthwhile for the authors to explore other models where the data is appropriate to do that (for example, the two main segments on Vancouver Is).

## WORKING PAPER 4: RECENT TRENDS IN ABUNDANCE OF STELLER SEA LION (*EUMETOPIAS JUBATUS*) IN BRITISH COLUMBIA

*Sheena P. Majewski*

*Rapporteur: Lianne Postma*

Discussion: It was noted that the graph presented of BC pup counts is not consistent with the text (e.g. need to use log scale for y-axis as per figure caption). The authors will checks and make changes accordingly.

There was some discussion of the possible cause of a lag that preceded the increasing growth rate. One possibility was put forward that it could be related to a delay related to age of sexual maturity. If there was an event in the late 1980s that affected the reproductive success of the population, there would have been a decrease in that particular cohort and then there would have been a pulse once the surviving pups reached sexual maturity and began reproducing.

There was discussion around the use of the appropriate model to evaluate the population trend data and growth rates for both pups and for non-pup counts. Previous assessments used a piecewise regression to identify the point where the growth rates changed significantly after a lag in recovery. The choice of the models that were used to determine growth rates were discussed in light of the changing population dynamics. The Committee recommended that further analyses be done to compare the fits of growth rates using different types of models (i.e. exponential (or log-linear), log (2$^{nd}$ order polynomial), piecewise linear regression of log counts) and then compare models using AICc. They will all likely still show an accelerating rate of increase, but this method will provide an objective and more rigorous assessment of whether or not there is a break point as opposed to a continuous trend in the data. There may also be better support from a different type of model that adds a second order term. This analysis will be updated in the document.

The Committee suggested conducting the model selection, and then reporting the growth rate of the exponential increase. However if the piecewise model is more appropriate, only report the last (most recent period) growth rate in the model. If a different model is supported and the model still shows that the growth rate is changing, then only report that rate. Because this is an update, the request for advice is only concerned with the most recent period of time.

There was some discussion regarding the use of correction factors to estimate absolute abundance based on the counts. The pup and non-pup counts are presented to provide a minimum estimate of abundance, allowing for analysis of population trends over time. The methods used in the past by Olesiuk and others to estimate absolute abundance of the population were discussed in terms of their applicability for this and future assessments. These methods included use of satellite telemetry data to correct for availability bias (portion of non-pups not visible during summer breeding surveys because they are in the water), and a second method using modified pup multipliers based on life table data.

The Committee noted that the scaling factors used for the pup multiplier may not be appropriate without further adjustment for increasing growth rates, especially in the areas where the population is possibly approaching K. Building an age-specific population structure involves a number of complex factors and there is a need to attach all the caveats of these to the model. Further investigation/updating of life tables for the eastern population and sensitivity analysis of the pup multipliers (similar to that done by Olesiuk in the past) might be warranted for future assessments. A question was raised about the idea that pregnancy rates may vary from year to year, especially if the sites are approaching carrying capacity (as is seen in seals). For this reason, using a constant (across site) multiplier to go from pups to adults may not then be appropriate. The use of the multiplier may also not be accurate when a particular site is approaching density-dependence. This may not have been an issue in the early years of the surveys, but may be more of an issue now and should be considered when going forward analyses of future surveys. In addition, growth rates are likely different in newly established rookeries versus older rookeries.

A reviewer noted that the pup ratio seems to be breaking down. A recommendation was made that perhaps in the future there is a need to develop a more rigorous model for estimating the total population. There was an additional comment about how the assumptions around the number of animals seen may influence the estimates of abundance (for example the number of animals in the water). Also, if the BC rookeries are coming close to reaching the max rates of fecundity, how might this affect movements off of the rookeries?

There was some discussion regarding adjusting the estimates of absolute abundance based on pup multipliers to reflect relative pup production in BC and SE Alaska (due to the movement of animals between the rookery at Forrester and BC sites) as used by Olesiuk 2010. A question was raised about where the two estimates for pups come from? It appears that one estimate uses a pup multiplier for BC only and the other is for BC and Alaska both that is then adjusted for non-pups in BC. A simpler approach was proposed, for example using pups counted in 2013 adjusted with a multiplier of X, yielding the number of pups born in BC waters. The advice we are being asked to give is only for the BC pup production. Without a good model to account for movement between BC and Alaska it would be better to just report the BC number. In addition, we do not have enough information about new rookeries in Alaska to be able to account for the influence of these rookeries on the BC portion of the population. So again, it was suggested to not address SE Alaska in the numbers (only be needed for range wide estimates). However, the SE Alaska animals may spend a lot of time in Canadian waters and determining an estimate of that number may have an impact on prey species in Canadian waters. While that would be good information to know, it is likely a separate question from the request for science advice needed.

Satellite telemetry may be able to address movements between rookeries between BC and SE Alaska. However, in the working paper the telemetry data was only used to correct for number of animals assumed to be in the water for BC haul-outs and rookeries. This information then leads to an amount of scaling that is less than that for the pup number that uses the pup multiplier. So, during the breeding period, there is a need to determine the 95% confidence of how much time animals were in the water. Then, the estimate derived from the portion hauled-out would have fewer assumptions than the use of the pup multiplier and would be a better number to use. Again, there would be a need to look at the use of the various correction factors.

The estimate based on proportion hauled out (number of animals during the breeding season as the population estimate) is only for BC portion of the population but does not account for all animals using BC waters. Thus, there is a need to bring in more information from SE Alaska. This may have been done previously, but need to revisit any work done to see what methods were used and to what extent the information may be able to be updated. The authors may then be able to determine if there has been an increase or not in total number of animals using the BC coast. It was noted that these previous methods and analyses may have been done but have not been published.

A question was raised about the possible influence of mis-identification rates of pups as non-pups, juveniles as adults (etc.). This may be a non-issue, but this point moved the discussion back to the suggestion that in the future a full blown population model using mortality and birth rates may be appropriate, or is the likelihood better that there will be more telemetry data to get more information on time spent hauled out? If the mortality rates come from the Forrester Is. Rookery (the biggest rookery), they may be more applicable and useful for a full population model. But for only the BC portion of the population, the telemetry option may be more feasible and the better approach. If this is the way to go, there may be a need for a more detailed explanation of the telemetry approach and how it is used and what the tags represent. This will help to characterize the uncertainty and characterize the haul out probability appropriately.

Further discussion of the limitations of the existing correction factors and life table data will be included in this document, as well as recommendations for updating the pup multipliers in collaboration with NOAA. However, estimates of absolute abundance based on pup multipliers will not be included. Due to uncertainties and limitations in available life table data, it was recommended that the pup multiplier not be used to derive an estimate of absolute abundance for this assessment and it was suggested that it would be good to go through the document and remove all references to the pup multiplier. Confidence intervals for the estimates based on telemetry data will be explored and included in the document. It was recommended that the possibility of updating life tables and telemetry studies be examined for future assessments.

There was some discussion regarding how the uncertainty associated with the counts on the rookeries is accounted for in the numbers. Were duplicate counts from a single reader used? Were count comparisons between readers (ideally with a very experienced reader, as for seals) possible? For this current survey there was the opportunity to have some comparisons to NOAA readers. Recounts were conducted when there was greater than 10% discrepancy. The intention is to compare counts for all BC obliques with an experienced counter (e.g. Olesiuk) prior to finalizing the document. The recommendation of the Committee was to continue with exchange of photos with NOAA (some subset – for example 50 photos) and use that for comparison and the determination of uncertainty in the counts.

In addition, the correction factor for the pup counts needs some uncertainty factor added to it. It could come from the readers or the camera or the one-week differences. It was suggested to try to re-read some subset of the photos and use those second counts to develop the uncertainty.

There was a question about the comparison of the ease of counting animals in the vertical photos as compared to the oblique photos and whether a correction factor for pups not visible in oblique photos is still necessary/relevant. Given some comments about problems with distinguishing all the animals in the oblique photos, should DFO move to taking vertical photos for the surveys? It was noted that the different type of photos had different strengths and weaknesses. Vertical photos are more expensive to obtain and the new high resolution cameras and use of an experienced pilot in a small plane have improved the oblique photos to the point that they may be of comparable quality to the verticals; due to cost and availability of equipment it is likely that the vertical photos will continued to be used in future surveys, but that the correction factor applied to the oblique counts should be re-evaluated.

Counts will be validated by comparison with counts from a second counter (either a subset or full counts) and estimates of uncertainty for the pup and non-pup counts added to the document.

## WORKING PAPER 5: SUSTAINABILITY OF A FLEXIBLE SYSTEM OF TOTAL ALLOWABLE ANNUAL CATCHES OF NARWHALS (*MONODON MONOCEROS*)

*P. R. Richard and R. Young*

*Rapporteur: Hilary Moors-Murphy*

Discussion: The Committee asked about $F_r = 0.5$ being used based on the assumption that the starting population is below MNPL. In reality, we do not know if the population is below or above MNPL, so how do we justify using $F_r = 0.5$? The authors replied that in the past, they looked at historical numbers of narwhals to determine whether the population has declined substantially (thus putting the population under MNPL) or not, and a judgment was made. Under circumstances where it was thought that a population had declined, $F_r = 0.5$ was then used. For most narwhal populations, Fr=1 was used, assuming the populations were large and over MNPL.

Additionally, PBR guidelines suggest using the largest population known historically as something close to K. This estimate can be used to backup that the narwhal population is probably below MNPL, providing some justification for the use of $F_r$ = 0.5 in the model. $F_r$ = 0.1 is meant to be used when a population is in the critical zone. To the best of our knowledge, narwhals are not in the critical zone.

The Committee suggested that if the authors use $F_r$ = 0.5, it would help if the unknowns relevant to this assumption were explicitly stated in the text (i.e., clearly state that we don't actually know if $F_r$ = 0.5 is correct because we don't know if the population is above/below MNPL). The implications/limitations of assuming $F_r$ = 0.5 should also be discussed in greater detail.

The Committee suggested running the simulations also using $F_r$ = 1. $F_r$ = 0.5 vs $F_r$ = 1 will impact simulations for large populations differently than small populations, and this should be discussed in the paper.

The Committee noted that the distribution formula in the text (related to process error) is incorrect. Make sure the equation presented corresponds to the model code used, and that the model code used is correct. Also, the authors should make sure the possibility of negative values is included in the process error model. The authors noted that there is a mistake in formula of equation in the text, but it was modeled correctly. They will double check this and ensure the formula in the text and the model code is correct.

The Committee suggested that the process error should be changed to incorporate possibility of some catastrophic event, as they can and do happen (e.g., recent ice entrapments that caused 2-3% mortality). Such a model might be a more realistic process error model. Also, the process error model should incorporate some stochasticity. The authors replied that the results would likely be the same even if we did this, and if there is a catastrophic event, the next PBR update would capture it.

The Committee recommended using a log normal process error distribution. To do this, replace the current process error term with a normal random deviate (mean = 0, SD = s), then exponentiate it. You will then end up with a log normal distributed error.

The Committee asked about referring to the 2008 Research Document for determining hunting loss – was more recent information provided on hunting loss and if so, can you incorporate this more recent hunting loss data? The authors replied that this is irrelevant, the model does not incorporate hunting loss rates and is calculated based on PBR only. It is in the application that hunting loss rates become incorporated into the Total Allowable Catch. This is something that has to be considered when implementing such a system. The Committee suggested pointing this out again in the discussion, being very clear about this caveat in terms of application.

The Committee noted that in the PBR simulations, the authors should underline that removals are a representation only and what is happening in reality is important to consider and could change results. For example, if hunting removals are targeting large males, then PBR is likely conservative. However, if hunting removals target females, then PBR is not likely conservative and recovery will be much slower. The authors should more explicitly discuss this point (as not targeting removal of females is a key assumption in PBR).

The Committee noted that the model assumes that if five times the PBR is taken in one year, than the hunt would be closed in subsequent years. In reality, this will be hard to implement. These implementation issues should be reiterated in the discussion.

## WORKING PAPER 6: UPDATED GENETIC MIXTURE ANALYSIS OF NUNAVIK BELUGA SUMMER STOCKS TO INFORM POPULATION MODELS AND HARVEST ALLOCATION

*T. Doniol-Valcroze, M.O. Hammill and L. Postma*

*Rapporteur: Jack Lawson*

Discussion: The Committee suggested that the authors add a map of the study area to the document, in addition to a more detailed description of the sampling methodology (in what communities the samples were collected, and where the whales were actually killed, if known). The Committee also suggested that the authors add the colour haplotype proportion figures of the meeting presentation to the paper as well.

Upon reviewing the analyses, the Committee proposed that there may be an analytical impact of cluster sampling, since beluga travel in groups, and suggested that the authors attempt to address this in the document. The authors responded that this could not be addressed at this stage as much of the catch information is not subdivided by hunting "event" so one could not determine if multiple landed whales were from a single pod. However, the authors suggested that this type of information is improving recently so going forward these sorts of questions might be answerable. The authors felt that the large sample size might balance out the impacts of clustered sampling in any case.

The Committee reviewed the Sanikiluaq sample proportions and asked for further explanation of the relatively high Cumberland Sound haplotype proportion in the sample classification. Could it be that with the current genetic data we can't discriminate three populations, but instead two (e.g., EHB and non-EHB)? The authors responded that they had previously tried to split the samples based on a two-stock model as EHB is so distinct, whereas other stocks are much less distinct. While the summer stocks are distinctive, the genetic signal in the mDNA, or the impacts of historic founder effects, may mean the samples are less representatives of movements of whales between areas (e.g., James Bay is a very distinct stock, and there may be a "south Hudson Bay" stock). The authors felt that perhaps there are more, and more genetically-complex, beluga stocks than identified currently. The authors were confident in the identification of EHB stock in the harvest samples, but less so for other source stocks (suggesting that there may be more source stocks for the Cumberland Sound harvest samples). The authors suggested that, as presented, there may be a false perception of precision in the stock genetics data; for example, one inference from the paper could be that there is some movement of Cumberland Sound beluga whales into southern Hudson Bay, but the Committee felt that the genetics data as presented might not support this. The Cumberland Sound beluga harvest samples might be considered an "outgroup" in that they might be composed of whales from a more complicated stock structure. Given this, the Committee recommended that the authors edit the paper to emphasize the uncertainty in eastern Arctic beluga stock structure. Therefore, the authors agreed to redo the analysis without the Cumberland Sound source group (i.e., keep only EHB and WHB as sources to which samples are assigned).

The Committee asked what is the key conservation issue for EHB beluga? The authors responded that resource conservation is the goal of current management plans, so they have to be careful how they describe the results so that the paper still highlights the importance of controlling the EHB beluga harvests. The Committee suggested that a Fisher's Exact test on the harvest data may show that increased sample sizes may still not allow for finer stock discrimination. Further, the Committee suggested that the paper should emphasize the uncertainty in the contributions, and remove reference to an "increase" in the EHB harvest proportions in Sanikiluaq; since many communities share the 60 EHB beluga quota, the Committee felt that it is important to highlight the harvest proportions taken as EHB.

The authors pointed out that the analysis has not broken down the harvest samples at the community level, but in any case many of the sample sizes would be too small to allow meaningful comparisons. The Committee asked if the EHB haplotype is present in all harvests. The authors responded that all of the hunts represent mixtures of haplotypes, so it is difficult to partition the hunt-based samples by other than proportion of haplotypes – beluga carrying the EHB haplotype are available to be taken by hunters in many areas.

For several regions with only a couple of harvest samples, the Committee asked the authors to provide an indication of how important these hunts are to the stock structure analysis – if there is little hunting effort to provide these small samples, that suggests a different context to interpret the results than if the small sample size was the result of a larger landed hunt or greater effort (e.g., which areas are well sampled?). The authors pointed out that the management scheme might have been established to have hunters harvest nearer their communities. In any event, the authors felt that the derived proportions might be considered to be precautionary.

The authors suggested that the purpose of the paper was to update the proportions of genetically-discrete beluga stocks in the beluga harvest data, not to assess hunt impact by extrapolating the genetic stock proportions using the landed values.

To better understand the lengthened hunting season, the Committee suggested that the authors look more closely at the ten whales taken in that period until mid-July. The authors replied that there would be issues with this small sample size. The authors stated that it is likely the next management plan will use the 4.5% EHB stock contribution estimate from the extended season analysis as it does not over emphasize the contribution of these ten extended-season whales. Further, the results of these analyses are consistent with satellite tag information on beluga movements.

The authors summarized the major result of this study in that, with the current dates of the extended hunt, the Sanikiluaq harvest is composed of approximately 4.5% EHB stock whales, but with a large CV. Previously, the end result of interest was the proportion of stock contribution in a community's hunting effort, rather than the exact location from where the whales were taken. While there are few samples from the July harvest, making it difficult to partition results further than they have been in the paper, the Committee recommended that the authors have a closer look at the "winter season" data independent of the entrapment events (since the entrapments do have a high probability of including related whales).

The Committee suggested that the authors compare the 1982-2006 to 1997-2013 periods, and use this as a way to commend the Sanikiluaq group for what they have been doing right in their harvesting. Also, the Committee suggested that the authors look more closely at the Dec-Jan-Feb data, almost all of which will be from ice entrapments.

Since a previous paper by Colbeck et al. suggested migrating beluga whales groups are often highly related, the Committee suggested that it may be possible to take a similar approach to these analyses. The authors were not sure if this is possible since a lot of sample size will be lost through sample partition, and it might be harder to do genetic cluster analysis. While this sort of analysis is difficult to do with this data currently, the Committee recommended that DFO collect better data on hunt documentation to include information as to whether multiple samples were taken from a single beluga pod etc.; this would be useful to find out if the precision of the hunt proportion estimates are being influenced (and perhaps overestimated) by beluga social structure and hunting methods that are likely to capture groups.

## APPENDIX A: TERMS OF REFERENCE

## ANNUAL MEETING OF THE NATIONAL MARINE MAMMAL PEER REVIEW COMMITTEE (NMMPRC)

### National Peer Review - National Capital Region

October 20-24, 2014
Ottawa, Ontario

Chairperson: Don Bowen

### Context

The National Marine Mammal Peer Review Committee (NMMPRC) holds an annual meeting to conduct scientific peer-review of marine mammal issues. This meeting provides the opportunity for collaborative review of scientific results by marine mammal experts from Fisheries and Oceans Canada (DFO) and from other (non-DFO) organizations. Following NMMPRC review and approval, scientific results are used to provide sound scientific advice for the management and conservation of marine mammals in Canada. When time permits, this annual meeting is also an opportunity to review ongoing research projects and provide feedback or guidance to the scientists involved.

### Objectives

This year, the papers to be reviewed will include topics pertaining to Baffin Bay narwhal and Eastern Canada-West Greenland bowhead whale, Eastern Hudson Bay beluga, sea otter and Steller sea lion (see below for individual topics).

### Expected Publications (see individual topics below)

- Science Advisory Report(s)
- Research Document(s)
- Proceedings

### Participation

The following groups were invited to participate in the meeting:

- Fisheries and Oceans Canada (DFO) (Ecosystems and Oceans Science, and Ecosystems and Fisheries Management)
- Greenland Institute of Natural Resources
- St. Andrews University, Scotland
- National Oceanic and Atmospheric Administration (NOAA) U.S. Geological Survey (USGS)
- Nunavut Wildlife Management Board
- Nunavik Marine Region Wildlife Board
- Makivik Corporation
- Nunavut Tunngavik Inc.

## Topics

### 1 and 2. Bowhead Whale and Narwhal Abundance

### Context:

### Results of 2013 High Arctic Cetacean Survey

DFO conducted a comprehensive survey of narwhal, beluga and bowhead whales in key areas of the eastern Canadian Arctic in August, 2013. Together, the surveys covered the Canadian range of summering aggregation areas of Baffin Bay narwhals, Eastern High Arctic - Baffin Bay and Cumberland Sound beluga, and Eastern Canada-West Greenland bowhead whales. Several of these areas had never been surveyed before and a comprehensive survey of the entire summer distribution of narwhal and bowhead whales had never been attempted in the past.

Narwhal, beluga and bowhead whales inhabit Arctic waters year-round and are facing tremendous changes in their environment, ranging from increased human activities (e.g., oil and gas exploration, shipping) to climate change. These species also have widespread economic, social and cultural importance for Inuit, and it is therefore crucial to establish sustainable hunting levels based on accurate and up-to-date abundance estimates for long term sustainability of the stocks/populations. Obtaining these estimates, however, is complicated by the population structure of narwhals (which separate into several summering aggregations with varying degrees of site fidelity), by the highly mobile nature of bowhead whales, and overall by the vast range of these animals in the eastern Canadian Arctic.

DFO has recommended that Baffin Bay narwhals be managed on the basis of five summering aggregations (i.e., Somerset Island, Admiralty Inlet, Eclipse Sound, Eastern Baffin Island, and Jones Sound). With the exception of narwhals in Admiralty Inlet (surveyed in 2010), survey estimates for the other four aggregations date from 1996 to 2004. For bowhead whales, the most recent abundance estimates have been criticized for being based on surveys flown in 2002 that did not include the entire summer range of the population. Moreover, there is evidence that the distribution patterns of some of these cetacean summering stocks may be changing.

### Bowhead Whale Genetic Capture-Mark-Recapture (gCMR)

Regional Fisheries Management and Science sectors at DFO have jointly developed a multi-year science and management plan for bowhead whales that includes the evaluation of alternative methods to estimate population abundance. Among these is the genetic mark-recapture of individual bowhead whales from genetic analysis of biopsy samples obtained during the multi-year sampling program. This research is part of a longer term plan to evaluate the distribution and movement patterns of bowhead whales.

### Objectives:

### Results of 2013 High Arctic Cetacean Survey

The results of this survey are expected to produce up-to-date abundance estimates and sustainable harvest advice (based on the visual component of the surveys; the photographic component will be reviewed in 2015) for the five narwhal summering stocks and the Eastern Canada-West Greenland bowhead population in Canadian waters. The survey results will also provide new information on the spatial distribution of narwhal, bowhead whales and beluga in their summering areas. Additional estimates based on the photographic component of the survey will be completed and reviewed in 2015.

**Bowhead Whale Genetic Capture-Mark-Recapture (gCMR)**

To evaluate:

- A Bayesian mark-recapture approach that has been developed specifically for bowhead whales sampled from the EC-WG population (that addresses the issues of low recapture rates and movements of animals to and from unsampled areas);
- Abundance estimates for sampled and unsampled areas and an overall population abundance estimate; and
- Future efforts for bowhead biopsy sampling of the EC-WG population required to update the model to more reliably estimate EC-WG bowhead whale abundance, using genetic-mark recapture methods.

**Topic 1 (narwhal) Expected Publications:**

- Four Research Documents
- One Science Advisory Report

**Topic 2 (bowhead whale) Expected Publications (aerial survey results and genetic mark-recapture results):**

- Three Research Documents
- One Science Advisory Report

**3. Recent trends in the abundance and distribution of sea otters in British Columbia, based on the 2013 survey**

**Context:** The Sea Otter (*Enhydra lutris*) was assessed by COSEWIC as Threatened in 2000 and listed as Threatened under the *Species at Risk Act* (SARA) in 2003. In 2007 the species was reassessed by COSEWIC as Special Concern, and subsequently down-listed to Special Concern under SARA in 2009.

Sea Otter populations in BC have been surveyed since 1977, as a means of monitoring the recovery of this species in Canadian Pacific waters. A standardised survey methodology suitable for on-going assessment of the Sea Otter population was developed (Nichol *et al.* 2005), and remains in place today. Assessment of trends in abundance and growth are dependent on a time series of survey data and therefore on-going population surveys at regular intervals are important.

The Recovery Strategy for the Sea Otter (*Enhydra lutris*) in Canada (2007), and the superseding Management Plan for the Sea Otter (*Enhydra lutris*) in Canada (2013), both identify population assessments in the form of regular surveys as required to monitor progress towards recovery. Specifically, the Management Plan identifies the undertaking of “annual surveys of the Sea Otter population in index areas, areas of range expansion, and other portions of their range as needed, as well as a total population survey every five years, to monitor population trends and distribution” as a high priority for the recovery of this species, and to assess progress towards the broad strategies and conservation measures outlined in the Management Plan.

Additionally, there is First Nation interest to harvest Sea Otters for Food, Social, and Ceremonial purposes; currently all harvest of Sea Otters, including FSC, is closed. In order to consider if and how such removals may occur without adverse impact to the population (i.e. sustainable harvest), DFO requires an accurate understanding of population status and trends, which may inform the development of a potential FSC harvest plan or other similar approach. This understanding will be informed by the results of this ongoing population survey; not having this information will place the department in a position of risk, as any potential ongoing infringement of Aboriginal Rights may not be adequately defended.

Lastly, information on population abundance and range expansion can be used to inform management actions and decisions made by DFO Fisheries Management and Aquaculture Branches (e.g. with respect to minimizing incidental catch and entanglement, predation (e.g. of shellfish, spawn-on-kelp etc.), and siting considerations).

**Objectives:** In 2013, a total population survey was undertaken by DFO as part of the ongoing series of population assessments for this species. This request is for the analysis of the 2013 survey data as per the standardised survey methodology developed by Nichol *et al.* (2005). Specifically, the objectives are to provide the following:

Survey Procedure Assessment

- Describe the survey condition as per Nichol *et al.* 2005.

Current Range

- Describe the range observed through the 2013 total population survey in terms of observed survey assessment areas.
- Describe the changes in range that have occurred since the 2008 total population survey (Nichol *et al.* 2009), since the species' reintroduction in 1969-72, and in comparison to historic.

Abundance

- Provide a minimum abundance estimate for the population as per the 2013 total population survey observations, in total and by area.
- Provide an estimate of abundance for the population as per the 2013 total population survey observations, in total and by area.
- Describe the changes in estimated abundance that have occurred since the 2008 total population survey (Nichol *et al.* 2009), since the species' reintroduction in 1969-72, and in comparison to historic range in BC.

Population Trends

- Describe the current estimated growth rate observed through the 2013 total population survey, by area and in total.
- Describe the changes in estimated growth rate that have occurred since the 2008 total population survey (Nichol *et al.* 2009), and since the species' reintroduction in 1969-72.
- **Expected Publications:**
- One Research Document
- One Science Advisory Report

**References:**


Nichol, L. M., J. C. Watson, G. E. Ellis and J. K. B. Ford. 2005. An assessment of abundance and growth of the sea otter (*Enhydra lutris*) population in British Columbia. DFO Can. Sci. Advis. Sec., Res. Doc. 2005/094.

Nichol, L. M., M. D. Boogards, R. Abernathy. 2009. Recent trends in the abundance and distribution of sea otters (*Enhydra lutris*) in British Columbia. DFO Can. Sci. Advis. Sec., Res. Doc. 2009/016.

## 4. Population status assessment for Steller Sea Lion

**Context:** The Steller Sea Lion (*Eumetopias jubatus*) was assessed by COSEWIC as Special Concern in 2003 and listed as Special Concern under the *Species at Risk Act* (SARA) in 2005. The species was re-assessed in 2013 and the COSEWIC status remains at Special Concern. This species is restricted to only five breeding locations (consisting of 7 rookeries) in British Columbia that occupy less than 10 km², with approximately 70% of births occurring at a single location (Scott Islands). The population is increasing, but is sensitive to human disturbance while on land and is vulnerable to catastrophic events such as major oil spills due to its highly concentrated breeding aggregations. The species is near to qualifying for Threatened, but has recovered from historical culling and deliberate persecution.

Steller Sea Lion populations and distribution in BC have been surveyed since 1971, as a means of monitoring the recovery of this species in Canadian Pacific waters. Beginning in 1994, the surveys have been conducted as part of an international range-wide survey that extends from California to Alaska to Russia. Survey techniques were standardized (Olesiuk *et al.* 2008) and schedules coordinated with the National Marine Fisheries Service, to coordinate monitoring of this highly mobile transboundary species and provide a comprehensive evaluation of the Eastern Population of Steller Sea Lion over its entire range.

The Management Plan for the Steller Sea Lion (*Eumetopias jubatus*) in Canada (2011) identifies DFO's "support [and contribution] to the coordination of range-wide surveys, every four years" as a high priority for this species, to meet the Research and Monitoring Objective R2 – "Conduct range wide population assessments through coordinated Canadian and US surveys, where feasible." The information obtained through such surveys is necessary to monitor risk to the population by identifying new or re-established rookeries on the coast of British Columbia, and to inform management actions that serve to protect the species from identified threats.

Additionally, First Nations harvest of Steller Sea Lions occurs occasionally. Currently, harvest limits are unknown but considered to be negligible; however, harvest limits established for FSC purposes are set to ensure harvest occurs within sustainable limits, which is informed in turn by population assessments. Should there be interest in increasing FSC harvest levels, the population assessments would be used to inform the development of a sustainable harvest plan or similar approach.

Lastly, the results of this population assessment will inform actions undertaken to meet objectives identified in the Recovery Strategy for the Transient Killer Whale (*Orcinus orca*) in Canada, specifically R2 – "Minimize the risk of prey population reductions from anthropogenic activities, until precise prey needs can be determined." Transient Killer Whales are listed under SARA as Threatened, and could be vulnerable to fluctuations in their prey species.

**Objectives:** In 2013, a province-wide aerial survey was undertaken by DFO as part of the ongoing series of population assessments for this species, in coordination with a range-wide survey effort conducted by NMFS conducted that same year. This request is for the analysis of the 2013 survey data as per the standardised survey methodology developed by Olesiuk *et al.* (2008). Specifically, the objectives are to provide the following:

Current Range

- Describe the range (e.g. population distribution, location of established, re-established, and/or new rookeries) observed through the 2013 total population survey.
- Describe any changes in range that have occurred since the 2010 population survey (Olesiuk 2008), and in comparison to historic range.

Abundance

- Provide a minimum abundance estimate for the population (pups and non-pups) as per the 2013 population survey observations, in total and by area (e.g. rookeries).
- Provide an estimate of absolute abundance for the population (pups and non-pups) as per the 2013 total population survey observations, in total and by area (e.g. rookeries).
- Describe the changes in estimated abundance that have occurred since the 2010 total population survey (Olesiuk 2008), and in comparison to historic range.

Population Trends

- Describe the current estimated growth rate observed through the 2013 total population survey, by area (e.g. rookeries) and in total.
- Describe the changes in estimated growth rate that have occurred since the 2010 total population survey (Olesiuk 2008).

To determine the current population status for the Steller Sea Lion in Canadian Pacific waters, based on the 2013 survey.

**Expected Publications:**

- One Research Document
- One Science Advisory Report

**References:**

Olesiuk, P. F., D. G. Calkins, K. W. Pitcher, W. L. Perryman, C. Stinchcomb, M. Lynn. 2008. An evaluation of Steller sea lions (*Eumetopias jubatus*) pup counts from 35mm oblique images. DFO Can. Sci. Advis. Sec., Res. 2008/064.

Olesiuk, P. F. 2008. Abundance of Steller sea lions (*Eumetopias jubatus*) in British Columbia. DFO Can. Sci. Advis. Sec., Res.2008/063.

**5. Narwhal Harvest Credit System**

**Context:** DFO has approved an Integrated Fisheries Management Plan (IFMP) for narwhal fisheries within the Nunavut Settlement Area (NSA) as negotiated between the Nunavut wildlife co-management organizations and DFO. A key component of this management regime is a Marine Mammal Tag (MMT) transfer policy. An evidence based three-phase approach was negotiated for development and implementation of MMT transfer in the narwhal harvest. Phase 1 provides for the transfer of unused non-migratory narwhal MMT amongst the communities that harvest within a specific Management Unit (MU) during the harvest year for which the MMT are issued. There is no transfer of migratory period narwhal MMT provided for under Phase 1, which was implemented April 1, 2013. Phase 2 may provide for the transfer of migratory period MMT between the communities of Arctic Bay and Pond Inlet in the Admiralty Inlet MU and Eclipse Sound MU respectively, and amongst the communities of Clyde River, Qikiqtarjuaq, Pangnirtung and Iqaluit within the Eastern Baffin Island MU.

There is a desire on the part of Inuit to have a "flexible quota system" management provision implemented in the fishery, similar to what was employed under Community Based Management (CBM) of narwhal and beluga in Nunavut prior to negotiation and implementation of the IFMP. Under CBM, there was provision to carry-over unused quota for use in the subsequent harvest season or to borrow-back up to 15% of the following years' quota for use in the current harvest season. There is also a desire on the part of Nunavut Tunngavik Incorporated (NTI) to have narwhal Total Allowable Harvest (TAH) established for each MU in

5-year blocks, rather than annual TAH harvest limitations. This information will be used to determine whether a harvest credit accumulation and/or borrow-back system can be sustainably implemented in the narwhal harvest within the NSA.

**Objectives**: The objectives are to answer the following specific questions:

If the harvest credit accumulation system proposed is implemented within each of the narwhal Management Units (MU: Northern Hudson Bay; Somerset Island/ Admiralty Inlet; Eclipse Sound; East Baffin Island; Grise Fiord) will the harvest of narwhal be sustainable?

**Proposal 1. 100% carry-over for 1 year only**

All unused MMT in a given harvest season within a narwhal MU is carried over for use in the next harvest season only. UnusedMMTs carried-over from the previous year are applied first to narwhal harvests in the current year. Carried-over MMTs expire at the end of the harvest year for which they were carried over. No more than 2 times the annual narwhal TAH may be landed in the MUover 2 consecutive harvest seasons.

**Proposal 2. (0% to 100%) carry-over for 1 year only**

If 100% carry-over for 1 year only is unsustainable, is there any proportion of carry-over less than 100% that is sustainable?

**Proposal 3. (0% to 100%) carry-over for consecutive years**

If some percentage of carry-over for 1 year is sustainable, can unused MMT from each harvest season be accumulated for use in subsequent harvest seasons for consecutive years, potentially indefinitely until the existing TAH is modified and still be sustainable? In this case unused annual MMT within a MU would continue to accrue for use in subsequent harvest seasons.

**Proposal 4. 100% borrow-back**

In any given harvest season, any portion of the next years TAH for the MU may be used in the current harvest season. However, the MU's TAH for the subsequent season is discounted by the amount borrowed for use in the current harvest season.

**Proposal 5. (0% to 100%) borrow-back**

If 100% borrow-back is found to be unsustainable, is there any proportion of borrow-back less than 100% that is sustainable?

**Proposal 6. Five year TAH**

If the 5 year sum of annual TAH for each narwhal MU within the NSA was applied as an overall narwhal harvest limit that may be prosecuted at any time during this 5 year consecutive period, would it be sustainable?

**Expected Publications:**

- One Research Document
- One Science Advisory Report

**6. Eastern Hudson Bay Beluga**

**Context:** With the signing of the Nunavik Inuit Land Claim Agreement in 2006 the Nunavik Marine Region Wildlife Board (NMRWB) became the main instrument of wildlife management for the Nunavik Marine Region (NMR). The objectives of the NMRWB are to establish wildlife management systems that reflect Nunavik Inuit harvesting rights, while being governed by the principles of conservation (NILCA s. 5.1.3 (a) and (c)).

Eastern Hudson Bay (EHB) beluga were designated by COSEWIC as Threatened in April 1988; the status was re-examined and designated as Endangered in May 2004.

Given existing conservation concerns for the EHB belugas, the NMRWB has established a Total Allowable Take (TAT) for this stock. The level of sustainable harvest has been heavily informed by DFO Science in the past (e.g. DFO Sci. Advis. Rep 2014/005). TheNMRWB is also in the process of implementing a new management system which relies substantially on genetics research, specifically the proportion of EHB belugas harvested in different regions of Nunavik (Turgeon *et al.* 2012). Turgeon *et al.* (2012) include analysis of data up until 2006 and based their analyses on groupings of sampling data based on sub-regions. Given that the NMRWB recently approved new management zones, adjustments to the data grouping would improve accuracy and bring the science advice into alignment with the NMRWB's decisions. Additionally there is more recent data available that would improve the confidence in the proportions currently being used. Having access to this science advice, with improved accuracy and confidence, and being more reflective of the NMRWB's management decisions, would provide the NMRWB with the best available information, which is essential in ensuring the sound and adaptive management of EHB beluga.

**Objectives:** To determine what is currently the best estimate of the proportion (and associated uncertainty) of EHB belugas harvested in Sanikiluaq, both annually and when analyzed based on season? This evaluation should also take into account the voluntary harvest closure from July 15th until September 30th, which has been in place since 2012. The NMRWB is interested in answering the question of when the voluntary summer closure Sanikiluaq has in place would be most effective to conserve EHBbeluga?

**Expected Publications:**

- One Research Document
- One Science Advisory Report

# APPENDIX B: LIST OF PARTICIPANTS

## DFO

Christine Abraham

Alejandro Buren

Don Bowen

Nell den Heyer

Jean-François Gosselin

Garry Stenson

Simon Nadeau

Mike Hammill

Thomas Doniol-Valcroze

Veronique Lesage

Jack Lawson

Hilary Moors-Murphy

Lianne Postma

Linda Nichol

Steve Ferguson

Bernard Leblanc

John Ford

Sheena Majewski

Jen Shaw

Patt Hall

Stefan Romberg

Melissa Landry

Paul Cottrell

Marianne Marcoux

## EXTERNAL REVIEWERS AND STAKEHOLDERS

Tim Fraser – St. Mary's University

Tim Tinker - USGS

Rod Hobbs – NOAA

David Lee – Nunavik Tunngavik Inc.

Kaitlin Breton-Honeyman - Nunavik Marine Region Wildlife Board

Greg Gilbert – Makivik Inc.

# APPENDIX C: AGENDA

## Meeting of the National Marine Mammal Peer Review Committee

October 20-24, 2014
Delta Hotel - Ottawa City Centre
Ottawa, ON

Chairperson: Don Bowen

Daily schedule plan as follows, but allow for some flexibility:

Start: 0900
Break: 1030-1045
Lunch: 1200-1330
Break: 1500-1515
End: 1400-1730

## DAY 1 - Monday October 20

| Time allowed (min) | Paper # | Topic | Rapporteur | Time Guideline |
|---|---|---|---|---|
| **15** | n/a | Welcome and instructions for participants, rapporteurs, etc (D. Bowen) | n/a | 0900-0915 |
| **60** | WP6 | Updated genetic mixture analysis of Nunavik beluga summer stocks to inform population models and harvest allocation (T. Doniol-Valcroze, M.O. Hammill and L. Postma) | Jack.Lawson | 0915-1015 |
| **15** | | break | | 1015-1030 |
| **90** | WP1a | Instantaneous availability bias estimation for calculating aerial survey abundance estimates for narwhal (Monodon monoceros) in the Canadian High Arctic (Cortney A. Watt, Natalie C. Asselin, Jack R. Orr, and Steven H. Ferguson) | Jack.Lawson | 1030-1200 |
| **1.5 hrs** | | Lunch | | 1200-1330 |
| **15** | n/a | Brief introduction to the 2013 high arctic cetacean survey | | 1330-1345 |
| **75** | WP1b | Identification of duplicate sightings from the 2013 double-platform High Arctic Cetacean Survey (D. Pike, T. Doniol-Valcroze) | Nell.denheyer | 1345-1500 |
| **15** | | break | | 1500-1515 |
| **60** | WP1c | Spatial modelling of narwhal density in Baffin Bay fiords during the 2013 High Arctic Cetacen Survey (HACS) (T. Doniol-Valcroze, J.-F. Gosselin, D. Pike, J. Lawson) | Nell.denheyer | 1515-1615 |

## DAY 2 - Tuesday October 21

| Time allowed (mins) | Paper # | Topic | Rapporteur | Time Guideline |
|---|---|---|---|---|
| **90** | WP1d | Abundance estimates of Baffin Bay narwhal stocks in Canadian waters based on the 2013 High Arctic Cetacean Survey (T. Doniol-Valcroze, J.-F. Gosselin, D. Pike, J. Lawson) | Linda Nichol | 0900-1030 |
| **15** | | break | | 1030-1045 |
| **60** | WP2a | Instantaneous availability bias correction for calculating aerial survey abundance estimates for bowhead whales (Balaena mysticetus) in the Canadian High Arctic (Cortney A. Watt, Bernard Leblanc, and Steven H. Ferguson) | Linda Nichol | 1045-1145 |
| **1.5hrs** | | Lunch | | 1145-1315 |
| **90** | WP2b | Abundance estimate of the Eastern Canada – West Greenland bowhead whale population based on the 2013 High Arctic Cetacean Survey (T. Doniol-Valcroze, J.-F. Gosselin, D. Pike, J. Lawson)<br>N. Asselin, K. Hedges, S. Ferguson) | Sheena Majewski | 1315-1445 |
| **15** | | break | | 1445-1500 |
| **120** | WP2c | Abundance estimates of the Eastern Canada-West Greenland bowhead whale (Balaena mysticetus) population based on genetic mark-recapture analyses (Timothy R. Frasier, Stephen D. Petersen, Lianne Postma, Lucy Johnson, Mads Peter Heide-Jørgensen, and Steven H. Ferguson) | Sheena Majewski | 1500-1700 |

## DAY 3 - Wednesday October 22

| Time allowed (mins) | Paper# | Topic | Rapporteur | Time Guideline |
|---|---|---|---|---|
| **90** | WP5 | Sustainability of a flexible system of total allowable annual catches of narwhals (Monodon monoceros) (P. R. Richard and R. Young) | Hilary Moors-Murphy | 0900-1030 |
| **15** | | break | | 1030-1045 |
| **30** | WP5 | Continued… | Hilary Moors-Murphy | 1045-1115 |
| **1.5hrs** | | Lunch | | 1115-1245 |
| **90** | WP3 | Trends in the abundance and distribution of sea otters (Enhydra lutris) in British Columbia updated with 2013 survey results (Linda M. Nichol) | Lianne Postma | 1245-1415 |
| **15** | | break | | 1415-1430 |
| **90** | WP4 | Recent Trends in Abundance of Steller Sea Lion (Eumetopias jubatus) in British Columbia (Sheena P. Majewski) | Lianne Postma | 1430-1600 |

## DAY 4 - Thursday October 23

| Time allowed (mins) | Paper# | Topic | Rapporteur | Time Guideline |
|---|---|---|---|---|
| **90** | SAR1 | BB narwhal stock abundance SAR | n/a | 0900-1030 |
| **15** | | break | | 1030-1045 |
| **30** | SAR1 | Continued… | n/a | 1045-1115 |
| **60** | SAR3 | Recent trends in the abundance and distribution of sea otters in British Columbia, based on 2013 survey | n/a | 1115-1215 |
| **1.5 hrs** | | Lunch | | 1215-1345 |
| **90** | SAR2 | EC-WG Bowhead whale abundance estimate (from gCMR AND visual survey) | n/a | 1345-1515 |
| **15** | | break | | 1515-1530 |
| **30** | SAR2 | Continued… | n/a | 1530-1600 |

## DAY 5 - Friday October 24

| Time allowed (mins) | Paper# | Topic | Rapporteur | Time Guideline |
|---|---|---|---|---|
| **60** | SAR5 | narwhal harvest credit system | n/a | 0900-1000 |
| **15** | | break | | 1000-1015 |
| **90** | SAR4 | Population status assessment for the Steller Sea Lion | n/a | 1015-1145 |
| **1.5 hrs** | | Lunch | | 1145-1315 |
| **60** | SAR6 | Nunavik beluga | n/a | 1315-1415 |