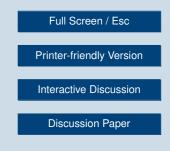**Ocean Science
Discussions**

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 1 finds the objectives of our paper important and highly relevant to ecosystem-based management of marine species. As a general comment to report 1, Referee 1 provides several ideas on how to extend the mechanistic representation of our framework, i.e. include further processes explicitly. However, quoting a leading scientist in the area: " ... it is clear that defaulting to the finest resolution and greatest complexity in all the dimensions (e.g. spatial, temporal, taxonomic, process detail) is not beneficial." (E.A. Fulton, Journal of Marine Systems 81(1–2), 2010, Pages 171–183). Uncertainties in additional processes may lead to degradation for overall model performance, which was also the essence of the meta analysis of Constanza and Sklar (1985) - this point is especially true when moving up

the trophic ladder. Our presented model represent a light-weight, but balanced choice between explicitly represented processes and aggregated processes. The strength of our model is that it aggregate processes in well-defined key drivers, T(larval transport), S(larval survival), C(carrying capacity), F(fishing pressure), M(predation) that can be tested and improved separately, depending on the data available. In this way, we meet users at a realistic level, not requiring extensive and unrealistic amounts of data to set up a similar framework in a different context. Finally, inclusion of many new processes in the framework will greatly increase the manuscript length beyond normal limits. Below we will in detail address the ideas and comments of referee 1. In a revised manuscript, we will emphasize the points above including our our motivation for our choice of process resolution, and acknowledge the potential limitations in the discussion.

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reviewer: *Include direct links with the POLCOM-ERSEM model outputs. In particular there is a need to link the population to the primary (or secondary) production.*

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: *Fishing mortality F is provided from a stock assessment model. If yes, why?*

Fishing mortality needs to be predicted directly by the model from recorded fishing effort, or catch.

authors: We are not sure why thus is important. Catch statistics and recorded fishing effort contains a lot of technical issues (e.g. discard, black landings, bycatch, catchability representation etc.) and we do not want to enter this extensive area in this paper. Rather, we work with F, which is the biological driver, and trust that ICES stock assessment can screen out F from recorded catch and fishing effort. Please also notice that our work output raw catch from fishing mortality F using similar equations relating F and raw catch as stock assessment models.

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reviewer: *Also, the fixed date of spawning (20 Feb) does not permit any realistic phenological changes that can be expected with future climatic variability (section 2.6) or even simply natural interannual to decadal variability.*

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 is 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, based on our previous work.

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reviewer: *I doubt that the fish stocks can be forecasted over a long period with this approach.*

authors: A main point of our paper is climatic variability (via T) confines long term predictability; this predictability is further lowered by other uncertainties in the biological model, when biological uncertainties are not correlated

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reviewer: *But at least the reader would like to see some results to prove the skills of the model to fit actual data (e.g. biomass two-year auto correlation and 1998-99 regime shift or year 2010 was exceptional by the recruitment of 1 year old fish) For instance, in 2010 the proportion of fish just one year old in the catch was more than 90 %.*

authors:
   1. the two-year auto correlation: is clearly visible in Figure 4 (as pointed out in the text)
   2. 1998-99 regime shift: this is a very good idea. Currently, there is no clear consensus on whether this is due to over fishing (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.
   3. "year 2010 was exceptional by the recruitment of 1 year old fish": This is a misunderstanding. Recruitment is always at age 1 for sandeel (but not maturation). High fraction of age-1 sandeel just means a year with small density

regulation or high recruitment potential. Unfortunately, the POLCOMS time series (needed for T) ends in 2004 (where the forcing time series stopped). Therefore we can not test this issue.

actions: We like the idea of addressing the 1998-99 regime shift as part of the model validation suite. 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.

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reviewer: *However, this evaluation against observed fluctuations of the stock(s) needs to be obtained independently of other (stock assessment) model outputs. That is my second main concern.*

authors: In an ideal world it is generally true that validation data should come from independent sources. However such independent data is not available for sandeel - or most other species; first, the observational data on stocks is generally limited and second, stock assessment tries to include all available data, and usually only one authoritative ("best") stock assessment is put forward. So it is difficult to avoid the "incestuous" aspect which concerns the reviewer. There is nothing fundamentally wrong in using same data for parameterization and validation - it just means that validation tests internal consistency of the parameterization, because in our case number of model parameters is far less than number of stock data inputs. This is like giving the R-squared for a fit. Further, if we understand the suggestion correct for overlapping data sets (one for parameterization/assimilation, one for validation), it also implies that one data set is "right" and the other "wrong", which - by construction - will falsify our model ... In our skill assessment, we never use

the same number for both assimilation and validation (apart from last column of table 2+3, which we will remove). It is trivial state that "more data is needed" in the paper. We think it is more constructive to meet users and try to orient modelling against available data sources (e.g. MyOcean and ICES data bases)

actions: In a revised manuscript we will accentuate our philosophy of orienting modelling against available data sources and do the best possible job with this. Further we will remove last column of table 2+3 which are tautologous. In the discussion we will clarify the interdependence of data used for parameterization/assimilation and validation so it is clear that the validation is meaningful and not circular.

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reviewer: *Data assimilation is very succinctly presented in the manuscript. Some more explanations and references would be useful for the reader, including the reason to choose this approach rather than parameter optimization*

authors: Our approach feature both parameter optimization (used for inferring carrying capacity) and data assimilation (for skill assessment and spin-up)

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

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reviewer: *Therefore, this model should be evaluated by producing pseudo-forecasts over a known period*

authors: *-*

actions: *this point is met in a revised manuscript when adding a figure addressing the 1998-99 regime shift as mentioned above*

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reviewer: *In the (short) section devoted to the validation of the model, the authors say that it is based on "fish landings and biological sampling data". But table 1 and 2 give results for biomass and recruitment from the ICES stock assessment.*

authors: This is a misunderstanding caused by our formulations. What we mean is that ICES stock assessment is (mainly) based on fish landings and biological sampling data

actions: This will be clarified in a revised manuscript

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reviewer: *Managers expects to see the model reproducing interannual variability, regime shifts and the exceptional observed high recruitment, with an excellent fit between spatially distributed predicted and observed catch.*

authors: One thing is what managers expect. Another things is the scientific reality. We think it is safe to say that no spatial ecosystem models based on forward integration of mechanistic rate equations today can produce excellent fits to observations. Ecosystem modelling are faced with many fundamental difficulties and limitations. Small steps forward are big steps forward. To bridge this gap, we also advocate flexible data assimilation, as a supplement, in our work.

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reviewer: *Finally, a surprising result is that based on the conventional cost function used "the model performs good in all cases", ie, even without data assimilation ...*

authors: We are not sure we follow the concern of the reviewer. If we have a good model, we have a good model. Then it is an academic question whether the model may be reduced any further without loosing predictive skill. However ICES stock assessment and our previous work (Christensen et al 2008, Can. J. Fish. Aquat. Sci., 65, 1498–1511) show that recruitment is highly variable, and predicting recruitment variability is inevitable for make progress on forecasting this species. Density effects does not iron out recruitment potential variability.

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reviewer: *Sandeels are not really typical pelagic fish species, and extension of this work to other species with whole pelagic life cycle may be not so direct. A movement model would be likely needed.*

authors: The presented framework actually already includes migration. This is detailed in the appendix as a "sensitivity test", along with a figure illustrating the impact

actions: We will add a reference in the main text to the appendix concerning this in a revised manuscript.

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reviewer: *A few minor comments:*
Title, abstract and discussion: Sandeels are not really typical pelagic fish species, and extension of this work to other species with whole pelagic life cycle may be not so direct. A movement model would be likely needed.
Introduction: page 1440. Indeed, there are examples of spatial modeling approaches for large pelagic species like tuna including movement and Maximum Likelihood Esti- mation (cf Lehodey et al 2008, 2010, Senina et al. 2008) used by the Western Central Pacific Fisheries Commission.
Introduction page 1441: some more information about fisheries, available fishing data or independent data collection would be useful.
Technical issues like " the use of Fortran 90 . . . " are of poor interest in such a paper.

authors:

actions: We will follow these suggestions in a revised manuscript

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