

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

Anonymous Referee #2

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The development of end-to-end models which combine physico-chemical oceanographic descriptors and organisms ranging from microbes to in a single modelling framework is a growing field. The demand for these models arises from the need for quantitative tools for ecosystem-based management, particularly models that can deal with bottom-up and top-down controls that operate simultaneously and vary in time and space. This paper presents a coupled end-to-end system of three components that was used to forecast sandeel stocks in the North Sea. However, the paper is poorly written and has several major problems that need addressing and I cannot recommend it as suitable for publication.

There are several major problems within this paper that need addressing. The model components themselves need clearer descriptions, especially the SLAM and SPAM

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model and how they are connected. Also, the description of the data assimilation system needs to be expanded and clarified. I am not an expert on data assimilation, but I have run models with and without assimilation and am familiar with the basics, but I do not understand the assimilation method used in this paper.

First, exactly how the SLAM model (as described in Section 2.2) is used in connection with POLCOMS-ERSEM is not clear. What is the time-step used in the SLAM model? How is it coupled to POLCOMS? And, how does the larval sandeel behaviour affect their physical transport? It seems unlikely that the larvae are truly passive particles throughout the entire larval stage – they should certainly be able to move vertically and may, therefore, somewhat change their horizontal transport. Finally, how many larvae were seeded at each grid cell? How was this number settled on? Have the authors checked that no significant differences arise from seeding more particles in each cell?

In general, more detail of the SLAM model formulation could be presented here to make this paper easier to follow – including the key parameters and the formulation of the growth model. However, there are still several potential problems with the SLAM model. 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.

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? 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 of survival than the use of a single survival factor throughout the domain.

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 t_{larv} (in Eq. A6) based on the larval duration calculated in

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SLAM or SPAM?

The SPAM model itself does not appear to have any additional environmental variability included such as temperature dependence on growth or spawning. It also isn't clear why the 10km x 10km grid in this setup is needed or influences 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.

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.

Section 2.6: I don't understand the reference to an operational system on line 24? I assume 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 extrapolation" that was used? And how do the authors know that the ensemble forecast would not lead to improvement in the results (line 6)?

Section 2.7: I don't understand the assimilation method used in this paper. Even if you assimilate TSB, you would not expect a cost function of 0 as most assimilation systems take into account the errors in both the observations and the model in their attempt to move the model variable closer to the observations. 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.

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

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be very useful to include a time series figure comparing the annual model results with the observations.

Section 3: Results. Is the forecast system just based on the SPAM model?

How long is the “reanalysis” period? And again, this just seems like it isn’t data assimilation but resetting the model variables to the observed values. The year ranges in the paper don’t seem to match up – on page 1448, line 14 the range is from 1990-2011, whereas the forecasts in Section 3.1 use 1990-2004. I assume the same values of F, M and Z0 are used in all the runs? This needs to be made clearer.

Section 3.1: How are the ensembles created? What is varied between them? What was the purpose of the ensemble runs and how were they used (beyond Figure 4)?

Some 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

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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.

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