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Proceedings of the Pacific Regional Peer Review on Application of a New Modelling Framework for the Assessment of Pacific Herring (*Clupea pallasii*) Major Stocks and Implementation in the Management Strategy Evaluation Process

June 26–28, 2023
Hybrid in Nanaimo, British Columbia

Chairperson: Steve Schut
Editors: Yvonne Muirhead-Vert and Travis Tai

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Foreword

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

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*MPO. 2024. Compte rendu de l’examen par les pairs de la région du Pacifique sur l’application d’un nouveau cadre de modélisation pour l’évaluation des grands stocks de hareng du Pacifique (*Clupea pallasii*) et mise en œuvre dans le processus d’évaluation des stratégies de gestion; du 26 au 28 juin 2023. Secr. can. des avis sci. du MPO. Compte rendu 2024/030.*

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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 Peer Review meeting on June 26–28, 2023 at the Vancouver Island Conference Center in Nanaimo, British Columbia (BC) while virtual participants joined online. The working paper presented for the peer review to evaluate Spatially Integrated Statistical Catch-at-Age Herring (SISCAH) as a stock assessment modelling framework for each of the five major stock assessment regions and to evaluate SISCAH as an operating model.

Participation included DFO Science, Resource Management, and external participants from Michigan State University, Landmark Fisheries Research, BC Seafood Alliance, Simon Fraser University, Heiltsuk First Nation, Pacific Marine Conservation Caucus/David Suzuki Foundation, Island Marine Aquatic Working Group, Landmark Fisheries Research, Council of the Haida Nation, Nuu-chah-nulth Tribal Council, Q'ul-lhanumutsun Aquatic Resources Society, Province of British Columbia, Herring Conservation and Research Society, and Lax Kw'alaams Band.

The meeting participants agreed the working paper met all of the Terms of Reference objectives and was accepted with revisions. The conclusions and advice resulting from this review will be provided in the form of a Science Advisory Report on the update and review of the model used for annual assessment of BC Pacific Herring stocks (choice of model, data, assumptions) to include four new elements to the model and demonstrate its utility for representing additional dynamics as operating models in the management strategy evaluation (MSE) analytical framework. The Science Advisory Report and the supporting Research Document will be made publicly available on the Canadian Science Advisory Secretariat (CSAS) website.

INTRODUCTION

A Fisheries and Oceans Canada (DFO) Canadian Science Advisory Secretariat (CSAS) Regional Peer Review (RPR) meeting entitled “Application of a new modelling framework for the assessment of Pacific Herring (*Clupea pallasii*) major stocks and implementation in the management strategy evaluation process” was held on June 26–28, 2023 at the Vancouver Island Conference Center in Nanaimo. Virtual participants joined online.The working paper listed below was reviewed during the RPR meeting.

The [Terms of Reference (TOR)]() (Appendix A) for the science review were developed in response to a request for advice from Fisheries and Oceans Resource 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, other government organizations, and academia.

The following working paper was prepared and made available to meeting participants prior to the meeting (the working paper abstract is provided in Appendix B):

Johnson, S.D.N., Cox, S.P., Cleary, J.S., Benson, A.J, Power, S.J.H., and Rossi, S.P. Application of a new modelling framework for the assessment of Pacific Herring (*Clupea pallasii*) major stocks and implementation in the management strategy evaluation process. 2023. CSAP Working Paper 2019PEL01.

The meeting Chair, Steve Schut, welcomed participants, reviewed the role of CSAS in the provision of peer-reviewed advice, and gave a general overview of the CSAS process. The Chair discussed the role of participants, the purpose of the RPR publications (Science Advisory Report, Proceedings, and Research Document), and the definition and process around achieving consensus decisions and advice. 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. It was confirmed with participants that all had received copies of the Terms of Reference, working paper, and the two formal reviews (Appendix B).

The Chair reviewed the agenda and the Terms of Reference for the meeting, highlighting the objectives and identifying Travis Tai as the Technical Rapporteur for the meeting and Yvonne Muirhead-Vert identified as the Rapporteur for the revisions table. The Chair then reviewed the ground rules and process for exchange, reminding participants that the meeting was a science review and not a consultation. Audio and text conversations were conducted and recorded during the hybrid (in person and online attendance) meeting. Members were reminded that everyone at the meeting had equal standing as participants and that they were expected to contribute to the review process if they had information or questions relevant to the paper being discussed. In total, 33 people participated in the (Appendix D) over the two and half day meeting.

Prior to the meeting, Chris Cahill (Michigan State University) and Elisabeth Van Beveren (DFO Science) were asked to provide detailed written reviews of the working paper to assist everyone attending the peer review meeting. Participants were provided with copies of their written reviews ahead of the meeting.

The conclusions and advice resulting from this review will be used to inform fisheries managers on the update and review of the model used for annual assessment of BC Pacific Herring stocks to include four new elements and demonstrate its utility for representing additional dynamics as operating models in the management strategy evaluation (MSE) analytical framework. The

Science Advisory Report and supporting Research Document will be made publicly available on the Canadian Science Advisory Secretariat website.

GENERAL DISCUSSION

Following a presentation by the authors, the two reviewers, Chris Cahill (Michigan State University) and Elisabeth Van Beveren (DFO Science), shared their comments and questions about the working paper. The authors were given time to respond to the reviewers before the discussion was opened to all participants. This proceedings document summarizes the discussions that took place by topic, where points of clarification presented by the authors in their presentations and questions and comments raised by the reviewers and participants are captured within the appropriate topics. Both reviewers' formal submissions are located in Appendix C.

TERMS OF REFERENCE OBJECTIVE ONE

Present a new statistical catch-age model including simulation-estimation tests and comparisons with 2022 base model parameterizations from the previous model (Cleary et al. 2019 modifications to Martell et al. 2012).

Spatially Integrated Statistical Catch-At-Age Herring (SISCAH) model: The Spatially Integrated Statistical Catch-At-Age Herring (SISCAH) model is a new density-dependent mortality model that represents spawn-on-kelp fisheries, discrete and not continuous fishing seasons, a single spawn index that blends dive and surface survey designs, and takes into account autocorrelation of residuals among age classes.

One participant felt that the SISCAH model was not acceptable in its present form and would have liked to have had more time reviewing the model before the meeting since it is a technically complex model. They believed that an earlier process which used a Technical Working Group to discuss data or model updates prior to the CSAS meeting would have been beneficial. The authors responded that the model has been under development for the last year and is at the point for testing. They believe that the new model is an improvement over the Integrated Statistical Catch Age Model (ISCAM).

Comparisons with ISCAM: The stock assessment model ISCAM has been used for the last ten years for stock assessments and requires a number of improvements, prompting the creation of new model. The results from the transition analysis showed estimates of natural mortality (*M*) follow a much smoother curve for ISCAM compared to SISCAH. Both models assume that *M* varies over time, but use different functions to predict how *M* changes over time. ISCAM uses a cubic spline function to limit the shift of *M* in any one year. Cubic curves are fit between a certain number of points throughout the time series, whereas SISCAH's variation over time is mediated through a density-dependent function. In the SISCAH model, low values of *M* are constrained by the lower limit parameter M_b. Overall, the SISCAH model showed that the data fit reasonably well, there were low standard errors for the estimated parameter, evidence of low bias in the self-test with low autocorrelation, and was suitable as an operating model (OM) to represent Pacific Herring data. Comparing SISCAH with ISCAM in the transition analysis, the best agreement was between Haida Gwaii (HG) and West Coast Vancouver Island (WCVI) herring stocks. Both of these regions have had recent periods of low productivity which allowed for better estimates of the depensation parameter and the lower limit for mortality.

Simulation self-test: Four simulated data scenarios for Central Coast (CC) and Strait of Georgia (SOG) were run in SISCAH:

1. simulated index data;

2. simulated age data;
3. both sets of simulated data; and
4. same as (3) with random priors.

The CC had a relatively low *M* interannual variation that ranged between -0.2 and 0.2 for residuals compared to other regions while the SOG simulations were less conclusive but also did not show concerning levels of bias and variation. The results from the simulation self-test also did not show strong retrospective patterns from the stock reconstructions after removing data one year at a time.

A reviewer expected to see some discussion on the underestimating of unfished spawning biomass (SB_0) for SOG. The authors explained that with catchability coefficient (q)=1, SB_0 is constrained, which may result in underestimates of biomass.

The value for m_1 is uncertain for SOG when defining a functional form to match the overall *M* trends, as positive production has not permitted an estimation of the depensation parameter m_1. A density independent *M* series for SOG could be a solution for m_1. The authors agreed to revise this section of the working paper to discuss the possibility of other parametrizations of *M* within SISCAH.

Simulation analyses: A reviewer asked if work could be done on the simulation analyses to show some bias since the authors have indicated that they had no major concerns with it. The authors explained that simulation analyses are designed to show if bias exists, but that bias isn't determined through changesto parameters or model construction.

One reviewer suggested that it may be possible to relate the potential magnitude of bias with estimated uncertainty distributions. The authors indicated that most management decisions are based on spawning biomass (SB) depletion which has a low bias but it is possible to do a bias correction. A point simulation estimation of errors or a range of errors would be an alternate way to assess the magnitude of the bias. The authors agreed to add some discussion on this topic in the working paper.

Results from simulation analyses showed simulated recruitment for SOG to be lower on average than the estimated recruitment (i.e., is biased low relative to the assessment model), whereas for CC, simulated recruitment tracked the estimated recruitment (is unbiased relative to the assessment model).

The question was raised of how the forecast for pelagics is affected if the bias is present in the retrospective analysis since short-term projections could produce a problem. The authors indicated that there is bias in stock assessment parameters and the simulation estimation errors or range of errors is a good way to assess the magnitude of the bias.

Non-concerning bias: It was suggested to generate a figure showing the original true value the positive confidence interval (CI; or the posterior), with on top of that mean positive CI of the parameter value taken across all simulations to determine if bias is concerning or not. The authors have agreed to create this figure.

Sensitivity analysis: The reviewer asked if a sensitivity analysis was conducted with a lower value of the asymptotic lower limit of density-dependent mortality rate (M_b) and m_1. The authors responded that an asymptotic lower limit is needed. Values of 0.3 and 0.2 were tested for M_b but this formulation of the model did not converge, and further sensitivity analyses were not conducted.

Simulations: A member of the group asked if all the OMs converged during the simulation tests. The authors indicated that not all OMs converged and non-convergent OMs were filtered out. They have agreed to describe the process for how they dealt with non-convergence during model development.

Closed-loop simulations: The closed-loop simulations within the working paper are based on a scenario with perfect information. A reviewer asked why the authors used perfect information scenarios over using those with observation error. The authors explained that due to time constraints they presented perfect information scenarios in the working paper, and ran some imperfect scenarios and presented them on the second day of the meeting.

Prior specification: A reviewer voiced their concern regarding the use of the data to develop the prior. To them, it looked like the data were used twice in the analysis. A solution was presented to remove the perception of the data being used twice in the analyses, which was to take the last ten years of data and use those numbers in the calculations. Half of the data would be used for the priors and the other half would be used to cross-validate the data. An alternative approach would be to use the same prior for all five regions instead of using a different prior for each stock. Some concern was also raised regarding the choice of priors since the values could drive B_{MSY}. The authors replied that the model is fitted to data and the priors provide reasonable starting points during estimation and that posterior estimates will reflect updates from the data (provided the data are informative).

It was noted that a number of Bayesian analyses were conducted with many of the priors set without providing an explanation of why these specific priors were chosen or why they were ecologically defensible. The authors explained that the priors on the life-history parameters were set based on values used in previous modeling approaches (i.e, ISCAM) whereas the selectivity priors were drawn from previous modelling approaches and adjusted to fit the data in a closed-loop simulation. The reviewer also wanted to have a better understanding on where the priors came from in Table 4. The authors acknowledged that the approach that they used to choose the parameters and hyperparameters was imperfect.

After a lengthy discussion on the priors, the authors agreed to search for a better approach for developing priors and an include an explanation on how they calculated them in the working paper.

Prior and posterior predictive checks: A participant suggested conducting prior predictive checks to see if the model/prior combinations were generating patterns that were consistent with the current knowledge of population dynamics for herring. The posterior predictive checks would show how well the model fit to the observed data. Another participant requested more context to this section of the working paper to evaluate and compare the results. The authors agreed to address this request by including more discussion on this and any prior predictive checks for m_1 and M_{b}.

Visualize priors: A reviewer asked if the priors could be shown as figures instead of writing the priors in the text of the working paper. The authors agreed to add more visualizations of the selected priors and noted that several of the priors were already visualized in Appendix C. The authors agreed to refer to the visualizations more in the text of the working paper.

Pairs plots: The authors presented pairs plots using maximum sustainable yield (MSY) reference points and details on the prior assumptions to the group on the second day of the meeting. There was some correlation of the m_1 parameter and B_0 as well as various parameters. The plots showed that there were differences between the stock assessment regions (SARs) except there was little correlation for m_1 and B_0 for the Strait of Georgia (SOG) plot. The authors also ran sensitivities for West Coast Vancouver Island (WCVI) that showed the shape of the curve was driven by m_1 depensation estimates. After reviewing the figures, the group decided it would be beneficial to include new figures for all five areas in Appendix C of the working paper.

Divergent transitions: A reviewer asked if the authors could describe in detail what steps they took to remove divergent transitions from the model runs or explain precisely why they believe the model fits and resulting posteriors can be trusted. The authors indicated that there was extensive Hamiltonian Monte-Carlo (HMC)/STAN tuning via adapt_delta and tree depth settings to back up the simulation estimates. Simulation tests and uncertainty analyses also provided external justification in the model estimates. The authors have agreed to add more explanation in the working paper for this section.

Non-equilibrium initial states: The authors were asked if more explanation could be added to the working paper on the estimation of Rinit instead of Finit and to include phasing in the parametrization table. The authors agreed to both requests.

Natural Mortality (*M*): Herring natural mortality over time was estimated to be different across all five stock assessment regions (SARs). It was hypothesized that variation in *M* was linked to ecosystem changes within each region and was most likely driven by changes in predation levels. The authors mentioned that they did not focus on predation and its effect on *M* since other projects are working on this topic. The reviewer asked if the authors explored age-varying *M* and they indicated that age-varying *M* was not considered since age-1 herring are not sampled and thus accurate age-1 data are not available.

Depensatory *M*: A participant asked what regions have better information and how this affects the estimation for m_1. The authors indicated that the comparison of depensatory *M* was explained in the discussion of the working paper. Another participant asked why *M* was parameterized with depensation instead of compensation.The authors explained that the projected random walks did not match the historical estimates. Finding ways to modify the parameterization of *M* in the projections to include compensation had little success.

Density-dependent mortality: It was suggested that discussion on density-dependent mortality (DDM) could be framed as an alternative time-varying approach in the background section of the working paper. DDM is not necessarily a better model but assumes an ecological process that better fits the data compared to using a random walk process. The authors will explain why depensatory *M* was used and will add more text to this section.

Temporal autocorrelation: A reviewer noted that there was no temporal autocorrelation when forecasting *M* or recruitment. They asked if some examples of some of the individual simulations could be shown in the meeting and not necessarily placed in the working paper. The authors showed some individual simulations at the peer review meeting.

Residuals: For Figure C.20 (Prince Rupert District blended spawn index and SISCAH spawning biomass estimates and standardized observation residuals), where the time series shows a decreasing trend from positive residuals (from the 1970 to 1980s) to negative residuals (from ~1990–2000).

In Figure C.41 (Central Coast age-1 recruitments and standardized recruitment process errors) there appears to be a decreasing trend in the residuals from 1950–1990 that does not have a trend line or other residuals plots like in previous sections. The authors agreed to add trend lines to all recruitment deviation plots.

TERMS OF REFERENCE OBJECTIVE TWO

Assess suitability of the new statistical catch-age model for the five major Pacific Herring stocks for estimating trends in biomass, depletion, and recruitment for each major stock. Present retrospective evaluation, trends, and stock status in 2022 relative to the limit reference point

(Kronlund et al. 2018) and the upper stock reference point based on the average productive period (DFO 2023a; DFO 2023b).

Model goodness of fit: All five SARs (Haida Gwaii (HG), Prince Rupert District (PRD), Central Coast (CC), Strait of Georgia (SOG), and West Coast of Vancouver Island (WCVI)) were evaluated individually for the goodness of fit.

For all SARs, there were no indications of poor model fit for recruitments relative to biomass around the stock-recruitment curve and SISCAH was able to track the cohort signals.

SISCAH harvest rates: A reviewer requested more precise writing and context on the B_{MSY} harvest rates. Using the SISCAH model, it was found that MSY was generally lower and B_{MSY} was higher for DDM. The equilibrium yield relationships with m_1 were not symmetrical and the value of m_1 had a significant effect on populations with low abundance. The authors agreed to frame the discussion and provide more information about the allocation to $B_{MSY.}$

MSY reference points: The SISCAH model provides more realistic estimates for the MSY-based reference points which results in lower harvest rates compared to ISCAM. In ISCAM, the MSY-based reference points are based on time-average natural mortality time-series in closed loop simulations. The question was raised why MSY is not plausible in some of the figures and for some results. The authors agreed to include more explanation and rationale for the approach in the working paper.

A reviewer requested some clarification on the density-dependent formulation for *M* and its depensatory effect on the population at low stock sizes. The authors discussed this point at the meeting and will include some text in the working paper.

It was also noted that MSY reference based points were highlighted in the working paper but not recommended. A participant requested more of an explanation of why they are not recommended. The authors agreed to include a discussion on reference points in the future work section.

Stochastic B_0 and deterministic B_0: A discussion occurred about the performance of the model and how stochastic B_0 is strongly correlated to deterministic B_0 with process error. Participants asked to clarify in the working paper the use of stochastic B_0 compared to using deterministic B_0 since SISCAH showed the data in a different way. The model is generating lower catch levels for herring than in previous assessments and this would be a change for fisheries management. Structural assumptions of a density-dependent model like SISCAH could affect the estimated long-term average of unfished biomass which would cause differences in the reference points (i.e., a lower estimate for the limit reference point (LRP)) and stock status.

It was suggested that the two estimated values for B_0 be included in Table 9 (SISCAH model standard errors for observational data in all major stock assessment regions and lag-1 autocorrelation factors for the age composition LN1 likelihood function) and to include some more discussion in the working paper on the two values.

A member of the group asked about the relevance of MSY targets for open-pond in the spawn-on-kelp (SOK) fishery and the authors have agreed to provide some context to this section in the working paper.

Allocation (MSY) plots: For areas with low depensatory effects (PRD and SOG), a participant asked if SOK versus roe allocation could be included in the working paper. These figures were added to the SAR and the authors agreed to add them and discussion to the working paper.

Forecasting data: A question was raised regarding forecasting data and why the last two years of data were not used in the forecasting. The authors explained that they simulated recruitment starting in 2020 to avoid the reliance on cohorts that have only been observed once.

TERMS OF REFERENCE OBJECTIVE THREE

Demonstrate the new statistical catch-age model as an operating model for the Pacific Herring MSE process. Identify key uncertainties related to environmental variability, natural mortality, data and assessment methods, and evaluate these in the context of precautionary approach (PA) compliant management procedures.

Catch data: Clarification was sought about whether the authors had considered all the unknown bycatch of herring in other fisheries (i.e., recreational and commercial) that would influence B_0. The authors explained the bycatch is implied in the M estimates and it could be explored in the future.

Perfect information: A member of the group asked if the stochastic runs could be included in the working paper to demonstrate what the model can do. The perfect information and the stochastic process information could be used to develop the model and make it more realistic. The authors explained that the perfect information runs were included in the working paper to demonstrate proof-of-concept and that future work would include stochastic process information.

Precautionary approach: A recommendation was made for the authors to include more text on the reference points with regard to the precautionary approach (DFO 2009) in the working paper. A participant wanted to understand the history of the policy in relation to the LRP and upper stock reference (USR) for herring. The authors noted that these models represent the precautionary approach to reduce the probability of overfishing and producing irreversible harm to stocks.

Alternating models of *M*: A participant suggested that the authors explore other OMs such as a linear model (e.g., Logan et al. 2005; Holt et al. 2015). A linear model may not be a realistic representation of the real relationship, but it may have statistical benefits. The authors indicated that they already visually explored the Logan et al. 2005 model, and it is feasible to include in the current SISCAH framework; however, they believe the Logan formulation will remove a stable unfished equilibrium unless an asymptotic lower limit is applied, as in the current SISCAH formulation. Authors will add an item to the future work section in the SAR to explore model performance when using a different density-dependent mortality model structure.

Reproducibility of the model: A reviewer expressed concern that the SISCAH model would be difficult to reproduce since it was not clear to see the assumptions with the prior choices. The prior notation within the working paper seemed to be more complex than necessary to the reader and they asked why tilde notation was not used. Writing the math in a complex way makes it more difficult for the reader to "see" the assumptions used for each prior. The authors indicated the notation is a reflection of the model's code. They agreed to revise to tilde notation in the working paper for clarity.

Matrices of empirical weight-at-age: A reviewer observed that the matrices of empirical weight-at-age ($w_{a,t}$) do not always display biologically realistic trends. They were curious if more work was done to try to improve these estimates. The authors assumed that the reviewer's comment is referring to Figures A.5–A.9 and the flat lines within the figures show imputing average weight-at-age estimates when there was missing or unavailable data. It was necessary to impute the averages since the catch-age model requires a complete matrix of weight-at-age for all years.

Spawn-on-kelp fishery model: Spawn-on-kelp (SOK) fishery has been included in a model for the first time. A participant asked about the assumptions that were made about the SOK fishery since the survival rates seem to be high and a bit optimistic from the mortality calculation. The working paper indicates a survival rate of 78% which is very specific and the participant understood that mortality could be in a range from 0 to 60%. The authors explained that for demonstration purposes it was assumed that all SOK harvest was from closed-pond fisheries, but that mortality rates from open pond fisheries could be applied in the future.

A participant asked if mortality as a function of oxygen level related to depth of deposition was examined and if not, would it be considered as future work. The authors explained that the SOK index does not consider egg mortality in the survey. The survey index estimates the number of eggs and then uses this value to determine the number of adults that laid those eggs. When extremely thick layers of spawn are present in an area, it may be that the eggs on the bottom layers would never hatch due to the lack of oxygen. This assumption could potentially lead to an overestimation of future returns.

A participant asked if mortality from other sources was accounted for. The authors indicated that they used the available data but if there was better bycatch or other sources of M that those data could be included to test more hypotheses and help fit the model.

The SOK removals had little effect on the biomass trends but creates yield curves that are sensitive to allocation among fisheries. There was a fairly minor effect on this for Haida Gwaii (HG) but it is important to include to understand the impact.

Dive survey: A participant noted that both the ISCAM and SISCAH models assumed that the dive surveys observed all available herring (i.e., q equals a fixed value of 1). This assumption can result in biomass underestimates if the survey misses herring spawn. They also suggested that the dive protocol could be included in the working paper. The authors explained they used an informative prior to determine spawn survey catchability.

Another participant suggested that if the catchability for the dive survey had a wider coefficient of variation (CoV), it could be used as a sensitivity analysis. The authors suggested estimation of q could be future work.

Blended index approach: The authors treated the spawn indices as a blend of dive and surface surveys. The blended index weighted catchability by proportion of the spawn index from each survey type.

A participant asked why the surface surveys for the last two years for HG were noted as diver observations. They also noted that the biomass value from the dive and surface surveys for HG could be inflated due to the lack of field work during the COVID-19 pandemic. The authors explained that the surface survey used drop cameras, and took egg layer samples, so was more similar to a dive survey than a surface survey. They agreed to add language to explain that in the working paper.

Historical catch: A participant voiced concern that the age compositions from catch data from the 1950–60s are more uncertain because those age compositions differ from the historical and local knowledge of how the reduction fisheries operated. One of the authors mentioned that old catch data contain age composition information but juveniles were underrepresented in the samples relative to historical accounts. The catches were modelled using available data, mainly ages 2 and up.

Environmental considerations: Climate change and ocean productivity were not addressed in the analysis and should be considered an uncertainty. However, environmental variability was modelled implicitly through time-varying natural mortality and recruitment in SISCAH.

TERMS OF REFERENCE OBJECTIVE FOUR

Propose an implementation plan for the Pacific Herring MSE process, including timelines for routine MSE updates, choice of operating models to include, and exceptional circumstances *that would trigger an earlier update of the MSE process than what is proposed in the implementation plan.*

Reassessment frequency: It was recommended that a minimum 3-year timeline be used to update the MSE unless new evidence reveals exceptional circumstances (i.e., disease outbreak or extreme anomalies in the data). The implementation and development of exceptional circumstances will be included in the MSE process.

Implementation plan for MSE process: A participant suggested that an implementation plan for the MSE process be included in the working paper. The authors mentioned that there is no roadmap on how to transition from a stock assessment process to advice using an MSE process. With some stocks, a new model is just adopted and implemented while other stocks have a phased-in approach when changing models or processes. All participants agreed that a process for implementing the new assessment and operating model, updates to the MSE, and identification of exceptional circumstances should be developed in a phased approach in consultation with managers, First Nations, and stakeholders.

Future Research

SISCAH spatial component: The spatial component was not used in this evaluation of the SISCAH model. It is possible that the spatial component could be used in future iterations of the model.

Parameterizations: Future work could explore other parametrizations of density-dependent natural mortality, depensatory and compensatory M, and survey catchabilities (q).

Collaborators: It was recommended that DFO continue to work with First Nations, stakeholders, and the provincial government to contribute to the development of the MSE.

CONCLUSIONS

The group was shown the revisions table with all revisions agreed upon by the authors. Meeting participants agreed the working paper satisfied all Terms of Reference objectives and the paper was accepted with revisions.

RECOMMENDATIONS AND ADVICE

DRAFTING OF THE SCIENCE ADVISORY REPORT

One of the authors agreed to track changes on the draft Science Advisory Report while it was being discussed with participants during the meeting. The Science Advisory Report was discussed at length and participants had the opportunity to contribute to key sections. At the end of the meeting, a draft Science Advisory Report was completed. The meeting Chair will work with the authors to finalize the draft Science Advisory Report. Once completed, the Centre for Science Advice Pacific (CSAP) office will circulate the draft SAR and draft Proceedings to all participants for final review and input.

ACKNOWLEDGEMENTS

The Centre for Science Advice Pacific (CSAP) congratulates the authors on a successful paper and appreciates the contributions from all participants. We thank the formal reviewers, Chris Cahill (Michigan State University) and Elisabeth Van Beveren (DFO Science) for their time and expertise providing their formal written reviews of the working paper. We would also like to thank Steve Schut for chairing the meeting.

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Logan, G., de la Mare, W., King, J. and Haggarty, D. 2005. Management Framework for Strait of Georgia Lingcod. DFO Can. Sci. Advis. Sec. Res. Doc. 2005/048. xiii + 102 p.

Martell, S.J., Schweigert, J.F., Haist, V., and Cleary, J.S. 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. v + 151 p.

APPENDIX A: TERMS OF REFERENCE

Application of a new modelling framework for the assessment of Pacific Herring (*Clupea pallasii*) major stocks and implementation in the management strategy evaluation process

Regional Peer Review – Pacific Region

June 26–28, 2023
Nanaimo, British Columbia

Chairperson: Steve Schut

Context

Pacific Herring (*Clupea pallasii*) in British Columbia (BC) are currently managed in five major and two minor stock assessment regions. The major stocks included in this model review are Haida Gwaii (HG), Prince Rupert District (PRD), Central Coast (CC), Strait of Georgia (SoG), and West Coast of Vancouver Island (WCVI). Pacific Herring are an important species to First Nations, with coastal Indigenous fisheries, as well as treaty and Aboriginal commercial fisheries in specific management areas.

Fisheries and Oceans Canada (DFO) uses a Management Strategy Evaluation (MSE) process to collaborate with First Nations, non-governmental organizations and the Herring Industry Advisory Board to develop and implement sustainable harvest strategies for Pacific Herring. The use of the MSE process aligns with DFO's "A Fishery Decision-Making Framework Incorporating the Precautionary Approach" policy (DFO 2009). The process uses an analytical framework to evaluate sustainability of harvest strategies by simulation testing alternative management procedures against operating model (OM) scenarios that represent a range of hypotheses about uncertain population and fishery dynamics. Performance of alternative management procedures (combinations of data, stock assessment model, and harvest control rule, MP) are measured against pre-agreed conservation and catch objectives for each stock area and fishery (Cox et al. 2019; DFO 2023a).

The MSE framework was first implemented in the herring advisory process in 2018 (Benson et al. 2023; DFO 2020). This framework uses a statistical catch-age model in both the MP (annual assessment of BC herring stocks) and to estimate key population parameters used in the operating models (Cleary et al. 2019; DFO 2018). Previous scientific reviews, ongoing consultation with First Nations and fishery stakeholders, and increasing survey costs indicate a need for added functionality in the assessment and operating models, including:

1. a method for integrating data from surface and dive surveys in estimation of the survey index;
2. inclusion of removals from the spawn-on-kelp fisheries;
3. implementing fishing mortality in discrete time steps within a fishing season; and
4. parameterizing depensatory natural mortality to better represent potential ecosystem impacts on herring stocks.

Fisheries and Oceans (DFO) Resource Management has requested that DFO Science update and review the model used for annual assessment of BC Pacific Herring stocks (choice of model, data, assumptions) to include elements (1) to (4) and demonstrate its utility for representing additional dynamics as operating models in the MSE analytical framework. This process will not include a stock assessment, forecast of mature stock biomass, nor harvest options for 2023 as those aspects will be covered in a subsequent Canadian Science Advisory Secretariate (CSAS) process[1].

Objectives

Guided by the DFO Sustainable Fisheries Framework, particularly the Fishery Decision-making Framework Incorporating the Precautionary Approach (DFO 2009), the following working paper will be reviewed and provide the basis for discussion and advice on the specific objectives outlined below.

Johnson, S.D.N., Cox, S.P., Cleary, J.S., Benson, A.J, Power, S.J.H., and Rossi, S.P. Application of a new modelling framework for the assessment of Pacific Herring (*Clupea pallasii*) major stocks and implementation in the management strategy evaluation process. 2023. CSAP Working Paper 2019PEL01.

The specific objectives of this review are to:

1. Present a new statistical catch-age model including simulation-estimation tests and comparisons with 2022 base model parameterizations from the previous model (Cleary et al. 2019 modifications to Martell et al. 2012).
2. Assess suitability of the new statistical catch-age model for the five major Pacific Herring stocks for estimating trends in biomass, depletion, and recruitment for each major stock. Present retrospective evaluation, trends, and stock status in 2022 relative to the limit reference point (Kronlund et al. 2018) and the upper stock reference point based on the average productive period (DFO 2023a; DFO 2023b).
3. Demonstrate the new statistical catch-age model as an operating model for the Pacific Herring MSE process. Identify key uncertainties related to environmental variability, natural mortality, data and assessment methods, and evaluate these in the context of precautionary approach (PA) compliant management procedures.
4. Propose an implementation plan for the Pacific Herring MSE process, including timelines for routine MSE updates, choice of operating models to include, and exceptional circumstances that would trigger an earlier update of the MSE process than what is proposed in the implementation plan.

Expected Publications

- Science Advisory Report
- Research Document
- Proceedings

Expected Participation

- DFO Science and Fisheries Management
- Academia
- First Nations
- Industry

[1] 2023/2024 Herring Stock Assessment, Forecast of Mature Stock Biomass, and Harvest Options. Canadian Science Advisory Secretariat Science Response Process, September 2023.

- Government Agencies
- Non-Governmental Organizations

References

Benson, A.J., Cleary, J.S., Cox, S.P., Johnson, S., and Grinnell, M.H. 2023. Performance of management procedures for British Columbia Pacific Herring (*Clupea pallasii*) in the presence of model uncertainty: closing the gap between precautionary fisheries theory and practice. DFO Can. Sci. Advis. Sec. Res. Doc. 2022/048. viii + 70 p.

Cleary, J.S., Hawkshaw, S., Grinnell, M.H., and Grandin, C. 2019. Stock Assessment for Pacific Herring (*Clupea pallasii*) in British Columbia in 2017 and forecast for 2018. DFO Can. Sci. Advis. Sec. Res. Doc. 2018/028. v + 285 p.

Cox, S.P., Benson, A.J., Cleary, J.S, and Taylor, N.G. 2019. Candidate Limit Reference Points as a Basis for Choosing Among Alternative Harvest Control Rules for Pacific Herring (*Clupea pallasii*) in British Columbia. DFO Can. Sci. Advis. Sec. Res. Doc. 2019/050. viii + 47 p.

DFO. 2009. A fishery decision-making framework incorporating the precautionary approach.

DFO. 2018. Stock assessment for Pacific Herring (*Clupea pallasii*) in British Columbia in 2017 and forecast for 2018. DFO Can. Sci. Advis. Sec. Sci. Advis. Rep. 2018/002.

DFO. 2020. Proceedings of the Pacific regional peer review of the Evaluation of Management Procedures for Pacific Herring (*Clupea pallasii*) in the Strait of Georgia and the West Coast of Vancouver Island Management Areas of British Columbia; July 25-26, 2018. DFO Can. Sci. Advis. Sec. Proceed. Ser. 2020/027.

DFO. 2023a. Management Strategy Evaluation Update and Evaluation of Upper Stock Reference Point Options for Pacific Herring (*Clupea pallasii*) in British Columbia, Canada. DFO Can. Sci. Advis. Sec. Sci. Resp. 2023/002.

DFO. 2023b. Pacific Region Integrated Fisheries Management Plan, Pacific Herring, November 7, 2022 to November 6, 2023. 22–2230: 234 p.

Kronlund, A.R., Forrest, R.E., Cleary, J.S., and Grinnell, M.H. 2018. The Selection and Role of Limit Reference Points for Pacific Herring (*Clupea pallasii*) in British Columbia, Canada. DFO Can. Sci. Advis. Sec. Res. Doc. 2018/009. ix +125 p.

Martell, S.J., Schweigert, J.F., Haist, V., and Cleary, J.S. 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. v + 151 p.

APPENDIX B: ABSTRACT OF WORKING PAPER

This paper presents a new stock assessment and operating model framework for Pacific Herring (*Clupea pallasii*) that attempts to resolve time-varying natural mortality, fishery reference points, and future harvest strategies. We develop and test a new Spatially Integrated Statistical Catch at-Age Herring (SISCAH) model that incorporates, inter alia, a density-dependent process for natural mortality, explicit representation of spawn-on-kelp fisheries, fisheries with discrete removal events over a short season, a single spawn index that blends dive and surface survey designs, and an age-composition likelihood function that accounts for correlation in residuals among age-classes.

The primary purpose of the paper is to evaluate SISCAH as a stock assessment modelling framework for each of the five major stock assessment regions (SARs), while our secondary purpose is to evaluate SISCAH as an operating model for future management procedure testing.

Overall, estimates of absolute biomass from the full SISCAH model (i.e., incorporating all new features) were qualitatively similar to the existing assessment framework, although the structural change to natural mortality processes, in particular, generated differences in recruitment and abundance over time, as well as biological reference points. When applied as a stock assessment for each region, SISCAH meets goodness of fit criteria with low sensitivity to model priors and no strong retrospective patterns. In simulation self-tests based on two SARs, relative error distributions showed relatively small amounts of bias for most parameters.

The most impactful change in SISCAH was a component Allee effect under density-dependent natural mortality (DDM) model that provides an ecologically plausible interpretation for patterns in Herring productivity and status in each SAR, such as slow recovery in Haida Gwaii or West Coast of Vancouver Island and persistent high productivity in Strait of Georgia. DDM also leads to plausible estimates of MSY-based biological reference points, with higher optimal biomass and often lower maximum sustainable yield than previous approaches.

Finally, we demonstrate the operating model capability of SISCAH in generating patterns of recruitment, natural mortality, and biomass that are qualitatively similar to those estimated from the historical period. In general, persistent periods of low productivity interspersed with bursts of high productivity arise from the combination of compensatory recruitment and depensatory natural mortality. These properties provide more realistic situations in which to test performance of potential future harvest strategies for all Pacific Herring SARs.

APPENDIX C: WORKING PAPER REVIEWS

Date: 13-jun-23

Reviewer: Elisabeth Van Beveren, DFO (Quebec region)

CSAS Working Paper #: 2019PEL01

Working Paper Title: Application of a new modelling framework for the assessment of Pacific Herring (*Clupea pallasii*) major stocks and implementation in the management strategy evaluation process

The following five questions provide general guidance for your review:

1. Is the purpose of the working paper(s) clearly stated?

Yes. Although the introduction focusses understandably mostly on the natural mortality part, in part 1.3 the entire purpose is clearly defined.

2. Has the working paper fulfilled the ToR objectives?

Yes. The paper presented a new statistical catch-at-age model, including all information (background, equations, etc.) and data (landings, surveys, biological data) needed to review it. The quality of the model could be assessed with all necessary tools (simulation testing, model fit, retrospective patterns, sensitivity analyses, etc.). The reference points and the estimation algorithm were both provided. Uncertainties related to the data input were identified as well as those related to parameter estimation.

I should note that although part of objective 4 is to “propose a choice of operating models and exceptional circumstances”, this was not explicitly done. I however believe that this can be better addressed at another moment and support the authors in their decision.

3. Are the data and methods adequate to support the conclusions, and explained in sufficient detail?

Yes. Every data source, including its uncertainty, was clearly described. Where necessary, there were references to documents providing further details. Model specifics (equations, parameters, etc.) were included and explained in detail. When desirable, the authors took care to even highlight model behavior in function of its unique properties (density-dependent natural mortality).

4. If the document presents advice to decision-makers, are the recommendations provided in a useable form, and does the advice reflect the uncertainty in the data, analysis or process?

There are recommendations related to the future steps of the MSE, which are clear and helpful.

5. Can you suggest additional areas of research that are needed to improve the working paper?

An enormous amount of work was presented in this working paper. Moreover, it was all very well structured and easy to follow. All the essential information was provided, and despite the length of the document, there was in my opinion nothing superfluous. I would like to highlight the quality of the analyses and the document in general, and can certainly see how it will help future herring assessments (or MSEs).

I do not have major concerns. I made minor suggestions and indications for clarification directly in the working document. A summary of some of those comments:

1. Simulation analyses show that some bias is possible. Although the authors note that this is of no major concern, some work could perhaps be done to demonstrate why that is. For instance, it might be possible to relate the potential magnitude of bias with estimated uncertainty distributions. Are they likely to still contain the true value? Or more directly, how could this affect our perception of stock state (error relative to reference points or other metrics on which management bases its decision)? Further, can some of the bias, such as typical in a terminal year, be mitigated (start year projections, suggestion for OM, etc.)?
2. Sensitivity analyses for Mb (e.g., what if this value is much lower?) and m1 would be of interest?
3. The closed-loop simulations are based on a scenario with perfect observations. Although for demonstration there is nothing wrong with this, it would presumably have been relatively easy to do this with more realistic imperfect observation, which will ultimately be used in the MSE? Why was this choice made?
4. The matrices of $w_{a,t}$ do not always display biologically realistic trends. Although I do not expect this to influence the results, has work been done in the past to try and improve these estimates?
5. This document focusses heavily on time-varying M. Was there any work done in the past to explore whether M might also be driven by changes in predation? Whether age-varying M might be more likely?

Date: 19 June 2023

Reviewer: Christopher Cahill, Quantitative Fisheries Center, Michigan State University

CSAS Working Paper #: 2019PEL01

Working Paper Title: Application of a new modelling framework for the assessment of Pacific Herring (*Clupea pallasii*) major stocks and implementation in the management strategy evaluation process

I structured my review by first answering the five questions provided to guide my review of the working paper. I then follow this section with minor editorial comments, which are extensive given the length of the working paper. In general, most of my comments are aimed at improving clarity and reproducibility of the analyses conducted in this working paper.

MAIN COMMENTS

Is the purpose of the working paper(s) clearly stated?

Yes. The purpose of the working paper is clearly stated in the Introduction.

Has the working paper fulfilled the ToR objectives?

The working paper has fulfilled the ToR objectives. The authors have added functionality in the SISCAH model that allows them to integrate data from surface and dive surveys, include removals from spawn-on-kelp fisheries, implement fishing mortality in discrete steps within the fishing season, and include depensatory adult natural mortality within a new assessment model. Furthermore, the authors conducted a transition analysis (to show how this model differed from the ISCAM assessment model), self-tests, a sensitivity analysis, and a demonstration of the SISCAH model's utility in performing a management procedure evaluations.

Are the data and methods adequate to support the conclusions, and explained in sufficient detail?

A great deal of effort and technical expertise has gone into the development and testing of the SISCAH model for Pacific Herring fisheries, and the authors should be commended for their efforts. However, I believe it would be extremely difficult for anyone other than the analyst(s) to reproduce the findings of this paper given the current level of detail provided in text. It is my interpretation that this is at odds with the "sufficient detail" wording in this guiding question, and consequently I believe that more rationale and technical detail needs to be provided and that the authors likely should also make the code required to conduct these analyses publicly available. My comments here are aimed at improving the reproducibility of these analyses as per standard reporting guidelines for Bayesian analyses (e.g., see Kruschke 2021).

Additionally, while the working paper conducted several Bayesian analyses, many of the priors were set without providing sufficient detail on why these specific priors were chosen or why they were ecologically defensible (i.e., see priors for important parameters such as steepness, Sbo). Furthermore, in situations where the authors wrote the math out for their priors they chose to do so in an unnecessarily dense way (see Table 3), where instead of the relatively common tiddle notation (i.e., "parameter ~ N(0, 1)") they chose a style that makes it more difficult to "see" the assumptions used for each prior.

Another concern regarding priors arises from how the priors for M1, M0 were set in lines 500-506 for the density dependent M model. It appears that the authors are essentially using the data twice—first, the authors fit a density independent model to the available data and then estimated plausible values for a density dependent M relationship, and then they read these estimated values in as priors into a second density dependent M model and ran an additional estimation scheme on the same datasets they used to generate the prior values. Unless I am misunderstanding this section, the way these M priors are being set is not appropriate and represents circular logic (see Gelman et al. 2013; McElreath 2016; Royle and Dorazio 2008).

In addition to the difficulty of following the rationale and math for prior choices, I also found some of the technical claims made by the authors alarming. For example, the idea that divergent transitions are acceptable within a Hamiltonian Monte Carlo/Stan posterior at levels <3% of the total number of draws is unfounded and not supported by *any* of the current literature on Bayesian modeling using Stan (McElreath 2016; Stan Development Team 2023). Divergent transitions indicate the sampling algorithm used by Stan was having a difficult time exploring the posterior, and such problematic posterior geometry can indicate 1) model identifiability issues, 2) model-prior-data conflicts, or even 3) that a model of such complexity simply should not be fitted to a given dataset.

The current consensus on divergent transitions is that if you cannot get them to go away via changing the sampling algorithm's step size, a reparameterization, or by changing the priors one simply cannot trust the resulting posterior emerging from the Hamiltonian Monte Carlo algorithm. It is my opinion that the authors should describe in detail what (if any) steps they took to remove divergent transitions from their model runs or explain precisely why they believe their model fits (and resulting posteriors) can be trusted despite the large body of evidence on Hamiltonian Monte Carlo diagnostics suggesting the opposite to be true (e.g., see Best and Punt 2020; Stan Development Team 2023). The current statement on page 11 implying the divergent transitions are acceptable at these levels because the authors wanted to avoid high amounts of parameter regularization is neither compelling nor sufficient.

Lastly, while I acknowledge that the authors conducted prior sensitivity tests for some priors, in general there are other important tests for Bayesian models that should be conducted. These include prior predictive checks (to see if the model/prior combinations are generating patterns consistent with the current knowledge of Herring dynamics) and posterior predictive checks (to demonstrate how well the model(s) fit the observed data). These are standard Bayesian model validation steps and thus if the authors are not going to do them they should in the least tell readers why they are choosing not to do so (e.g., see Gelman et al. 2013; Kruschke 2021; Stan Development Team 2023).

If the document presents advice to decision-makers, are the recommendations provided in a useable form, and does the advice reflect the uncertainty in the data, analysis or process?

Yes. However, I will note that the closed-loop MP simulations depend upon the posteriors estimated via the Bayesian analyses.

Can you suggest additional areas of research that are needed to improve the working paper?

I thought the author's list of future research recommendations in Section 11 addressed the major research avenues I would suggest for this species.

REFERENCES:

Best, J. K., and Punt, A.E. 2020. Parameterizations for Bayesian state–space surplus production models. Fisheries Research 222:105411.

Gelman, A., Carlin, J.B., Stern, H.S., Dunson, D.B., Vehtari, A., and Rubin, D.B. 2013. Bayesian data analysis. Boca Raton, FL: CRC Press.

Kruschke, J.K. 2021. Bayesian analysis reporting guidelines. Nat Hum Behav. 5:1282–91.

McElreath, R. 2016. Statistical rethinking: A Bayesian course with examples in R and Stan. Boca Raton, FL: CRC Press.

Royle, J.A., and Dorazio, R.M. 2008. Hierarchical modeling and inference in ecology: the analysis of data from populations, metapopulations and communities. Elsevier.

Stan Development Team. 2023. Stan Modeling Language Users Guide and Reference Manual, 2.3.2.

MINOR COMMENTS:

Introduction

Line 136: Suggested wording change “no fishing” should likely be “no sustainable fishing”

Line 141-143: Unclear wording in this sentence “In some of these…”

Line 149: Unclear from this phrasing why the authors think this risk is “seldom” assessed (I agree with you I am just saying the wording doesn’t show why this is seldom assessed).

Line 152: I think this topic sentence is vague and misleading to some extent as there are likely abiotic drivers of M as well, even if the majority of this particular CSAS focuses on density dependent M. Whether populations are regulated by density-dependent processes, density-independent processes, or some combination of both types of processes is a deep question with a rich body of ecological theory underlying it. My point is, I don’t think you can (without very good evidence) rule out density independent influences on M, and the wording in this section seems to imply that at least to me.

Line 172: re: “explanatory”. Do you mean mechanistically motivated rather than explanatory?

Line 182: Either remove “non-linear” or explain your rationale

Line 178-180: Are the authors saying their proposed approach is not parsimonious?

Line 184: Delete the sentence beginning with “Thus”, it does not seem relevant

Line 188-195: The example proposed by the authors does not preclude density independent processes being the reason that M is actually increasing, and thus the example is confusing. If you assume M is increasing due to density dependent processes but in reality it is increasing due to density independent processes, this could presumably be a problem. Thus, I recommend the authors tidy up their logic/writing here. Re: statistical uncertainty, I don’t think this is correct because you could model the dependency in successive M’s with or without a structural form underlying that function. Anders Nielsen has examples of this with state-space models and presented on these methods at the Rome CAPAM assessment workshop during October 2022.

Line 218: No dynamic programming or analytical solutions means you should likely drop the “optimal” here and elsewhere throughout the CSAS when referring to harvest policy performance.

General comment: it might help to explicitly talk about the stocks that have not recovered or that have recovered more slowly than would be expected under compensatory stock-recruitment models without density dependent M. Such information, when coupled with the observation that predators have increased, could be used to help motivate why DD M is important (rather than a depensatory stock recruitment function or some other way to model depensatory processes). Food for thought.

Line 250: authors should explain “ponding” jargon as this is the first time it is introduced in the CSAS.

Methods

Line 336: I would say multiple likelihood components rather than multiple types of data to be more precise.

Lines 388-391: Explain to readers why there is a difference here (implied using “either”)

Line 395: Why were age one fish assumed to have a constant M1? This seems counterintuitive given the premise that is forwarded suggesting it is important to consider M as density dependent. I think I know why this was done but the point is the authors should spell this out for readers.

Line 410: This seems like a wordy way of saying there isn’t a nice equilibrium solution as there is when you assume constant adult M

Lines 421-423: This would seem to be a problem from a statistical estimation perspective, and so the authors should explain why it is not or what they (authors) intend to do about it if they think it is indeed an issue. Or, perhaps, how this limits their inferences/interpretation of results.

Lines 424-440: This section may be fine but it is necessarily ambiguous because it is words rather than math and code. This model is complex enough that likely both the code and the math will be necessary to share for this work to be reproducible, as per my main comments above.

Line 490: Authors should provide more detail around what a tail compression procedure is and why it is justified in this specific case (with citations/support from the literature).

Line 495-497: Standard normal priors have a specific meaning in the Bayesian literature and are “variable ~ Normal(mu=0,sd =1)”, and so the math in the table is confusing to me. Why did the authors choose to write their priors in this way? This format/style seems to be unnecessarily difficult to evaluate.

Similarly, the entire section on model priors needs to be more adequately motivated and described. Where did these values come from? Were there prior sensitivity tests done? Were there prior-predictive checks performed to establish whether the priors were strongly influencing the inferences stemming from these models? Are readers simply to assume that the analysts have done this correctly—as written it is difficult or even impossible to evaluate. These are standard best practices for Bayesian analyses and thus should at least be discussed in the methods. See Kruschke 2021 for a nice write up of Bayesian model reporting guidelines.

Lines 500-506. This is not a valid way to set priors. The authors are essentially using the data twice to set their priors here by fitting a model to the data, taking the M estimates, and then fitting another model to that same to get their prior values for a different DD M model, which is then fitted back to the same dataset. Unless I am misunderstanding what is written here/being done, the way the authors are choosing to set these M priors is not supported by any literature on Bayesian statistics (e.g., see Gelman et al. 2013; McElreath 2016; Royle and Dorazio 2008).

If this is indeed what has been done, it seems to represents circular logic—fitting a model to a dataset to get a prior value and then refitting a different model using those prior values that originated from the same dataset to draw inferences about DD M (or evaluate harvest policies or assess the stock or whatever).

Another way to think about this is that a prior is (critically) a way to represent information that occurred *prior* to the current dataset being collected.

Line 510: This is one way of saying the model will not converge unless we assume we know steepness, which most of the fisheries science literature would suggest is likely not reasonable. Thus, the authors should explain how this choice of a remarkably strong (i.e., fixed) prior influences any inferences stemming from this choice later on in the CSAS report. One might expect it to strongly influence the upper exploitation rate limit to a harvest policy such as the precautionary rule form used by DFO, because steepness drives estimates of Umsy or Uref.

Line 512-513: This is not adequately described. If the authors are going to use informative priors they need to describe why/how they chose to do so, and what safeguards they took (see comments above) to make sure these analytical choices did not unduly influence their findings/advice. See Kruschke 2021.

Line 522: I am confused, is this "fixed assumption" the same as the prior listed in line 516?

Line 531-533: This is very confusing. Are you simulating from the prior to get a value that you then feed back into setting that prior? Which distribution are you talking about here? The prior?

Line 535:536: Is there a reference the authors can provide supporting this approach? In general all of these subjective prior choices need to be explained so that the analysis is reproducible and more objective.

Line 552: Code should be made available to improve reproducibility, especially if there is the intention of using this model to do MSE things for DFO in the future.

Line 561: unclear what is meant by "mixed model" and "absolute sample sizes" here

Lines 575-580. See major comments above.

One possibility for why this model runs into numerical difficulties is simply because there is not enough information content in the available data to estimate the things the authors want to estimate. This might mean that the model is degenerate or non-identifiable, and may be "held together" by their informative (and subjective) prior choices. This in general would not surprise anyone, as M is difficult to estimate in many stock assessments and I think the onus is on the analysts to show us that this is not happening with their model.

Line 605: What was the purpose of the random priors simulation? i.e., what specifically were the authors looking at/showing readers with this?

Lines 607-608: Was convergence achieved in each of the simulation replicates as per the criteria the authors listed for nlminb() fits above? If not, the authors should say what proportion of the models converged. This comment assumes this simulation was not done using full HMC/posterior simulation.

Lines 621-626: Did the authors consider posterior predictive checks, which are standard for Bayesian model evaluations? If not, why not?

Lines 645-646: Is this unpublished information from the authors? Unclear.

Lines 653-657: These insights are complicated enough that a figure or two should be used to show readers what is going on so that they are able to follow the analysts' logic more closely here—ditto for the remainder of this section.

Lines 678-684: Why are the authors jumping between Fcrash and Ucrash? Pick one or the other and keep it consistent. Again, difficult to follow what is going on in this section as it is written and a good figure or two might go a long way toward improving clarity.

Line 694: A citation should be added to bolster this claim.

Line 711: These should be visualized for readers lacking a technical background—as it stands it is difficult to know why these highly informative/specific priors were chosen and thus the analysts are expecting readers to "take it on faith" that these values are reasonable.

Lines 723: Again, this is confusing and it would be far clearer for the analysts to show readers their priors via figures rather than state them in the way that has been done here.

Lines 727-730: And what are the specific prior values for a50 selectivity? This is unclear as written—i.e., I do not think it is enough to simply say we used a “lognormal prior for a50 selectivity”.

Lines 840-841: Whether this is in another report, it is confusing here as written. In particular the “where the USR is defined as a proportion of mean biomass over a year range corresponding to a productive period, given in Table 6 (DFO 2023a)” bit.

Results

Line 939: Why 2+Bo?—not described for reader so unclear

Line 947-948: This sentence is unclear

Lines 954-955: Better to tell readers precisely what these observation model parameters are rather than expect them to go look this up in a table in a 237 page report.

Lines 966: Unclear what is meant by “changes in the underlying deterministic model”

Line 969: I still don’t understand this random priors simulation bit. Why not just pick increasingly more vague priors or something like that rather than do this? Thus far this CSAS has communicated little to no motivation on why this specific simulation test is being conducted, which makes it hard to evaluate the findings for this chunk of the simulation listed here.

Lines 982-985: This is disconcerting, and so I believe something is needed here from the authors to defend why they believe this is acceptable.

Lines 987-999: Expect to see some discussion of this later on, particularly as overestimating SBo might be expected to result in an overly optimistic TAC? Or, am I missing something here? Also, that m1 is clearly so dependent on the selection of an informative prior suggests strongly that there is little information within these datasets for SOG to estimate it, right? There should be sufficient discussion of this if this is indeed the case in the Discussion.

Line 1003: At what point does non-identifiability preclude the analysts' ability to decipher whether DD M is going on? I think there should be some explicit discussion around this in the text.

Line 1015: Is this over-estimation a concern? i.e., if you think there is more vulnerable biomass than there actually is one might expect this to lead to overly optimistic TAC recommendations.

Lines 1018-1020: Unclear why the authors think this is acceptable

Line 1032: Then isn’t this a problem for at least some stocks as there does not appear to be a great deal of information within the data to estimate m1 as per comments above? Authors should make this clear for readers and what the implications of this are for their main findings/conclusions.

Line 1046: Tell the reader precisely where this is located in the document

Line 1073: Figure 9. What are the phases being referred to in the caption for this plot?

Lines 1083-1085: This seems counterintuitive and thus I believe the authors should spend a bit more time discussing/clarifying this.

Line 1095-1097: Can you state this more clearly/precisely? For example, which life history quantities are you speaking of? All of them?

Lines 1107-1108: This claim that the results weren’t sensitive to prior choice seems to be at odds with statements made about the m1 prior for at least some of the SARs (see above comments)—is M not a component of the life history parameter set?

Lines 1112-1114: But h had to be fixed for at least one of the SARs and it was stated above that the m1 parameter was difficult to estimate. This is in general very confusing—I think there should be much more detail surrounding the sensitivity to informative/subjective prior choices made by the analysts here.

Discussion

Line 1217: potential effects? After all, you haven't demonstrated that DD M is the mechanism that has caused the populations to recover slowly or anything like that.

Line 1224: re: why herring are a good case study for DD M. Authors should put all of this information in the introduction so readers unfamiliar with their system(s) can use it to better understand where the authors are coming from (e.g., it would seem that at least some information on exploitation patterns among SARs would be particularly germane).

Lines 1232-1234: In addition to saying that SISCAH has higher unfished recruitment, perhaps make an effort to put that into plain English for readers lacking your technical expertise? i.e., why might this specifically matter to DFO managers/public? I think this could be done for many of the statements in this chunk of the CSAS.

Line 1241-1248: This paragraph is vague. Why are those earlier MSY estimates implausible? Perhaps this chunk just needs to be more clearly worded. Also, are the authors stating that the boom and bust cycles are caused by M rather than e.g., recruitment pulses? I guess this is not typically how I think of boom/bust or spasmodic/cyclic fisheries as per say Caddy and Gulland 1983—and I thought they claimed these types of dynamics were typically driven by recruitment rather than changes in adult M.

Line 1249: Again, more plausible based on what? The authors' gut feeling?

Line 1272-1273: I am confused, because I thought recovery should be slower under the DD M scenario.

Lines 1314-1315: I love the paper cited here and think it is well written, but it would seem to be critically important to compare the hierarchical shrinkage estimates of any such future analyses with the fixed-effects/single SAR version to safeguard against the more productive SARs' information content/data pulling the less productive SAR parameter estimates along for the ride. Food for thought.

Lines 1316: I thought some of the age structure plots buried in the appendices from Haida Gwaai and CC(?) showed recruitment pulses in roughly the same years—was this something the authors saw? If so, could be useful for motivating this future research idea here.

Appendices

Looking at the weight at age plots in appendix A—are there any other plausible hypotheses that could explain the patterns in M that the authors are describing using DD M? i.e., declines in mean weight at age are certainly expected for harvested fisheries but ecologically this might be an important determinant of an individuals reserves for suboptimal conditions.

Table B.2.6: Technically h is fixed in PRD so it isn't "estimated" as the caption suggests. In general, probably better to make a plot than to hide everything away in a table.

Line 1955: why does this recruitment standard deviation of 0.04 indicate no systematic bias in recruitments? This is confusing and not explained clearly.

Lines 1960-1961: This statement is only valid given the authors' questionable claim that chains from HMC can be trusted with < 3% overall divergent transitions, and so in the least this should be clearly stated for readers/reviewers. i.e., no one using stan would say that this number of DT's is acceptable.

Line 1967: Highly correlated?

Line 1982-1984: Prior predictive checks would help figure this out and likely should be done

Line 2136-2135: Prior predictive checks would help here as well. A key question for the authors is whether their choice of priors is driving their DDM estimates and hence ln(SBo), as they state there is clear correlation between these quantities and future research should attempt to reparameterize this in some way

Figure C.20. It seems like there are periods where residuals are more likely to be high or low—to my eye there are many positive values 1970-late 1980s, many low values from ~1990-2000 and the authors should provide some discussion around this point.

Figure C.25: I thought h was fixed for PRD but this caption states it was estimated

Figure C.37: Again, some dependence in this residual plot to my eye, so authors might attempt to explain what is going on here.

Figure C.41: There appears to be a decreasing trend in the residuals from 1950-1990. Also, this plot for some reason doesn't have a trend line and other residual plots in previous sections did—the authors should keep them consistent among sections.

Figure C.81: How is the posterior variance zero? Is this simply an artifact of fixing/racheting down the prior variance to a very low value? If so, this should likely be fixed for the performance of HMC rather than ratcheted down.

Lines 2625: it would be helpful if the authors would visualize these equations/sentences in a plot or two to show readers what they are talking about.

REFERENCES:

Caddy and Gulland 1983. Historical patterns of fish stocks. Marine Policy.

APPENDIX D: PARTICIPANTS

Last Name	First Name	Affiliation
Benson	Ashleen	Landmark Fisheries Research
Boldt	Jennifer	DFO Science
Bruce	Kevin	DFO Fisheries Management
Burridge	Christina	BC Seafood Alliance
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Curkan	Curtis	DFO Fisheries Management
Dorner	Brigitte	Heiltsuk First Nation
Driscoll	John	Pacific Marine Conservation Caucus/David Suzuki Foundation
Forrest	Robyn	DFO Science
Frederickson	Nicole	Island Marine Aquatic Working Group
Hawkshaw	Sarah	DFO Science
Johnson	Sam	Landmark Fisheries Research
Jones	Russ	Council of the Haida Nation
Keefe	Marisa	DFO Fisheries Management
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