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Ocean Science Discussions

Interactive comment on "Towards an integrated forecasting system for pelagic fisheries" *by* A. Christensen et al.

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First we are delighted to hear that referee 2 finds the objectives of our paper important and highly relevant to ecosystem-based management of marine species. Below is our response to all specific issues raised by Referee 2. Unfortunately this referee report is structured in a way that makes it difficult to sequentially address all issues in great detail - therefore in some cases we have grouped together related issues to produce a more readable reply

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reviewer: *Technical details about SLAM runs (time-step, larvae per cell, Lagrangian sampling sensitivity analysis, and more detail of the SLAM model formulation)*

authors: The SLAM model in this work is based on our previous work (Christensen et al 2008, Can. J. Fish. Aquat. Sci., 65, 1498–1511) and it is fair to ask readers interested in the detailed model development model to follow this reference. The present manuscript will grow excessively large if a detailed development of SLAM should be repeated

actions: in a revised manuscript we will add certain technical key parameters (time-step, larvae per cell and sensitivity on transport (T) from finite sampling)

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reviewer: *larval sandeel vertical behavior affect transport?*

authors: This may indeed have an influence. However, it is not possible in in present context to address this, because the POLCOMS hindcast committed to MyOcean only contains daily current residuals. Further, biological models on vertical migration is uncertain and at a research stage - this is also true for sandeels. However, from a related study (in prep.) we know that the impact range is not dramatic on the scale of other uncertainties in stock modelling.

actions: We will address this issue in the discussion in a revised manuscript

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reviewer: *The use of one single hatching date (20 February) is not realistic and does not permit any realistic changes due to inter-annual variability and future climate change.*

authors: Indeed the fixed date of hatching is an approximation. In our previous work (Christensen et al 2008, Can. J. Fish. Aquat. Sci., 65, 1498–1511) we made a throughout sensitivity analysis addressing the relation between hatching day (or hatching distribution) and transport connectivity(T). Unfortunately, only rather limited knowledge are available of the relation between spawning/hatching and environmental cues. For sandeel, nothing sufficient to support a parameterization is available.

actions: In a revised manuscript, we will extend the discussion of impact on T from to uncertainties in hatching date (and other factors), based on our previous work.

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reviewer: *If the growth model is just temperature dependent, why has ERSEM been run? Where do the ERSEM variables fit into the rest of the system?*

authors: The short answer is that it is because POLCOMS-ERSEM is the data product committed to MyOcean. It is true that we really do not use ERSEM variables in the present coupling.

actions: To avoid misleading readers we will replace POLCOMS-ERSEM with POLCOMS in a revised manuscript. (Please see comment below).

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reviewer: *It would be interesting to use the ERSEM variables into the growth model more specifically. This would produce variable survival based on food availability which might result in more realistic patterns*

authors: This can be either in (1) the Lagrangian model of larval survival (S) or (2) through the carrying capacity (C). (1) Several studies based on a generic type of bioenergetic model, (see e.g. Letcher et al 1996, Can. J. Fish. Aquat. Sci. 53 (4): 787-801) has been published. These models contain 60+ parameters, most of which can only be guessed or taken from other species. Thus, even though they provide interesting insight into biology, their quantitative skill is uncertain. And there is only limited observations to validate the model. Therefore introducing such a model would require a full paper by itself. Further we have not seen any published results that specifically documents that these models have higher predictive skill than a simple temperature-driven model, as applied in our study. Especially, no well-validated model of this type exist for sandeel. Indeed the predictions of these models are strongly dependent on zooplankton size spectrum, which are seasonally varying. ERSEM does not output zooplankton size spectra, which must be reverse engineered, introducing yet further assumptions. Finally, the bloom dynamics of zooplankton models, including ERSEM, does not match observations sufficiently well yet, even though progresses are good. These remarks also carry over to (2) above, noticing that fish growth/survival response to zooplankton signals is noisy and still at the research stage (it is difficult to disentangle the response to a single driver from other drivers). Following these observations, we feel it is well-warranted that we choose a simpler published and well-characterized growth/survival model in our work. However, to meet reviewer comments we can offer a statistical analysis of unexplained growth residuals in relation to zooplankton abundance and temperature, and include this in λ_0 (Eq. A4), if it turns out statistical significant. However, we do not expect this to do miracles, since the calanus finmarcicus/helgolandicus codynamics (which is believed to be important) is not represented in ERSEM.

actions: In a revised manuscript, we will extend the paper discussion on this issue, documenting that our choice of model is sensible. If statistical significant, we will include zooplankton abundance and temperature impact in growth (Eq. A4). If not statistical significant, we will replace "POLCOMS-ERSEM" with "POLCOMS" to avoid misunderstandings about the scope of model linking.

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reviewer: *Finally, it is not clear how the density independent growth and mortality in the SLAM model is related to the density dependence in SPAM (page 1443, lines 11-14), From Appendix A and Table 3 (which should really be Table 1, it appears as if larval growth is calculated in both the SLAM and SPAM models? Is this correct? Do they result in the same growth rates? Is tlarv (in Eq. A6) based on the larval duration calculated in SLAM or SPAM?*

authors: We agree that these details of the SLAM/SPAM coupling should be spelled more out

actions: In a revised manuscript we will detail these minor technical issues of the SLAM/SPAM coupling

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reviewer: *The SPAM model itself does not appear to have any additional environmental variability included such as temperature dependence on growth or spawning.*

authors: Conceptually, this acts is through the carrying capacity, see remark above.

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reviewer: *It also isn't clear why the 10km x 10km grid in this setup is needed or infiuences results? The model parameters appear to be constant over the entire domain and do not vary even over the stock assessment areas shown in Figure 1 and used in Table 4.*

authors: The 10 km grid scale allows for spatial emergence even though process parameters are homogeneous at a larger scale. All spatial figures (6,7,8 and 10) display clear gradients between many neighboring cells, so to us it is clear that the 10 km grid scale is needed. The stated point of the paper is also to zoom in on subregional scales, which requires this length scale. Further, the spatial figures indicate that the 10 km grid scale is sufficiently fine, since the spatial variability pattern is not "zig-zag". A spectral analysis will most likely indicate a peak around 20-30 km. In principle, we could let biotic parameters vary independently between all grid cells; however, this will lead to classical over-parameterization due to the limited spatial resolution on available biological data. Here, limited regional scale variability is a sensible choice, as we have done.

actions: In a revised manuscript we will elaborate the motivation for the 10 km grid scale in the SPAM model, as well as the over-parameterization risk by letting biotic parameters vary at a 10km scale.

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reviewer: *Section 2.5 Stock data and data assimilation. This section needs to be split into two sections and both expanded upon. More details of both the assimilation method and the actual data collected by ICES are needed. I am not convinced that ICES stock assessments can be considered as pseudo observations and assimilated independently into the SPAM model.*

authors: We think it is far beyond the scope of this paper to expand on ICES data collation and data analysis procedures. And far beyond the scope of this special issue. ICES results used in this work are well documented in ICES publications. Interested readers should consult provided references and follow references therein. ICES stock assessments constitutes the best available probe on stock biomass and other stock characteristics.

actions: In a revised manuscript we will elaborate the description of data assimilation so our approach is clear to the full audience. Further we will consider alternative data assimilation schemes, which include observational errors (this also covers the reviewer comment about data assimilation in section 2.7) In the revised discussion we will shortly comment about the suitability of ICES stock assessment as pseudo observations including major sources of uncertainty.

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reviewer: *Section 2.6: I don't understand the reference to an operational system on line 24? I as- sume the "lower trophic level" model referred to in this sentence is POLCOMS-ERSEM, but the POLCOMS-ERSEM system that this is based on is not from the operational system. Also, it still appears that the ERSEM variables are not actually used in either the SLAM or SPAM models so all that would be needed is an ensemble run of the POLCOMS system. From the rest of this*

section: what was the "simple statistical ex- trapolation" that was used? And how do the authors know that the ensemble forecast would not lead to improvement in the results (line 6)?

authors:

1. The model used is not the current operational system, but it it is the model configuration identical to the one that was used to produce the MyOcean products for hindcast and physics reanalysis. The same model in reduced confiugration (without the off-shelf area) constitutes the MyOcean v0 configuration, which was the operational configuration in place when MyOcean started until half a year or a year ago. Given the delays and technical issues involved around the v1 configuration it was the only model configuration available in time frames useful for this work and in fact well superior to the operational model at the time (which is why it has been chosen for the hindcast and the reanalysis) and up to date is the only configuration available with substantial multidecadal hindcast experiments.
2. "Simple statistical extrapolation" is explained at the very end of Section 2.6
3. We presumed the ensemble forecast would not lead to improvement in the results, but it was not actually tested.

actions:

1. -
2. In a revised manuscript we refer to the end of Section 2.6 at first occurrence of "statistical extrapolation"
3. We will removed the inaccurate statement (line 6) in a revised manuscript

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reviewer: *In this work, it appears that the observed TSB was just used to replace the model TSB. The same holds true to Table 2 and R.*

authors: No, it is a renormalization - the procedure is explained in Eq. 7.

actions: In a revised manuscript we will elaborate the description of data assimilation, see above.

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reviewer: *The introduction mentioned several events within the hindcast period including a regime shift in 1988-89, how well does this system reproduce those events? It would be very useful to include a time series figure comparing the annual model results with the observations.*

authors: Scrutinizing the 1998-99 regime shift is a very good idea. Currently, there is no clear consensus on whether this is due to overfishing (driver F) or a shift in zooplankton community (driver C) Notice that ERSEM may currently not test the latter hypothesis, since the calanus finmarcicus/helgolandicus codynamics is not represented in ERSEM.

actions: As part of a revised manuscript we will provide a figure showing how our model handles the 1998-99 regime shift This figure will further display the two-year auto correlation of the stock biomass and compare annual model results with the observations (as also asked for).

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reviewer: *Section 3+3.1: The reviewer has miscellaneous minor mainly factual questions:*

1. *Is the forecast system just based on the SPAM model?*
2. *How long is the "reanalysis" period?*
3. *The year ranges in the paper don't seem to match up*
4. *same values of F,M and Z0 are used in all the runs?*
5. *How are the ensembles created? What is varied between them?*
6. *was the purpose of the ensemble runs and how were they used (beyond Figure 4)?*

authors: The short answers are

1. Yes
2. 1983 - scenario start
3. They don't need to match up: 1990-2011 is the period defining historical average conditions
4. Yes, unless said otherwise
5. Cloning of a reference state
6. Resolve effect of climate variability. Ensemble runs were also used for Figs.5+9

actions: In a revised manuscript we will elaborate Section 3+3.1 to address these questions which are just minor technical clarifications

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reviewer: *Finally, the reviewer has a list of minor specific comments:*
The order of the figure references is not quite right. I don't see a reference

for Figure 3 in the paper, and Figure 7 comes before Figure 6. Also, both the reference in the text and caption for Figure 7 mention 3 years – but the figure only has 2 years shown (which also aren't labelled a and b).

Page 1438, Line 20: what are eigen dynamics? This is used again in section 3.4 along with the term "eigen fluctuations" and needs to be explained and referenced.

Page 1439, Line 7: the authors compare forecasting fish stocks to the "ubiquitous weather forecasts" which seems a strange comparison as the timescales and forcing are completely different.

Page 1442, Line 5: I believe that "SPAM" should be "SLAM" in this context as the SLAM model sits between the POLCOMS-ERSEM and SPAM models.

Page 1442, Lines 19-22: need better references for the individual components in the POLCOMS model.

Page 1444, Line 19: It would be better to reorder the tables and have "see Appendix A and Table 1 for model parameterization followed by the results of the model validation shown in the current Tables 1 and 2.

Page 1448, lines 4-6: what is meant by "hindcast" mode and "reanalysis" mode?

Page 1454, Line 6: here the authors refer to T as a "seasonal" matrix but throughout the paper it has been described as an annual transport matrix. I understand that it is only applied during the larval stage so might be considered seasonal but the authors should be consistent throughout.

Page 1454, Lines 18-19: mention of the need for online coupling to include feedback to ERSEM for grazing by the fish, but neither SLAM or SPAM seem to use the model zooplankton to begin with – so a better one-way coupling is needed as a first step. This sentence implies that the one-way coupling is already in place with ERSEM.

authors: -

actions: We will correct these typos/phrasing inaccuracies/elaborate the text at places pointed out to avoid misunderstandings in the audience. All type setting follows the template provided for the Copernicus LaTeX Package.

Interactive comment on Ocean Sci. Discuss., 9, 1437, 2012.