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# Proceedings on the Regional Peer Review of the Evaluation of Data and Model Assumptions on the Calculation of Management Parameters using the Pacific Herring Assessment Model (ISCAM)

# Compte rendu de la réunion d'examen régional par les pairs de l'Évaluation des données et des hypothèses du modèle concernant le calcul des paramètres de gestion à l'aide du modèle d'évaluation du hareng du Pacifique (MSIPA)

**June 27 & 28, 2012**
**Nanaimo, British Columbia**

**Du 27 et 28 juin 2012**
**Nanaimo, Columbie-Britannique**

**Meeting Chairperson:**
**Linnea Flostrand**

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## Foreword

The purpose of these Proceedings is to document the activities and key discussions of the meeting. The Proceedings include research recommendations, uncertainties, and the rationale for decisions made by the meeting. Proceedings 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.

## Avant-propos

Le présent compte rendu a pour but de documenter les principales activités et discussions qui ont eu lieu au cours de la réunion. Il contient des recommandations sur les recherches à effectuer, traite des incertitudes et expose les motifs ayant mené à la prise de décisions pendant la réunion. En outre, il fait état de données, d’analyses ou d’interprétations passées en revue et rejetées pour des raisons scientifiques, en donnant la raison du rejet. Bien que les interprétations et les opinions contenues dans le présent rapport puissent être inexactes ou propres à induire en erreur, elles sont quand même reproduites aussi fidèlement que possible afin de refléter les échanges tenus au cours de la réunion. Ainsi, aucune partie de ce rapport ne doit être considérée en tant que reflet des conclusions de la réunion, à moins d’indication précise en ce sens. De plus, un examen ultérieur de la question pourrait entraîner des changements aux conclusions, notamment si l’information supplémentaire pertinente, non disponible au moment de la réunion, est fournie par la suite. Finalement, dans les rares cas où des opinions divergentes sont exprimées officiellement, celles-ci sont également consignées dans les annexes du compte rendu.

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## SUMMARY

These Proceedings summarize the relevant discussions and key conclusions that resulted from a Fisheries and Oceans Canada (DFO), Canadian Science Advisory Secretariat (CSAS) Regional Advisory meeting held June 27 and 28 at the Pacific Biological Station in Nanaimo, B.C. One working paper focusing on Pacific herring stock assessment data and model assumptions and consideration of management parameters was presented for peer review.

In-person participation included Fisheries and Oceans Canada (DFO) Science and Fisheries and Aquatic Management Sectors staff; and external participants from First Nations organizations (the Haida First Nation, the Heiltsuk First Nation), the commercial fishing sectors (the Herring Conservation Research Society), the province of British Columbia, environmental non-governmental organizations, and academia (University of British Columbia). No remote (webinar) participation occurred for this meeting.

The Working Paper was accepted but several key suggestions were provided during the review which should be addressed in revisions prior to publication as a Research Document. Many of the conclusions and advice resulting from this review are intended for incorporation into future Pacific herring stock assessment and advisory documents. The Research Document resulting from the current Working Paper, when published, will be made publicly available on the CSAS Science Advisory Schedule at http://www.dfo-mpo.gc.ca/csas-sccs/index-eng.htm.



## SOMMAIRE

Le présent compte rendu résume l'essentiel des discussions et conclusions de la réunion régionale consultative de Pêches et Océans Canada (MPO) et du Secrétariat canadien de consultation scientifique qui a eu lieu les 27 et 28 juin à la station biologique du Pacifique de Nanaimo, en C.-B. Un document de travail présentant des hypothèses du modèle et des données quant à l'évaluation des stocks de hareng du Pacifique ainsi que des considérations relatives aux paramètres de gestion a été présenté aux fins d'examen par les pairs.

Au nombre des participants qui ont assisté à la réunion en personne, il y avait notamment des représentants des secteurs des sciences et de la gestion des pêches et de l'aquaculture du MPO ainsi que des représentants externes d'organisations des Premières Nations (Premières Nations Haïda et Heiltsuk), du secteur de la pêche commerciale (Herring Conservation Research Society), de la Province de la Colombie-Britannique, d'organisations non gouvernementales vouées à l'environnement et du milieu universitaire (Université de la Colombie-Britannique). Il n'est pas possible de participer à cette réunion à distance (webinaire).

Le document de travail a été accepté, mais plusieurs suggestions clés ont été faites au cours de l'examen et devront être prises en compte durant la révision précédant la publication en tant que document de recherche. Bon nombre de conclusions et de conseils découlant de cet examen devraient être intégrés aux futurs documents consultatifs et documents d'évaluation des stocks de hareng du Pacifique. Au moment de sa publication, le document de recherche découlant de l'actuel document de travail sera rendu public dans le calendrier des avis scientifiques du SCCS, à l'adresse suivante : http://www.dfo-mpo.gc.ca/csas-sccs/index-fra.htm.

# INTRODUCTION

A Fisheries and Oceans Canada (DFO) Canadian Science Advisory Secretariat (CSAS), Regional Advisory Process (RAP) meeting was held on June 27 and 28 at the Pacific Biological Station in Nanaimo to review 1) recent methodological refinements to the Pacific assessment model and impacts from data assumptions and structural changes, and 2) new approaches for the provision of science advice that incorporates probability values to inform risk-based decision-making. The Terms of Reference (TOR) for the science review (Appendix A) were developed in response to a request for advice from *Fisheries Management*. Notifications of the science review and conditions for participation were sent to representatives with relevant expertise from First Nations, commercial and recreational fishing sectors, environmental non-governmental organizations, academia, the provincial government and DFO science and fisheries management sectors. A meeting agenda (Appendix B) and working paper (WP) were also prepared and made available to meeting participants prior to the meeting (WP summary Appendix C).

**Working Paper**: “Evaluation of data and model assumptions on the calculation of management parameters using the Pacific herring assessment model (ISCAM)” *by Jaclyn Cleary, Vivian Haist, and Steven Martell (CSAP WP2012-P07)*

**Part 1**: Improve understanding of the impacts of data assumptions and recent structural changes to the herring stock assessment

**Part 2**: Risk-based decision tables for the provision of science advice for BC herring stocks

The meeting Chair, Linnea Flostrand, welcomed participants and invited all participants to introduce themselves and their affiliation. The Chair reviewed the role of CSAS in the provision of peer-reviewed advice, and gave a general overview of the CSAS process, including the role of participants and applicable publications (Proceedings and Research Document). In addition, the process around achieving consensus decisions and advice was described. Everyone was invited to participate fully in the discussion and to contribute knowledge to the process, with the goal of delivering scientifically defensible conclusions and advice.

The Chair also went over the ground rules and process for exchange, reminding participants that the meeting was a science review and not a consultation. Members were reminded that everyone at the meeting had equal standing as participants and they were expected to contribute to the review process if they had information or questions relevant to the paper being discussed. In total, 31 people participated in the review (Appendix D). Jennifer Boldt, Roger Kanno and Peter Midgley were identified as the Rapporteurs for the meeting.

The Chair reviewed the Terms of Reference for the meeting and invited feedback on the Agenda, whereby it was suggested and agreed upon that the format for reviewing Part 1 of the paper would be changed to review topics section by section in order presented in the WP. Therefore, for each section of Part 1, feedback from formal reviewers would precede general discussion followed by the development of conclusions and recommendations.

Participants were informed that Robyn Forrest and Gary Melvin had been asked before the meeting to provide detailed written reviews for the working paper to assist everyone attending the peer-review meeting. Participants were provided with copies of the written reviews (Appendix E).

# REVIEW

## PART 1 - EVALUATION OF DATA AND MODEL ASSUMPTIONS

Authors: Jaclyn Cleary, Vivian Haist, and Steven Martell. WP2012-P07
Rapporteur: Jennifer Boldt
Presenter: Jaclyn Cleary

### Presentation

The primary objective of Part 1 is to improve the understanding of the impacts of data assumptions and recent structural changes to the stock assessment model (ISCAM) on the calculation of current biomass ($B_t$) and biological reference points ($B_0$, $B_{MSY}$). This paper presented findings on the following four topics:

1.1) Spawn survey proportionality coefficient, q: sensitivity analyses of prior values for q and influence of these assumptions on estimates of $B_t$, $B_0$ and $B_{MSY}$.

1.2) Gillnet selectivity: further evaluation of changes to parameterization of gillnet selectivity, interactions with q, and the influence of these changes on estimates of $B_t$, $B_0$ and $B_{MSY}$

1.3) Age-class pooling: sensitivity analysis and alternate proportions for pooling of age classes.

1.4) PRD data: to examine the positive retrospective bias for the PRD stock

The four topics were presented consecutively with points of clarification addressed after each topic.

### Points of clarification

1.1) Spawn survey proportionality coefficient, q

1. Clarification was sought on what q represents. Response: it is the ratio of observed spawn from surveys to the model predicted estimate of the total spawn.

2. There was confusion regarding contradicting statements: results indicating "…little implication on SSB depletion…" (Section 1.3.1.2) and the conclusion that "Scenario A is the most conservative" (Section 1.4). It was pointed out that in Scenario A the biomass is lower but the influence on $B_0$ and the pattern in depletion is similar. Depletion is lower in all cases. The wording of this in the WP needs clarification.

3. Paper states "q2==1 is conservative" but it was suggested that term conservative should be replaced with the term "biased". Authors agreed biased is more appropriate wording.

4. Clarification was provided regarding what parameters are time-varying. The parameters q1 and q2 are updated annually.

5. In response to a concern that egg loss data, used for developing q prior, was out-of-date, it was noted the research was published ~ 14 years ago.

1.2) Gillnet selectivity and interactions with q

1. What is the cause of the decline in weight at age? There has been a decrease in weight at age in herring from Alaska to California and in areas that are closed to fishing. Since the decrease in weight-at-age is seen on such a large spatial scale, it is thought to be due to a large-scale, ecosystem-wide driver. A decline in weight-at-age has also been observed in other species.

2. Why was there no age-based scenario with the old q (=1)? Response: Scenarios A-D addressed the requested TOR and Scenario E was included for interest's sake. It seemed that effects of covariates were small.

3.  When reporting changes in key model parameters (as a result of changing q), authors should be cautious when stating the importance of a 10% change in depletion (top of page 27).  A 10% change might translate to a considerable amount of tonnage associated with quota.

4.  It was questioned whether modeling should ignore changes in weight at age. Response: it was ignored in the approach here because it is thought that future work needs to address more important issues regarding changes in gillnet selectivity.   How gillnets have been fished has changed over time so it was suggested to move towards having time varying gillnet selectivity. Changes in weight at age will be taken into account through that approach (comparisons of annual selectivity estimates).

<u>1.3) Age class pooling</u>

1.  It was noted that implications of pmin estimation is relevant to other stocks/species assessments and is dependent on sample data and life history. Simulation work is likely the best way to find an appropriate pmin value for a specific stock.

<u>1.4) PRD retrospective bias</u>

1.  Why was the first year in time series 1972?  Response: Prior to 1972 the herring fishery was an interception fishery whereas after 1972 it was a terminal fishery (the roe fishery started in 1972); data prior to 1972 were based on reduction fishery.

2. The change from an interception to a terminal fishery was not consistent with changes in productivity; therefore maybe some catches are being included that shouldn't be? It was acknowledged that prior to 1972 there are questions as to accuracy of capture sites associated with the PRD (i.e. catches between stocks).

3.  It was pointed out that there has been difficulty getting seine test samples from parts of the PRD because the fish stay deep until they move shallower to spawn, therefore test fishery data may be biased.  Future work could examine the effect of excluding test fishery data from this area.

4.  It was asked whether there are explanations for the sharp decline in biomass in 2004. Response: no apparent cause was identified.

<u>Other points of clarification</u>

1.  What is the extent of catches being under reported or not included in the time series?

Response:  Food and bait and special use catches are included in catches but spawn in kelp (SOK) are not. SOK harvests have been relatively large in certain areas in the past but recently harvests are smaller, sometimes SOK is the only herring fishery due to other fishery closures.  There are differences in open vs. closed SOK ponding but no modeling approach to account for fish use in these two types of fisheries. In open pond SOK fisheries, the eggs are removed and the spawners are free; in closed pond SOK fisheries there is some adult mortality from handling.  Modeling these factors will not be trivial and requires statistics on the number of closed ponds and open ponds and fish mortality, etc.

2.  Have there been any adjustments to landings to account for underestimates of what was actually landed?  Response: No, there have been no corrections.  Currently, all catch is validated at the dock.  During the reduction fishery, there may have been misreporting of catch locations.

3.  Does "no catch" imply that fishing mortality was not modeled, ($F = 0$)? Response: yes.

4.  It was confirmed that each stock region has a separate sampling program.

5.  How different are the surface and dive spawn survey methodologies and were their overlaps for comparisons in the time series that can be included into modeling?  Response: Both surface and dive surveys estimate length of spawn; however, the methods to estimate density and width of spawn are different between the two surveys.  In surface surveys, the offshore extent of spawning width and density are estimated using a grappling hook; whereas, during dive surveys, divers have

a sampling design and they are in the water. Some 1998 dive surveys overlapped with surface surveys but the model does have the abrupt switch between the two survey methodologies. The widths of spawn were underestimated by surface surveys in areas with steep depth gradients. Adjustments are made to the widths of spawn from surface surveys. The two q's are adjusted to account for the fact that the correction is not perfect.

**Formal reviews and discussions**

See also Appendix E for written reviews

General suggestions and comments

1. The next assessment report should provide a point-by-point response to previous reviewer feedback.

2. The GN selectivity section (1.2) needs more description of model scenarios and results.

3. For describing different model runs and case scenarios, it would be beneficial if unique scenario names were applied and the definition of "Base" case represented each scenario most favoured by authors.

4. The paper lacks a concluding section reinforcing key outcomes, conclusions and author recommendations.

5. Full range of uncertainty should be incorporated in Part 2 risk probabilities advice (reflecting different model options and possibilities). Avoid choosing scenarios where there is poor evidence for any superior model formulations.

1.1) Spawn survey proportionality coefficient, q

Reviewers agreed q must be <1. It was asked whether the informative prior for q is appropriate for all areas, years and depths. An area specific prior may be considered for future assessments. In the meantime perhaps uncertainty could be addressed by broadening the standard deviation of the prior.

It was asked why a 20 year horizon was chosen for estimating the allocation to each fishery for calculating reference points. Response: Authors looked at extremes and found no large impact on $B_{msy}$ or exploitation fraction, etc.

With respect to the confounding of parameters: there were local minima that were similar in likelihood to other minima found. The reviewer suggested authors show trace plots of individual parameters and some derived parameters being estimated. The reviewer also suggested including in the paper: autocorrelation and pairs plots, information on issues of getting convergence and how well parameters are being estimated.

There was discussion about the appropriateness of the prior. It was noted that when the estimate of q1 is updated from a prior value the data are providing information about the value of q but in other cases q is not being updated. One should look at confounding parameters and consider broadening prior standard deviation. Weight on age-structure can be dealt with separately; broadening standard deviation will result in a result similar to Scenario C; provides freedom to move parameter away from prior if that improves fit; this is an acknowledgement that there is little information in the data. It was noted that for SOG and WCVI the base scenario is similar to the uniform scenario and estimates of q are similar to the informed prior for these cases. It was questioned whether the data are more informative on q prior for these stocks? Response: Unlike the SOG, the WCVI is difficult to survey due to weather and logistics and has greater constraints on informative data therefore for WCVI results reflect q prior input. In the SOG, where the estimate of q is similar to the prior, the data have information. Suggestion was made to take a closer look at PRD to determine how q changes in relation to the prior (analysis allows q to be greater than 1). The reviewer suggested including transparent posterior envelopes to Figure 1.1 page 15 and the caption for Table 1.2 needs to clarify MPD.

It was emphasized that the largest change in output is between fixed and estimated q2. Range of q values is quite large (and the inverse of these are the multipliers of biomass, so they can have significant impact on output). The question was asked whether q should be bounded by 1 and it was noted that there are cases where q>1 in Table 1.2. Response: It is difficult to add a bounding value; as q approaches a value of 1, the MPD will also approach a value of 1, so there will be convergence issues in the MCMC. Conceptually, q cannot be >1, but statistically it can be >1. The reason q can be >1 is due to the conversion of spawn to biomass using 100 eggs/gram; minor changes in relative fecundity could change, thereby inflating q. Also, q is an average value. It was stated that when q >1, it helps identify issues in the assessment and by fixing q at 1, it may hide those issues. The prior on q is based on information (egg loss rates, etc) and how the prior is developed is more important than fixing its value. It was suggested that perhaps there need to be stock-specific research on q-estimates.

$F_{msy}$ estimates were noted as being high, with some values greater than 1 and 2, which is consistent with a short lived species undergoing recruitment overfishing. Assuming fish are fully mature at age 3 and gear selectivity is shifted to the right (i.e. spawning prior to being fully available to gillnet fishery) that would also be consistent with recruitment overfishing (but it was not suggested that this is occurring). Estimates of M are also high. Response: An age-based selectivity with a weight offset was used which may also result in high $F_{msy}$.

For the priors in Table 1.2, how are the egg loss data relevant and why not consider Scenario B (uninformative prior), pending updated egg loss studies? Response: The results do not provide a means to choose between the 4 scenarios.

A suggestion was made to further investigate sources of confounding effects among parameters.

PRD has low M and high q values, so there could be something going on. It is likely that q is confounded and may be difficult or impossible to resolve but pairs plot from base model might be useful. It was emphasized that all known uncertainty related to an assessment should be presented to help managers consider what to do, which may include output from various scenarios.

**<u>Conclusions and recommendations - Spawn survey proportionality constant</u>**

1. Sensitivity analyses presented here support continued use of Bayesian prior (approved and implemented in September 2011) for the September 2012 assessment. Assessment authors should consider broadening the standard deviation on the prior for future assessment to account for uncertainty associated with q prior.
2. Provision of pairs plots of posterior parameter estimates in future research documents was recommended to check for confounding among parameters.

<u>1.2) Gillnet selectivity and interactions with q</u>

More background information was sought on rationale for applying an age-based, weight-covariate scenario, and why did authors choose the two different selectivities? Response: More contrasting / comparative output was sought and authors wanted to look at parameterization used since the 1980s (weight-based). Last year, one of the authors developed an age-based with weight-covariate component to incorporate a size function. Before that, a weight at age function was initially applied to selectivity at a time when declines in weight at age were not detected and when there was a minimum gillnet mesh size (applied to exclude smaller fish). Regulations now exclude mesh restrictions so selectivity has changed. The current analysis examined the effect of this change in model output.

Clarification was sought on the mu and sigma definitions (whether they are estimated or have priors) and the lambda results. Response: Lamda is an estimated parameter (uniform prior) added so that there is an effect from changing weight at age but it is still basically an age-based approach. It was suggested that lambda values be included in Table 2.2.

It was noted that, because the selectivity curve shifts to the right (Fig 2.2), it appears that some fish are not available to fishery but the model has to explain catch somehow, so it inflates biomass estimates. A reviewer suggested the text should clarify what is being compared (p. 28 selectivity comments) and elaborate on the significance of the selectivity curves.

A reviewer suggested changing the term “Base” to “status quo” to describe relevant scenarios and she reiterated that rationale for choosing the desired scenario needs more clarity. Reviewer did not agree with author’s recommendation about (age-based) selectivity because age-based is smoothing and removing between year variability. Figure 2.2. indicates there is variability that will not be captured with the age-based parameterization. Reviewer emphasized that changes in mesh size will have an impact on selectivity and that gillnet selectivity issues have not been addressed.

Factors affecting data quality and modeling error were identified: 1) possible fish weight errors in the time series as a function of preservation, reduction fishery, brining, freezing etc; 2) the duration of the gillnet pool fishery is longer and the pool fishery may have fewer boats; 3) gillnet fishing often starts with large meshes and then switches during the fishery to try to increase catch effort; (gear used varies within season and among years); 4) effects of sex (model does not take into account variation in sex ratios and size differences), and, 5) effects of changes in relative ovary size, which there is evidence for and which would affect roe yield. The models assume stationarity in these issues. Can samples taken from gillnet fishery samples be excluded? Response: gillnet removals need to be accounted for (fishing mortality) but selectivity could be excluded from modeling although this is not a trivial issue.

No preference was shown for age-based or age-based selectivity with weight covariate and meeting participants did not think this was a high priority issue. There was some debate about whether weight or length would be better indicator of size for the analysis. A suggestion was also made to reject weight-based parameterizations because fishing behaviour has changed and fish have become smaller. It was suggested that report should include more information and discussion on these likely effects.

There was discussion about how future Management Strategy Evaluation work could focus efforts on testing harvest control rules that people can agree on, and how they perform under different scenarios and incorporate the uncertainty into decision making framework, rather than running different scenarios.

**<u>Conclusions and recommendations - Gillnet selectivity (and interactions with q)</u>**

3. The sensitivity analysis presented here supports continued use of the age based, weight covariate parameterization for gillnet fishery selectivity that was used and implemented in 2011.
4. It was noted that changes in gillnet selectivity would be expected, since changes in the prosecution of the fishery are not reflected in this assessment.
5. While not considered a priority for further model development at this time, suggestions were made to explore alternate treatment of the data including:
    a. Considering time-varying selectivity (potentially caused by changes in weight data quality, fishing methods, and gear types, changes in ovary size).
    b. Treating males and females separately.

<u>1.3) Age class pooling: sensitivity analysis</u>

A reviewer noted that pmin = 0.02 was used in hake assessments but this binning was discontinued since the model produced estimates of large cohorts in years that were not believed to exist. The reviewer suggested that authors think more about whether age classes should be binned for herring (method was originally developed for longer lived rockfish with many age classes). Response: binning for herring tends to occur at older age classes, so likely does not miss strong cohorts.

A reviewer asked why there was such high variation for pmin scenarios and suggested that perhaps statistical properties of binning may not be appropriate for herring. Response: For WCVI example (pmin=0), the multivariate logistic does not fit as well as multinomial (predicted vs. observed proportions). Multivariate logistic fits better at small proportions. An author also showed comparisons of multivariate and multinomial outputs with pmin=0.02, and the difference was small.

Clarification was sought on how model deals with binning and multiple fisheries. Response: binning is done on predicted and observed ages for each fishery in each year.

When age class proportion is zero, is it due to fishery or due to sampling? Response: There could be a number of things going on, but generally there are relative few fish aged at 8, 9, and 10 in samples.

A reviewer suggested taking the likelihood values out of Table 3.1 because they are not comparable.

A paper published by Chris Francis was referred to (Francis 2011) on weighting of age composition data. The paper was said to recommend down weighting of age composition data, thus changing the relative weight between biomass index data and age composition data. Some participants suggested that a multinomial likelihood was more defensible and removes the pmin issue. Additional work is required to resolve this issue.

Suggestions for future work to address binning effects included: using a lower plus-bin; using a multinomial likelihood; and using a multinomial likelihood with weighting. This could be examined through simulation work. Future work to include the multinomial was suggested because it fits the data better and produces results that are similar to the multivariate when pmin = 0.02. It was also noted that examining posterior intervals may be useful. The current analyses show that results are sensitive to pmin but that pmin=0.02 produces similar results to the multinomial. It was recommended to proceed with what was approved in September 2011 (pmin=0.02).

**<u>Conclusions and recommendations - Age class pooling sensitivity analysis</u>**

6. For September 2012 assessment, the methods used for September 2011 assessment (pmin 0.02) are recommended as interim measures because there is no objective basis for selecting a pmin given current information.
7. Sensitivity analyses show that biomass estimates are sensitive to changing pmin with multivariate logistic likelihood.
8. Future work is recommended to evaluate (through simulation) statistical properties against alternate age class modeling options.

<u>1.4) Retrospective bias in Prince Rupert District biomass estimates</u>

It is difficult to find reasons for retrospective bias and authors were commended for looking at this and finding some potential reasons. There were some interesting results from truncating time series and separating data from spatially different areas for gillnet and seine fisheries but no specific reason can be provided for the discontinuity in 2004. It was suggested that gillnet selectivity may be causing the problem.

Is there any merit to splitting PRD into two geographical areas? Is it realistic? Response: Spawn timing and habitat are different and separated by Skeena River discharge and there has been speculation that Port Simpson and Big Bay (Areas 3 and 4) are closely associated with Ka'shakes (Alaska stock), but it hasn't been confirmed.

A question was posed about the possible benefits of omitting either of the PRD biological sample data sets? Response: It is possible. Seine gear is relatively non-selective and gillnets have issues with selectivity varying over the time series. It was noted that there is difficulty in getting purse seine samples from Areas 3 and 4 because fish stay in deep water where they are difficult to sample until just prior to spawning.

Some data in Table 4.4 were identified to be questionable, some samples might relate to SOK.

Concerns over PRD retrospective bias are reduced because there is no indication of retrospective bias in biomass estimates for 2007 and after; therefore, current modelling generally believed to be appropriate.

There was some discussion about the possibility of treating PRD as two separate stocks and about excluding the 1976 winter fishery data from Base model. It was also noted that parts of the PRD may be part of a larger stock with Alaska Ka'shakes (which has also declined).

**<u>Conclusions and recommendations - Retrospective bias in PRD biomass estimates</u>**

9. Biomass estimates from 2007-2011 do not show retrospective pattern; therefore concerns raised during 2011 assessment about overestimation of current biomass appear to be resolved.
10. Application of the current modelling approach is recommended for the September 2012 assessment.
11. It was recommended that the time series prior to 1970 (winter fishery data etc) be excluded from the Base model due to uncertainty in catch locations.
12. Future work is recommended to explore treating Areas 3 and 4 versus Area 5 as separate stock units because separating PRD by gear and area (Areas 3 and 4 for gillnet versus Area 5 for seine) greatly reduced the retrospective pattern in the time series before 2007.

## PART 2 – RISK-BASED DECISION TABLES FOR THE PROVISION OF SCIENCE ADVICE FOR BC HERRING

| | |
|---|---|
| Authors: | Jaclyn Cleary, Vivian Haist, and Steven Martell. WP2012-P07 |
| Rappoteur: | Roger Kanno and Jennifer Boldt |
| Presenter(s): | Jaclyn Cleary |

**<u>Presentation, points of clarification and discussion</u>**

Authors invited clarification and discussion during their presentation and a collective summary of this information is provided below. One reviewer, Robyn Forrest, with regrets, was unable to participate in discussions due to illness.

Jaclyn Cleary reviewed the Terms of Reference and stated that the intent of Part 2 of the paper was to discuss the policy, background, current approach, including decision tables and risk metrics for the provision of science advice as there was some confusion in the interpretation and utility of risk metrics proposed in the 2011 SAR. Suggested discussion for the meeting included consideration of candidate management objectives (which were not a focus of the paper) and alternate risk metrics.

Figures were presented (Appendix F) to illustrate the current herring Harvest Control Rule (HCR) and how it compares to the DFO default (using the Strait of Georgia example). Some people preferred the word "lower" over "conservative" when comparing management frameworks. It was suggested that authors should include these figures in the report to aid the evaluation of reference points. Although required by the Precautionary Approach (PA), Limit Reference Points (LRP), Upper Stock References (USR) and Target Reference Points (TRP) have not been established for Pacific herring stocks. A comment was made that policy (PA) and reference points appear to be based on the concept that harvest is driving the population whereas factors other than commercial herring fishing are believed to be preventing areas from rebuilding.

Hypothetical differing selectivity curves (Appendix F) were presented to demonstrate how different gear types (i.e. food and bait, roe seine and roe gillnet) target different age classes of fish. But modelling of these effects requires prior knowledge of the allocations to each gear type as it affects the calculation of MSY-based metrics and probabilities.

There was some discussion around development of reference points when different productivity periods are suggested in the time series and what are “irreversible harm” and “recovery periods” and how recovery strategies can be applied with regards to PA. The PA framework was said to define the LRP as the level below which there is a high probability that productivity will be so impaired that serious harm will occur, but “serious harm” could be interpreted in terms of levels of biomass or the length of time to return to a given biomass etc. To identify a reference limit in the recovery context, suggestions included using a shorter time series of data as they have with Fraser River salmon (e.g. only the last 20 years of data instead of the last 100 years) or arbitrarily choosing a representative time period when biomass estimates were considered at acceptable levels from which stocks could recover. Examples of empirical approaches that have been used to define reference points were provided: New Zealand invertebrate (rock lobster and abalone) fisheries and Canadian Maritimes herring. The Maritimes herring example relates to development of a Limit Reference Point (LRP) based on observed recent minimum estimates of herring biomass from acoustic surveys (Clark et al 2012; DFO 2012).

The topic of rebuilding and re-opening strategies was discussed in terms of the recent fishery closures for 3 stock areas over the last 5-10 years (HG, CC and WCVI). Authors suggested an interim re-opening level of 0.35%Bo, which could be implemented with the goals of observing biomass estimates above cutoff for a defined time period. They suggested this work could be done in association with risk analysis/simulation to assess whether the stock will decrease.

One of the reviewers noted that if changes to the model decrease Bo or increase the scale of biomass, this will make harvest control rules less precautionary. He further stated that the status quo on the east coast of Canada is that when models change, reference points calculated with new models can be considered invalid and new models are not generally accepted unless there is a re-examination of reference points. The point was raised as to whether we can ever get back to Bo in current conditions if there has been a productivity shift in the closed areas. The HG, CC and WCVI have been closed to commercial fishing and they are not rebuilding so there are other pressures limiting these stocks and there is a need to be precautionary. No recommendation was made to adopt 35%Bo as a reference point.

Central Coast First Nations representation raised concerns that because of uncertainty and depressed stocks in the region that they are not meeting their need for Food, Social and Ceremonial (FSC) fisheries. To address uncertainty over depressed stocks, they are collaborating with academia on their own science. There was also concern shown over whether a 0.25Bo cutoff is still appropriate for CC if that value decreases with changes to the assessment model.

The uncertainty associated with the causes of low productivity and the need for an assessment to determine appropriate rebuilding and harvest strategies prior to reopening fisheries presented in the 2011 SAR were reiterated. There was discussion about the length of time that a stock needed to exceed an abundance threshold before it could be re-opened to commercial fishing but it is was agreed that this was a topic outside of the scope of this review.

The need for a work plan to identify management goals and calculate useful reference points and risk metrics (e.g., include shorter time series, etc) was identified. Someone emphasized that decision tables only include uncertainty arising from one of many plausible models. There was a sense that MSY values for Pacific herring are not appropriate and since they are not currently in use, probably shouldn’t be included (and PA doesn’t require them to be). Management showed interest in seeing metrics describing next year’s forecasts relative to last year’s biomass estimates, as well as longer historical trends, such as presented as a ratio of Bo (depletion). Concern was shown over “decision table” terminology and it was explained that the terminology comes from statistical literature.

Parameters identified for consideration for September 2012 Decision Tables: 1) P(SB2012<SB2011); 2) P(SB2011<0.25B0); 3) P(U_3+2012>20%), and 4) trend over most recent 5 years.

To consider variability associated with low and high productivity eras, it was suggested that posterior distributions of biomass estimates could be divided into thirds (poor, average, good) and management could refer to the outcomes of different ranges in the distributions to consider difference productivity regimes. Information on productivity regimes could come from published literature and recruitment trends. A response to this was that Management would prefer to see a reduced number of tables and not advice scenarios divided into thirds. This will be an iterative process between Science and Mgt and can be discussed before the September assessment review meeting.

The need for a management strategy evaluation (MSE) work plan was emphasized. Management objectives are needed to help set up other reference points and metrics but further work and stakeholder process is needed. Concerns were raised over which of the risks Management should be most concerned about. The decision tables will provide probabilities but Risk = probability x severity and severity was not included. There was discussion of whether to include new versus old Bo reference points for September assessment and it was decided that both should be considered.

PRD catches include catch of 2 year olds which provide an indicator of upcoming recruitment. This is currently not in the assessment, but is included in the Decision Tables. Why use long-term average recruitment for HG, CC, and PRD? Perhaps it would be better to use a more recent time series average (for low productivity era).

<u>Minor editorial suggestions:</u>

Page 9, 2nd paragraph, remove last 2 sentences.

Section 5.1 refers to PA draft rebuilding guidelines (currently in approval process).

Table 3 (Page 13) numbers need to be verified: 2010 TAC was 10,000 (not 5,000); and clarify that these are Science recommended/simulated TACs (not Management approved TACs).

## Conclusions and recommendations

13. Interim and long-term work plans related to developing management frameworks and candidate Precautionary Approach reference points are required.
14. For consideration for the September 2012 assessment, dialog between science and management prior to that meeting needs to occur to discuss the role of the three decision table metrics thought to be most relevant: P(SB decline), P(SB < 0.25Bo) and P(U > 20%).

## ACKNOWLEDGEMENTS

A special thank you to both Robyn Forrest and Gary Melvin for providing detailed and thorough formal reviewers that assisted in clarifying and evaluating technical information during the meeting. The assistance of Jennifer Boldt, Roger Kanno, Paul Starr and Peter Midgley is greatly appreciated for being rapporteurs or being willing to clarify details from the provision of their notes. Nic Dedeluk also deserves a big "thank you" for helping circulating meeting material and helping with other meeting logistics.

## REFERENCES

Clark, D.S., Clark, K.J., Claytor, R., Leslie, S., Melvin, G.D., Porter, J.M., Power, M.J., Stone, H.H., and Waters, C. 2012. Limit Reference Point for Southwest Nova Scotia / Bay of Fundy Spawning Component of Atlantic Herring, Clupea harengus (German Bank and Scots Bay). DFO Can. Sci. Advis. Sec. Res. Doc. 2012/025. iii + 14 p. http://www.dfo-mpo.gc.ca/Csas-sccs/publications/resdocs-docrech/2012/2012_025-eng.pdf

DFO. 2009. A fishery decision-making framework incorporating the Precautionary Approach. http://www.dfo-mpo.gc.ca/fm-gp/peches-fisheries/fish-ren-peche/sff-cpd/precaution-eng.htm

DFO. 2012. Reference points consistent with the precautionary approach for a variety of stocks in the Maritimes Region. DFO Can. Sci. Advis. Sec. Sci. Advis. Rep. 2012/035. http://www.dfo-mpo.gc.ca/csas-sccs/Publications/SAR-AS/2012/2012_035-eng.pdf

Francis, C. 2011. Data weighting in statistical fisheries stock assessment models. Can J. Fish. Aquat. Sci. 68: 1124-1138. http://www.nrcresearchpress.com/doi/pdf/10.1139/f2011-025

# APPENDIX A: TERMS OF REFERENCE

## Evaluation of Data and Model Assumptions on the Calculation of Management Parameters using the Pacific Herring Assessment Model (ISCAM)

### Pacific Regional Science Advisory Process
### June 27-28, 2012

### Pacific Biological Station, Nanaimo, BC
### Chairperson: Linnea Flostrand

**Context**

Annually, an assessment of Pacific herring abundance and forecasts for the coming year is generated for each of the five major and two minor stocks in British Columbia, using a statistical catch–age-model. The assessment framework integrates data sampled from the fishery and the population with analytical methods to model population dynamics and components of harvest control rule. The annual assessment is reviewed through a Canadian Science Advisory Secretariat Regional Advisory Process (RAP) and harvest advice is provided to Fisheries and Oceans Canada (DFO) Fisheries Management each fall to inform the development of the Integrated Fisheries Management Plan (IFMP, DFO 2011a).

Refinements to the herring statistical catch-age model have occurred on an ongoing basis since its earliest version (Haist and Stocker 1984). The most recent major review occurred in September 2011 (Martell et al. 2012), addressing areas of concern identified in recent RAP reviews (see DFO 2009a, DFO 2011b, DFO 2011c, DFO 2012a, DFO 2012b) and the herring stock assessment review workshop held in June 2010. The September 2011 meeting reviewed changes to: gillnet selectivity, spawn index catchability coefficient (*q*), and the likelihood function for age-compositions. Updated estimates of unfished biomass ($B_0$) were also presented, prompting changes to the stock-specific commercial fishing thresholds (effectively Limit Reference Points, LRP) calculated as 25%$B_0$. Structural changes to the herring assessment model were approved and the new model (iSCAM, integrated statistical catch-age model) was adopted for the provision of Science Advice in 2011 (2011 biomass estimates and forecasts for 2012).

The most recent working paper (Martell et al. 2012) also discussed compliance of the existing herring assessment framework with DFO Precautionary Approach policy “A fishery decision-making framework incorporating the Precautionary Approach” (DFO 2009b)”.

Proceedings from the September 2011 meeting (DFO 2012b) identify priorities for future work, primarily associated with improved understanding of the impacts of data assumptions and structural changes to the stock assessment model (iSCAM) on the calculation of current biomass ($B_t$) and biological reference points ($B_0$, MSY). Fisheries Management has requested Science advice on these future work priorities and on new approaches for the provision of advice that incorporate probability values to inform on risk based management decisions.

**Objectives**

*Working paper to be reviewed:*

*<u>Evaluation of Data and Model Assumptions on the Calculation of Management Parameters using the Pacific Herring Assessment Model (ISCAM)</u> – CSAP Working Paper 2012-P07. Jaclyn Cleary, Vivian Haist, Steven Martell, Jake Schweigert*

The objectives of this peer review process are to:

1. Improve understanding of the impacts of data assumptions and recent structural changes to the stock assessment model (iSCAM) on the calculation of current biomass ($B_t$) and biological reference points ($B_0$, $B_{MSY}$). This will be done through reviewing:

    a) Information on changes to parameterization of gillnet selectivity and the influence of these changes on estimates of $B_t$, $B_0$ and $B_{MSY}$.
    b) Sensitivity analyses for alternate prior values for $q$ and influence of these assumptions on estimates of $B_t$, $B_0$ and $B_{MSY}$.
    c) Authors may also explore and report on:
        - Positive retrospective bias in Prince Rupert District biomass estimates.
        - Sensitivity analyses and alternate proportions in pooling of age classes (2% was applied for 2011 assessment).
        - Effects of fitting the model to 2 separate sources of age composition data: test charter data and commercial fishery data.
        - Methods for including 'catch' and 'spawning biomass' associated with spawn-on-kelp (SOK) fishery as input data to the annual assessment.

2. Improve interpretation and selection of risk-based decision tables, with probability values and defined risks associated with different management options. This will be done through reviewing:

    a) Science advice as decision tables – discuss how decision tables capture uncertainty in science advice through probability values, moving fisheries management towards risk-based decision-making.
    b) The interpretation and utility of risk metrics proposed at September 2011 RAP (DFO 2012b, relating to probabilities of $SB_{t+1} < 0.25B_0$; $SB_{t+1} < SB_t$; $U_t > 0.20$)
    c) Consideration of candidate management objectives and alternate risk metrics
    d) Outcomes based on existing herring decision rules (using recruitment categories of poor, average, good) with those arising from decision tables.

**Expected publications**

CSAS Proceedings (1)
CSAS Research Document (1)

**Participation**

DFO Science Branch
DFO Fisheries Management Branch
Province of B.C.
Commercial and recreational fishing interests

First Nations organizations
Non-government organizations
Academia

## References Cited and Additional Information


Cleary, J.S., J.F. Schweigert, and V. Haist. 2010. Stock assessment and management advice for the Bitish Columbia herring fishery: 2009 assessment and 2010 forecasts. DFO Can. Sci. Advis. Sec. Res. Doc. 2009/079. vii + 81 p. http://www.dfo-mpo.gc.ca/CSAS/Csas/publications/resdocs-docrech/2009/2009_079_e.pdf

DFO. 2009a. Proceedings of the Pacific Scientific Advice Review Committee (PSARC) Pelagic Subcommittee Meeting: Stock assessment and management advice for BC herring fishery, 2009 assessment and 2010 forecasts and herring multi-stock analysis; September 2, 2009. DFO Can. Sci. Advis. Sec. Proceed. Ser. 2009/037. http://www.dfo-mpo.gc.ca/CSAS/Csas/Publications/Pro-CR/2009/2009_037_e.pdf

DFO. 2009b. A fishery decision-making framework incorporating the Precautionary Approach. http://www.dfo-mpo.gc.ca/fm-gp/peches-fisheries/fish-ren-peche/sff-cpd/precaution-eng.htm

DFO. 2011a. Pacific Region Integrated Fisheries Management Plan, Pacific herring, November 7,2011 to November 6, 2012. Fisheries and Oceans Canada. http://www.dfo-mpo.gc.ca/Library/344588.pdf

DFO. 2011b. Stock Assessment Report on Pacific Herring in British Columbia in 2010. DFO Can. Sci. Advis. Sec. Sci. Advis. Rep. 2010/064. http://www.dfo-mpo.gc.ca/CSAS/Csas/publications/sar-as/2010/2010_064_e.pdf

DFO. 2011c. Stock Assessment Report on Pacific Herring in British Columbia in 2011. DFO Can. Sci. Advis. Sec. Sci. Advis. Rep. 2011/061. http://www.dfo-mpo.gc.ca/csas-sccs/Publications/SAR-AS/2011/2011_061-eng.pdf

DFO. 2012a. Pelagics Standing Committee reviews of: Stock assessment and management advice for the British Columbia herring stocks, 2010 assessment and 2011 forecasts, and Pacific herring biological sampling program design; September 1-2, 2010. DFO Can. Sci. Advis. Sec. Proceed. Ser. 2011/058. http://www.dfo-mpo.gc.ca/csas-sccs/Publications/Pro-Cr/2011/2011_058-eng.pdf

DFO. 2012b. A review of the Pacific herring assessment framework and stock assessment and management advice for Pacific herring 2011 status and 2012 forecasts, September 7-9, 2011. DFO Can. Sci. Advis. Sec. Proceed. Ser. 2011/062. http://www.dfo-mpo.gc.ca/csas-sccs/Publications/Pro-Cr/2011/2011_062-eng.pdf

Haist, V., and M. Stocker. 1984. Stock assessment for British Columbia herring in 1983 and forecasts of the potential catch in 1984. Can. MS Rep. Fish. Aquat. Sci. 1751: 50p.

Martell, S.J., J. Schweigert, J. Cleary and V. Haist. 2012. Moving towards the sustainable fisheries framework for Pacific herring: data, models, and alternative assumptions. Stock Assessment and Management Advice for the British Columbia Pacific Herring Stocks: 2011 Assessment and 2012 Forecasts. DFO Can. Sci. Advis. Sec. Res. Doc. 2011/136. http://www.dfo-mpo.gc.ca/Csas-sccs/publications/resdocs-docrech/2011/2011_136-eng.pdf

## APPENDIX B: AGENDA

**Evaluation of Data and Model Assumptions on the Calculation of Management Parameters using the Pacific Herring Assessment Model**

**June 27 and 28, 2012**
**Pacific Biological Station, Seminar Room**
Centre for Science Advice Pacific
Pacific Regional Science Advisory Process
**Chairperson: Linnea Flostrand**

**Day 1- Wednesday June 27**

| | | |
|---|---|---|
| 09:00 | Welcome & introductions | Linnea Flostrand |
| 09:15 | Review agenda & housekeeping | Linnea Flostrand |
| 09:30 | CSAS overview & meeting procedures | Linnea Flostrand |
| 09:45 | Review terms of reference | Linnea Flostrand |
| 10:00 | Presentation: Part 1 of working paper: *Pacific herring assessment model - evaluation of data and model assumptions* | Jaclyn Cleary |
| **10:40** | **Break** | |
| 11:00 | Part 1: Reviewer feedback (& author responses) | Robyn Forrest |
| 11:40 | Part 1: Reviewer feedback (& author responses) | Gary Melvin |
| **12:20** | **Lunch Break** | |
| 13:30 | Part 1: Identifying issues and topics needing discussion - *Evaluation of data and model assumptions* | RAP Participants |
| **14:45** | **Break** | |
| 15:00 | Part 1: - Issues & topics -discussion and resolution | RAP Participants |
| ***16:30*** | ***Adjournment*** | |

**APPENDIX B -AGENDA, continued**

**Day 2- Thursday June 28**

| | | |
|---|---|---|
| 09:00 | Introductions & housekeeping | Linnea Flostrand |
| 09:15 | Review day 1 outcomes & agenda for day 2 | Linnea Flostrand |
| 09:30 | Presentation: Part 2: *Management parameters and risk-based decision tables* | Jaclyn Cleary |
| 10:10 | Part 2: Reviewer feedback (& author responses) | Robyn Forrest |
| **10:45** | **Break** | |
| 11:05 | Part 2: Reviewer feedback (& author responses) | Gary Melvin |
| **12:15** | **Lunch Break** | |
| 13:20 | Part 2: Identifying issues and topics needing further discussion - *Management parameters and risk-based decision tables* | RAP Participants |
| **14:45** | **Break** | |
| 15:00 | Part 2: - Issues & topics -discussion and resolution | RAP Participants |
| 16:00 | Parts 1 and 2- Clarify meeting conclusions and recommendations | RAP Participants |
| 16:20 | Next steps: proceedings and revised research document. Herring assessment review September 5 & 6. | Linnea Flostrand |
| ***16:30*** | ***Adjournment*** | |

# APPENDIX C: SUMMARY OF THE WORKING PAPER

Part 1 of the WP was aimed at investigating four components of the recent Pacific herring assessment model. The work was described under the following four headings:

1. Spawn survey proportionality coefficient, q: sensitivity analysis of prior values for q and influence of these assumptions on estimates of Bt, B0 and BMSY.
2. Gillnet selectivity: further evaluation of changes to parameterization of gillnet selectivity, interactions with q, and the influence of these changes on estimates of Bt, B0 and BMSY.
3. Age-class pooling: sensitivity analysis and alternate proportions for pooling age classes.
4. Examine observed positive retrospective bias in biomass estimates for the Prince Rupert District.

Part 2 of the WP described how herring population assessment advice can be related to the DFO Precautionary Approach and decision-making framework and included the 5 candidate biomass reference points and four catch rate reference points listed below. For demonstration purposes, decision tables for the six reference points were provided for the Strait of Georgia and Prince Rupert District stocks for assessment results associated with age-class pooling at 0.01 and 0.02.

**Biomass reference points:**

| | |
|---|---|
| *P* (SB decline) | Probability the spawning stock in year t+1 will decline from previous year's level, calculated for a given harvest level; *P* ($SB_{2013} < SB_{2012}$) |
| *P* ($SB < 0.25B_0$) | Probability the spawning stock in year t+1 will decline below cutoff (the current herring single-reference point); *P* ($SB_{2013} < 0.25B_0$) |
| *P* ($SB < 0.75B_0$) | Probability the spawning stock in year t+1 will decline below $0.75B_0$; *P* ($SB_{2013} < 0.75B_0$). Note: this is the management target level recommended by the Marine Stewardship Council (MSC) for the management of Low Trophic Level (LTL) species. |
| *P* (SB < 0.40 Bmsy) | Probability the spawning stock in year t+1 will decline below a LRP of 0.4 Bmsy (candidate LRP as identified in Annex 1b of the DFO (2009) framework). |
| *P* (SB < 0.80 Bmsy) | Probability the spawning stock in year t+1 will decline below an USR of 0.8 Bmsy (candidate USR as identified in Annex 1b of the DFO (2009) framework) |

**Catch rate reference points:**

| | |
|---|---|
| *P* (U > Umsy) | Probability the annual exploitation rate (realized harvest rate) is greater than the optimal harvest rate resulting in MSY; *P* ($U_{2012} > Umsy_{2012}$) |
| *P* (U > 1/2 Umsy) | Probability the annual exploitation rate (realized harvest rate) is greater than half the optimal harvest rate resulting in MSY; *P* ($U_{2012} > ½ Umsy_{2012}$) Note: ½ Fmsy is the maximum target harvest rate recommended by the Marine Stewardship Council (MSC) for harvest of Low Trophic Level (LTL) species. |
| *P* (U > 2/3 Umsy) | Probability the annual exploitation rate (realized harvest rate) is greater than 2/3rds the optimal harvest rate resulting in MSY; *P* ($U_{2012} > 2/3\ Umsy_{2012}$) Note: 2/3 Fmsy (or F0.1) is the maximum target harvest level identified in the Policy for Canada's Atlantic Fisheries (Anon 1981). |
| *P* ($U_{3+}$ > 20%) | Probability the annual exploitation rate (realized harvest rate) of herring age3-and-older is greater than 20% of the prefishery forecast biomass; *P* ($U_{3+,\ 2012}$ > ) |

## APPENDIX D: ATTENDEES

| Last name | First name | Association | June 27 | June 28 |
|---|---|---|---|---|
| **DFO** | | | | |
| Fort | Charles | DFO, Science | A | A |
| Fu | Caihong | DFO, Science | A | A |
| Goruk | Andrea | DFO, FAM | A | X |
| Hall | Peter | DFO, FAM North Coast | A | X |
| Joyce | Marilyn | DFO, Science | A | A |
| Kanno | Roger | DFO, FM | A | A |
| McCarter | Bruce | DFO, Science | A | X |
| Melvin | Gary | DFO, Science St Andrews | A | A |
| Mijacika | Lisa | DFO, FM | A | A |
| Petley-Jones | Beth | DFO, Science | A | X |
| Ryall | Paul | DFO, FM | A | A |
| Schweigert | Jake | DFO, Science | A | A |
| Spence | Brenda | DFO, FM | A | A |
| Tanasichuk | Ron | DFO, Science | A | X |
| Taylor | Nathan | DFO, Science | A | X |
| Boldt | Jennifer | DFO, Science | A | A |
| Cleary | Jaclyn | DFO, Science | A | A |
| Daniel | Kristen | DFO, Science | A | A |
| Evanson | Melissa | DFO, Science | A | A |
| Flostrand | Linnea | DFO, Science | A | A |
| Forrest | Robyn | DFO, Science | A | X |
| | | | | |

A= attended, X= absent

APPENDIX D -ATTENDEES, continued

| EXTERNAL | | | | |
|---|---|---|---|---|
| **Last name** | **First name** | **Association** | **June 27** | **June 28** |
| Amoroso | Ricardo | UBC Fisheries | A | A |
| Dorner | Brigitte | Hakaii Network | A | X |
| Gladstone | William | Heiltsuk | A | A |
| Haist | Vivian | Haist Consulting | A | A |
| Hay | Doug | DFO Scientist Emeritus | A | A |
| Jones | Russ | Council of Haida Nation | A | A |
| Moody | Reg | Heiltsuk First Nation | A | A |
| Morley | Rob | Canadian Fishing Company | A | A |
| Newman | Earl | Heiltsuk | A | A |
| Starr | Paul | Independent / HCRS | A | A |

A= attended, X= absent

## APPENDIX E: WRITTEN REVIEWS

**By Dr. Robyn Forrest,** Research Scientist, Marine Ecosystems and Aquaculture Division, DFO, Pacific Biological Station, Nanaimo, B.C.

Review of Cleary et al. 2012: Evaluation of Data and Model Assumptions on the Calculation of Management Parameters using the Pacific Herring Assessment Model (ISCAM)
June 27-28, 2012

**General comments**

This is a review of the above-titled research paper (Cleary et al. 2012), which provided analyses additional to the 2011 stock assessment for Pacific herring (Martell et al. 2011), following reviewers' recommendations (see Flostrand 2011). As such, this is not a review of the iscam stock assessment model. There may be outstanding reviewer concerns from the 2011 assessment that have not been addressed in the research document under current review, for example, the appropriateness of B0 and Bmsy-based reference points for species such as Pacific herring. I will not refer back to the recommendations and concerns of the previous reviewers but I expect them to be listed with point by point responses in the next stock assessment for Pacific herring (due Autumn 2012). In my opinion, this should be a feature of all future Pacific stock assessments.

In general, the authors of the current research document have presented a fairly comprehensive set of sensitivity analyses in response to previous reviewers concerns about: (i) the priors used for the parameter used to scale estimates of spawning biomass (q); (ii) treatment of gillnet selectivity; (iii) pooling of data in the multivariate-logistic age composition likelihood function; and (iv) retrospective bias in previous assessments for the Prince Rupert region.

Overall, I found the analyses to be well-executed and illuminating with respect to the concerns of the previous reviewers. I do have some concerns with some of the conclusions drawn by the authors and some of the results would benefit from presentation of more information. I found the section on treatment of gillnet selectivity to be quite poorly written and difficult to follow, especially the interpretation of results and, especially, motivation for consideration of the three alternative forms of the selectivity function. Re-use of scenario names and re-definition of base cases for each of the four sections made following the analyses more difficult than it should have been. The document could be improved by starting with the most simple analyses (e.g., prior for q) and continually building on them with new scenario names. In most assessments, "Base Case" implies that the authors have considered the many alternatives they tested and have selected the base case as the candidate model around which sensitivity runs are presented. In the second set of analyses presented here (gillnet selectivity), the base case is recommended to be rejected!

In my opinion, there should be a final synthesizing section that contains careful discussion of all the alternatives that were considered (there is quite a complex set here) and selection of a candidate base-case model for the assessment with justification for the choice. This would give the reviewers a good starting point for discussion.

It seems clear to me that there must be strong confounding among parameters in these assessments. Pairs plots, analysis of autocorrelation etc. would greatly assist understanding how structural assumptions and parameters within the model interact. I believe a previous reviewer (Cox) suggested looking at how parameters (q in particular) are confounded.

Finally, I have not seen Part II as I write this. I assume that the uncertainty intervals generated in these analyses will be translated into management advice. I sincerely hope there will not be multiple sets of decision tables reflecting all of the scenarios presented here but rather a final candidate set, accompanied by some thoughtful synthesis and discussion of the alternatives.

In the absence of seeing Part II, I have two main concerns about how the information contained in the present document will be translated into advice:

i) it is quite easy to get caught up in debates about what the “best” assumption is. This is a dangerous game and absent simulation studies or further information, it may be better to incorporate as much of the full range of uncertainty as possible, with some advice as to relative plausibilities, and let the managers make the risk assessment;

ii) while I strongly agree that q must be less than 1 and also that it is not the role of scientists to deliberately build risk-aversion into stock assessment models, I am concerned that acceptance of a new prior for q will result in a rapid jump in current estimates of herring biomass and accompanying quota. I feel that recommending a sudden increase in quota is risky given all of the outstanding uncertainties. Perhaps a “slow-up” approach is needed or some other precautionary approach agreed upon by managers and stakeholders. Careful thought needs to be given as to how the herring stock’s response to possible increases in quota should be monitored and responded to.

My specific comments are listed below as bullet points.

**Specific comments**

**1. SENSITIVITY ANALYSES OF PRIOR VALUES FOR *q***

- Base case scenario. Is the informative prior appropriate for <u>all areas</u> and <u>both periods?</u>

It is very unlikely that q=1 and I agree that statistical models should attempt to be as risk neutral as possible. But it is hard to believe that factors such as egg loss, bias in density estimation, the proportion observed and drift in observation over time could be consistent between surface and dive surveys. The Rooper reference apparently found that egg loss is a function of depth (for example). Having said that – maybe all of this comes out in the wash – our real interest is in the uncertainty and its impacts on risk and advice – pretending we know the “best” answer is not the right approach.

- P13: SSBo/Bo: This notation is confusing since Bo also refers to spawning biomass.

Just stick to one or the other.

- P13: “In calculating Bmsy we needed to partition fishing mortality to each gear type”

Twenty years seems like a long time for averaging ratios among fisheries – has the ratio been consistent over this period, volatile or trending? Since we are mainly interested in current status and projected status why not use a shorter time frame? How was 20 years decided? I recommend the authors look at these ratios over time. If they have changed a lot in a particular direction, I recommend using a more recent time period.

- P13 “For all five stock areas, choice of *q*-scenario has little implications on time series of spawning biomass depletion relative to Bmsy-based zones”

This may be – but since you are using the model to set quota, the q assumptions are critical. I assume this will be addressed formally in Part II, but it should be discussed here as well.

Importantly, since the recruitment “zones” are hardwired the q prior must surely affect whether recruitment is predicted to be poor, average or good (or have I misunderstood how this is done).

It is not entirely clear to me why recruitment is divided up into discrete categories – why not use a continuous range rather than absolute definitions of what is good?

- P14. “Any issues with model convergence or extraneous estimated parameter values would show up in these comparisons of objective function values.” Would they? If similar objective function values can be found for multiple parameter combinations, couldn’t the trace plot remain stable while the estimates are going all over the place? You should show the autocorrelation plots as well to analyse convergence. One of the authors mentioned difficulties achieving convergence – how were these overcome? It would be more honest to discuss which scenarios had problems converging.

For an assessment with the types of problems under discussion here (confounding among parameters describing the scale and productivity of the population), pairs plots of posterior parameter estimates are also important diagnostic statistics. These were requested by the previous reviewers and I am disappointed that confounding between q and other parameters has not been presented in this document.

- P14. “Comparisons presented herein did not reveal any biases from one method (scenario) over another”

Bias seems to be an odd choice of word since bias is the thing we are worried about.

- P 14. “By implementing the Bayesian prior during the estimation procedure we are able to explicitly acknowledge that we do not survey all spawning events, and we are able to use information from past independent studies to inform the assessment process”. I agree but I am not sure it is appropriate to use the same prior assumptions about egg loss and proportion surveyed for both types of surveys. For example – during the surface surveys, submerged eggs are not observed and during the dive surveys, surface eggs are not observed.

- P 15. Figure 1.1. Some of the graphs go off the page.

- P 15. Figure 1.1. Could the authors comment on why the base scenario is similar to the uniform scenario for SoG and WCVI? The estimates of q are similar in these areas – is this because the data are more informative about q? This seems to be the case for SOG – q1 is updated and q posteriors are similar for the two scenarios. For WCVI, the q priors are returned.

Transparent posterior envelopes could be displayed here – see C. Grandin (PBS) for code. It could be useful to see as it would allow overlap of envelopes to be seen.

**2. GILLNET SELECTIVITY: FURTHER EVALUATION OF CHANGES TO PARAMETERIZATION OF GILLNET SELECTIVITY**

In general, I found this section difficult to follow and some of the conclusions poorly supported. I’m not clear on the rationale for using the age-based weight-covariate approach as a base case, especially because it is rejected in the conclusions. This section would benefit from a rewrite, containing better explanations of the alternative approaches and more precise statements and conclusions.

- P24-25. I feel that better description of AIC and BIC is needed. Isn’t it usual to present the equations used in these approaches or at least describe how they work? Why use both? What should you do if the results were different? There is a statement that their performance is situation-dependent. What does this mean?

- P25 – 26. Equations should be numbered. Mu and sigma are used to describe different quantities – for example weight at 50% vulnerability and age at 50% vulnerability in the first two equations they are not the same from scenario to scenario. For the age-based weight-covariate method, it is very unclear to me how lambda is treated. Is it an estimated parameter? If so does it have a prior and where are the estimates reported? It must surely be confounded with other parameters. If it is not estimated, then what assumptions are made about it?

- P26. “Values of $\lambda^{v} \neq 0$ imply a positive or negative affect of variation in growth on selectivity. I can see from the equation that if weight at age is below average, it would decrease the selectivity at age if selectivity is size-based (i.e., shift the curve to the right). Lambda seems to determine the degree to which changes in weight at age impact selectivity. Why would it ever be negative? (‘affect’ should be ‘effect’)

- P27. “Model estimates of $B_0$, $B_{2011}$, and stock depletion ($B_{2011}/B_0$) change by 10% or less in all stock areas”. Be careful of implying significance (or lack of) saying that changes are very little etc. 10% might mean a lot of money!

- P27. “For PRD, the single-step change from q1est./q2fixed to use of a Bayesian prior for *q* (Scenario A vs. C) resulted in a decrease in current abundance and current status”

The step change resulted in a decrease in *estimated* current abundance … this may be picky, but the authors make these kinds of statements in a few places. Similarly, AIC and BIC don’t “prefer” scenarios. They may provide greater support for them.

- P28. “Use of the age-based weight-covariate method allows for selectivity to change over time; which shows selectivity for older fish in more recent years. Consequently, the stock assessment model assumes, or may be assuming, an increase in selection of older fish and the appearance of younger fish that are not available to the GN fishery.”

It is not clear what is meant by this statement or which scenario the age-based weight-covariate method is being compared to? If selectivity is weight based, then selectivity on older fish would decrease (i.e., the curve would shift to the right). If purely age-based, the curve would stay in the same place regardless of weight at age (i.e., the curve would be further to the left). I think the authors mean the age-based weight-covariate method is estimating an increase in selection of older fish compared to weight based method because of greater rigidity of age based method.

The statement about the appearance of younger fish doesn’t really make sense. Do the authors mean more young fish <u>are</u> available to the fishery? Or are they referring to the fact that the model shifted all the biomass estimates up in the age-based weight-covariate scenario to account for the same historical catches under a different selectivity function?

Sorry to pick again, but models don’t assume they estimate.

- P28. “presenting a more optimistic outcome for younger age-classes than what may actually be present (overestimation of current biomass, $B_t$)“

How do you know you are overestimating biomass?

- P29. The discussion of natural mortality on younger age classes does not make much sense to me – especially since M is age-invariant in this model. The authors seem to be saying that the age-based weight-covariate scenario results in curves shifted to the left compared to the weight only method - and that this somehow implies a lower rate of natural mortality for younger fish.

Obviously selectivity can only affect fishing mortality on different age classes – natural mortality has nothing to do with the fishing gear. But from an estimation perspective, changing assumptions about selectivity can affect model estimates of natural mortality because selectivity, mortality and biomass can be strongly confounded. The authors assert that lower estimates of natural mortality are expected in the age-based weight-covariate scenario but I think this would only hold if the biomass estimates were more or less the same between scenarios (as we would also see higher fishing mortality on younger age classes and so need to explain the observed catches by some other means – either by estimating lower mortality or higher biomass of younger age classes).

It seems odd to reject this scenario because the model didn't estimate lower mortality. Why would it? It would only need to do this if the biomass is estimated the same, which it appears not to be. Glancing through Table 2.2. it appears that Scenario D (with higher fishing mortality on younger age classes) has highest B2011 estimates – so there would be no need for lower M.

- P29. "Given both the model selection criteria and biological considerations, we recommend proceeding with the *age-based* parameterization for GN selectivity." I'm not sure I agree with the rationale here and would like to hear a more detailed explanation in the meeting (I have no strong opinion on the best selectivity function to use).

**3. AGE-CLASS POOLING: SENSITIVITY ANALYSIS AND ALTERNATE PROPORTIONS FOR POOLING OF AGE CLASSES.**

- P33. Methods. The alternative (multivariate logistic and multinomial) likelihoods should be formally presented and assumptions stated (e.g., what sample sizes were used in the multinomial)?

- P35. "The sensitivity of the multivariate logistic likelihood to a *pmin* value of 0, which was not observed for the multinomial likelihood, suggests a pmin value >0 should be used for the herring assessments, pending results from simulation studies. Alternate values may also be explored as unresolvable scenarios in a decision table"

We stopped binning age classes > 0 in the hake assessment because it produced estimates of large cohorts in years we knew there weren't any. Several commentators noted that binning might be a more appropriate approach for longer lived species with many age classes. For species like herring with fewer age classes, think about whether it makes sense to bin age classes. Are herring like hake, where spawning populations can be maintained by very strong but rare cohorts? Binning may reduce the ability to detect strong cohorts.

- Why do the authors think the variance was so high for the pmin=0 scenario? Is it because the model can't explain absences of certain age classes in some years? This needs more careful thought and discussion. What were the sample sizes like? Where were zeros occurring and why? Was it happening in certain years? It would be very helpful to see age composition data with and without binning – say for pmin=0 and pmin=0.02.

**4. EXAMINE OBSERVED POSITIVE RETROSPECTIVE BIAS IN BIOMASS ESTIMATES FOR THE PRINCE RUPERT DISTRICT**

I have no major concerns with this section. Analysing causes of retrospective bias is notoriously difficult and the authors have done well to uncover one cause – disagreement between age composition datasets.

Again, I reiterate my earlier point that a general discussion section is recommended, although this may appear in Part II (which I have not seen).

**By Dr. Gary Melvin,** Research Scientist, DFO, Maritimes Region, St. Andrews NB

Review of Cleary et al. 2012: Evaluation of Data and Model Assumptions on the Calculation of Management Parameters using the Pacific Herring Assessment Model (ISCAM)
June 27-28, 2012

**General:**
The terms of reference for the meeting are based on the outcome and recommendations from the September 2011 Pacific herring assessment framework and stock assessment and management advice. A number of recommendations for future work were identified in the Proceedings of the meeting (page 9), some of which are being addressed at the current meeting and others which are research in progress. The analyses are presented in a working paper divided into two parts.

"Evaluation of Data and Model Assumptions on the Calculation of Management Parameters using the Pacific Herring Assessment Model (ISCAM) – CSAP Working Paper 2012-P07. Jaclyn Cleary, Vivian Haist, Steven Martell, Jake Schweigert"

Part 1: Improve understanding of the impacts of data assumptions and recent structural changes to the herring assessment model, and;
Part 2: Risk-based decision tables for the provision of science advice for BC herring stocks.

Overall there is an extensive amount of background material and history on the assessment process and management approach in dealing with the 5 major and 2 minor Pacific herring stocks. The authors are to be commended for the clarity of their working paper summaries and the fact that the documents were available in sufficient time to undertake the review.

**PART 1: Improve understanding of the impacts of data assumptions and recent structural changes to the herring assessment model**

There have been a number of significant changes to the assessment model and parameterizations, the most recent being HCAM to iSCAM in 2011. Differences were observed in un-fished biomass $B_0$, trends, and scaling of biomass. These observations have raised concerns and questions related to the outputs of the model and the perception of stock status. In fact although the iSCAM model was accepted at the September meeting one of the formal reviewers did not agree. My general perception of the documentation provided is that there is still a fair amount of uncertainty associated with the model outputs given the sensitivity to parameterization, in particular the spawning bed  index "q". The main topics associated with improvement and understanding of impact on biomass and reference points, addressed in the working paper, include:

1) Spawning survey proportionality coefficient q.
2) Gillnet selectivity
3) Age class pooling
4) Prince Rupert District data.

**Background:**
The assessment model utilizes 3 key sources of data; commercial catch/landings, age composition data, and a single survey (spawn survey index). There are also sources of catch not included in the landings data (food and bait, special use, spawn-on-kelp). How extensive are these removals?

Catch reporting and monitoring has changed over time with better accounting in recent years assumed. Has there been any adjustment in the historical catches to account for under reporting? Are there discards and if so are they taken into consideration?

The fact that the commercial data are divided into three time periods suggest there has been a change in the targeting of fish (fishing patterns) likely reflecting a change in selectivity of the fishery over time. For the reduction fishery anything goes, for the seiners what is present is taken and for the gillnetter gear selectivity reflective of gear size. These changes are likely to affect the catch at age matrix. Any general comments?

Catches is several stocks (HG, CC, and WCVI) have been 0 for the past several years. Does this mean no catch and if so what are the implications for the assessment model? I assume F=0 and all other parameters are the same. What about biological samples in these areas? Does the HCRS covers the closed stocks.

The spawn survey index is a critical component of the assessment. Unfortunately, it is divided into two periods1951-1987 and 1988 to present due to a change in methodology without overlap of sampling methods. This creates a bit of a scaling problem between periods due to no linkages.

The assessment model for providing advice has changed several times since 1980's all with the purpose of improving the assessment and advice provided to managers. They have however all changed our perception of stock status when implemented. The model changed again in 2011 from the HCAM to the iSCAM model and our views of the stocks has changed slightly depending upon the parameterization.

Harvest control rule are established and clear, but the implementation and allocations depend upon the estimates of current biomass

**1.1 Spawn survey proportionality coefficient q.**

Four scenarios were examined to evaluate the impact of survey q on the assessment outputs (Bt, B0, and Bmsy, Fmsy and parameter estimates (q1, q2, M and h). The scenarios used fixed, informed and uninformed priors to evaluate the impact. By far the largest change in most of the outputs and estimated parameters is the shift from a fixed q2 to and estimated q2. Depending upon the model run and area q1 can vary from 0.18 to1.02 and q2 from 0.25 to 1.07. This will have a significant impact as a multiplier. From a biomass perspective Scenarios B and C produce increased biomass estimates throughout the time series and scenario C higher estimates than B for the low level stocks CC and HG.
Q values greater than 1 were questioned by a reviewer last year and it was explained as a ratio and consequently could be greater than 1. However, that being true it is not possible for a q to be greater than one so is it bounded for input as more than 1 for estimations? If not it could represent a bias for the amount greater than 1.00.

Fmsy seems quite high relative to a number of other herring stocks (eg. North Sea Fmsy=0.25). The levels predicted from the various model runs are characteristic of a very short lived species or a recruitment fishery. With the exception of PRD natural mortality also appears high compared to values used elsewhere in the world for herring. <u>Any suggestion why M is lower in PRD than the other stocks?</u>

The real question is what is missed in the survey and how variable it is from year to year. If the index represents 95% of spawning on an annual bases then the assumption of q=1 is not a significant problem. However, if it represents much smaller portion of SSB and is variable from year to year then the assumption of a constant q is invalid and will lead to a great deal of uncertainty in the output estimates. <u>Do the authors have a feeling of how representative the index is for the different stocks?</u>

<u>Another question is can the survey coverage be that restrictive that it misses a major portion of spawning and the spawning dynamics that variable from year to year and over time that it cannot be captured by the current coverage/methods</u>? If so then the spawn survey index is a poor index of abundance. Are there other factors at play such as egg loss timing?

I agree completely with panel assumption that q was likely the single most important parameter affecting the model outputs. Based on the information provided, the different scenarios did not produce any bias and the residual patterns were similar among the scenarios for each stock. Consequently, no method could be eliminated based on statistical performance. As in the previous review, although a model parameterization was accepted, there is still a fair amount of uncertainty about the biomass levels. **The current analysis does not really resolve the issue of survey q.**

**1.2 Gillnet Selectivity:**
Over the years there appears to have been a number mesh size changes in the gillnet fleet that would result is a change in selectivity. This was likely due changes in the size of available fish. Regardless gillnet nets are selective and the gear change will have affected the size and likely weight of fish captured by the fishery. The impact of 5 alternative gillnet selectivity assumptions were examined. <u>Is there a reason the age-based q1est/q2 fix was not run given the aged-based Bayesian prior is the recommend parameterization for GN selectivity?</u>

I agree that the changes to gill net selectivity seem to have little influence on abundance from the information provided. Again the major differences occur for model runs that estimate q1 and q2 compared with a fixed q2. Biomass is extremely variable depending upon the stock area and generally increases for the C, D, E scenarios, with the exception of the PRD. Statically there is a preference for the age based weight-covariate in 4 of the 5 stocks. Scenario E recommended and is consistent with other the other two fisheries. <u>Given the changes in weight-at age observed over the time series wouldn’t a scenario that captures the variability be more suited to analysis than one that appears to smooth within age differences.?</u> Selecting the age-based model does not incorporate the within age variability as illustrated by the figure 2.2. The better fit may simply be related to lack of variability within the time series.

Fmsy is again extremely high in my mind compared to other herring stocks. Natural mortality is also high relatively to other estimates, but out of the question.

Page 26 2.2.2 Model Scenarios “q2 fixed at zero” I believe should read q2 fixed at 1.

**1.3 Age Class Pooling:**
The 2011 assessment base model used to investigate the sensitivity of pooling age classes for pmin (for pooling) ranging from 0.00 to 0.04. Based on table 3.1 there is clearly an impact due to pooling of age classes that represent < 2% or using zero for the age-group. Noticeable difference were observed B0, B2011, Fmsy, Bmsy and q1 and q2. The total objective function would suggest that a pooling option >0 but less than or equal to 2.0 could be justified depending upon the stock area. Under this type of option there is some concern for the loss of older age groups for some fisheries or gear types. <u>How does the model handle these changes or absence of year classes in a fishery?</u> Does the model simply assume they didn't exist. <u>How is this carried over to the other fisheries where the un-pooled age-classes exceed the Pmin value.?</u>

<u>On a functional note, when an age class proportion is zero from the fishery is it due to poor sampling or a true absence of the year class?</u>

**1.4 Retrospective Biomass Pattern PRD Data:**
Historically there has been a strong retrospective pattern with the PRD assessment which tended to overestimate the PRD SSB. Yet I agree with the authors that for the base model in recent years it appears to be minimal and without a consistent pattern. The interesting outcome occurs with using a truncated time series. Again historically there is a strong retrospective pattern up 2003 with an abrupt, unexplained change in 2004. Thereafter it appears to be without pattern. However when one fishery is removed from the model the pattern disappears, suggesting a contradiction in the data. I also agree with the authors that the two geographically different fisheries impact on the model outputs may suggest different stocks, but further investigations into the concept is warranted.

**Summary**:
The authors have undertaken a significant about of sensitivity analyses to address several of the issues raised at the September 2011 meeting regarding the acceptance of the iSCAM model and parameterization. Unfortunately, nothing conclusive can be drawn from their efforts and there is still a lot of uncertainty associated with the model outputs. One of the primary concerns is the marked increase in spawning stock biomass for most if not all areas associated with the change in assessment models. Applying a HCR associated with an estimate of SSB that varies depending upon the assessment model is a bit disconcerting.

## PART 2: Risk-based decision tables for the provision of science advice for BC herring stocks

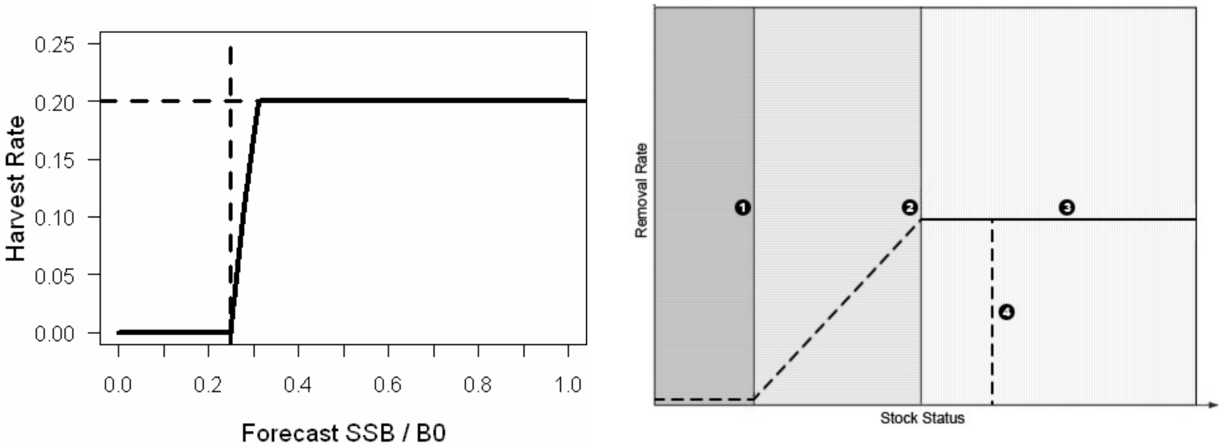
The authors provide a good overview of the harvest control rule as they apply to Pacific herring and the general requirements for a precautionary approach. For Pacific herring the harvest control rules are well established and clearly defined. Based on a forecast procedure estimates of SSB are predicted a year in advance and the harvest rules applied. The forecast is however premised on an estimate of SSB and recruitment to provide advice on harvest amount and rates. No estimates of error or risk is associated with the advice. Unfortunately given the recent increase in SSB associate with the change in assessment models there is real concern as to where the stock is in absolute biomass terms and what will be the impact of increased catches based on the revised SSB, especially on the low level stocks, HG, CC, and WCVI. In other parts of the world stocks without a recruitment index use an estimate of recruitment in based on a running average, a geometric mean recruitment over a specific time period or a standard value.

Under the Precautionary Approach (PA) reference points and stock status zones must be defined, harvest strategy and decision rules established, and risk/uncertainty accounted for. This means that objectives for the stock have been identified so that you know where you don't want to be, where you should take a more restrictive Harvest approach, and when you feel comfortable to harvest at optimum levels. All of these points can be defined based on an acceptable assessment model. However, the reference points will change over time and with changing parameterization of the assessment model. This is clearly illustrates in the scenarios presented in Part 1 of this report. SSB, Fmsy, and Bmsy can change dramatically depending upon the parameterization of the assessment model. Consequently any approach utilizing reference points to define zones must not change substantially or if it does new reference points must be established/calculated for the new parameterization. Reference points for the precaution approach can be in terms of SSB or F.

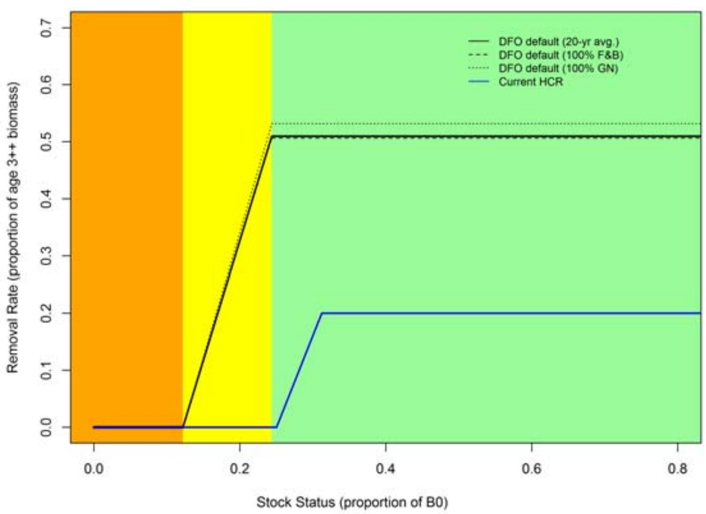
In the report the authors seem of the view that Harvest control rules and the PA with defined reference points are mutually exclusive. However, there are several stocks in the North Sea where the PA is applied, but a HCR is used to limit the annual degree of change in advice. For example there is a HCR between Norway and the EU that restrict the change in recommended catch to 15% regardless of direction. Combining the PA with a HCR has the benefit limiting the amount of variability, especially for high variable stocks such as pelagic fishes.

The authors have provided several tables of probabilities for different criteria under a variety of TAC's to help manager in their decision making process. While an number of the critera are commonly used by managers it is incumbent upon the managers to request specific risk analysis for scenarios they would like to see. Science is usually able to provide a risk analysis for most scenarios but requires some direction on what management needs. Regarding risk, a risk neutral situation is usually defined as the 50% probability where the outcome can go either way. Risk adverse if the probability is less than 50% and risk prone is greater than 50%. It is also important that when providing risk tables an example of how to interpret the table is presented. This will ensure the results are interpreted properly.

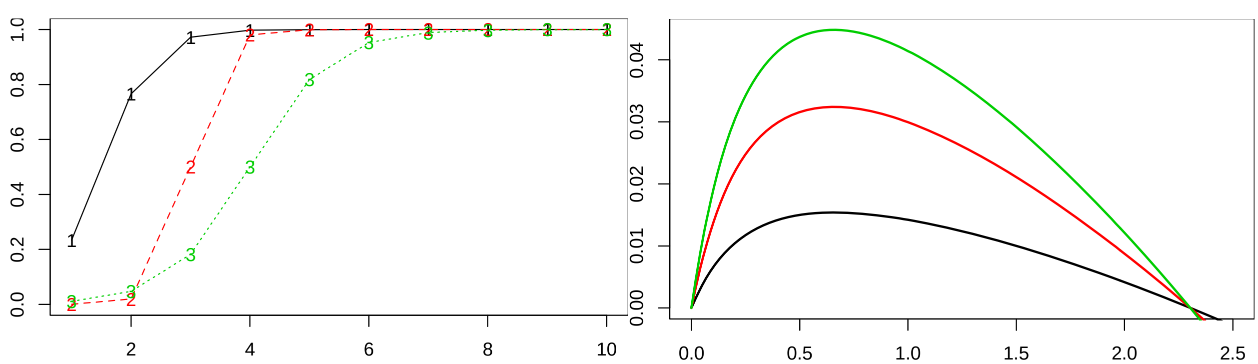
# APPENDIX F: ADDITIONAL MATERIAL PRESENTED ON PART 2




Compliance of current herring procedure with the DFO decision-making framework




Comparison of current and DFO HCRs (SOG)




Consider 3 fisheries with 3 hypothetical selectivity curves