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Ocean Science Discussions

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

Anonymous Referee #1

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Comments:

A general consensus has emerged in the last two decades to promote an ecosystem management approach for marine species with the acknowledgment that climate and associated marine environmental variability need to be accounted for in addition to fishing impact when modeling marine species populations dynamics. The international project GLOBEC has for instance generated a large body of literature on this topic. Nevertheless, the management of almost all exploited stocks continues to be based on standard stock assessment models only driven by catch data and in the best cases with some fishing independent data (scientific trawling, acoustics, tagging, larvae sampling. . .). One major reason of this is the lack of modeling tools that can combine both environmental and fishing impacts within a stock assessment framework, ie with rigorous methodology and demonstrated skills to simulate the population dynamics and closely fit all available observations. The work presented in this manuscript can be seen as an effort into this direction and thus is very welcome and should be encouraged.

However, this being said, I have been disappointed by the manuscript and the approach it describes from several points of view and I would recommend a full revision of this work based on the following comments.

First, the effect of environmental variability. It is introduced by the Lagrangian model of larvae drift used to generate annual average matrices of connectivity (note that this model is described in section 2.2, line 5, as an advection –diffusion model which is not the case I think). This is a good start even if other key drivers (predation, starvation) are simply parameterized in the formulation of S (survival rate). It would be interesting however, to have more direct links with the POLCOM-ERSEM model outputs. In particular there is a need to link the population to the primary (or secondary) production. This could be through the carrying capacity (C) that is used here only as a tuning parameter to fit the predicted catch with observation. In addition, if I understand correctly, the 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. This is a key output needed to evaluate the skills of the model.

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. I am not an expert for this fish species, but there is maybe some relationship to environmental variable that could be used to determine the timing and intensity of spawning?

The only link between environmental variability and the population dynamics model (SPAM) is thus this average matrix of potential larval recruitment produced by the SLAM model, with a signal that is then smoothed through density dependence effects

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and the estimation of the carrying capacity (to fit fishing mortality) introduced in the SPAM model. In this latter model there is no more links to environmental variability (e.g., effect of temperature and food on juveniles and adults?). I doubt that the fish stocks can be forecasted over a long period with this approach. But at least the reader would like to see some results to prove the skills of the model to fit actual data. The authors indicate in the introduction that sandeel stocks display strong fluctuations with factor 2 in biomass between years, and that a regime shift also has been observed in 1998-99, or that the year 2010 was exceptional by the recruitment of 1 year old fish. So how the model does reproduce all this variability? This is really the first results, maybe the only ones needed for this paper, that need to be presented before describing how fish stocks can be projected in the future and how to measure the new MSY.

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.

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 (eg with Maximum Likelihood Estimation) usually preferred in fish population dynamics models. Here data assimilation is used as in operational weather or ocean models to correct the state variables at each time step of the simulation from the bias between observation and prediction. If the model is robust, ie with strong theoretical bases (e.g., Navier Stokes) and a good parameterization, the forecast (obviously without data assimilation) should be good. However, if the data assimilation is used with a model based on wrong mechanisms or bad parameterization, the biases could be partially corrected by data assimilation but the forecast should quickly drift from the realistic trajectory. Therefore, this model should be evaluated by producing pseudo-forecasts over a known period, ie running the model with data assimilation until a given date and stop data assimilation to see the forecasted trajectory over a known period. But this should be not realized with

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pseudo observations from another model (the ICES stock assessment model) to which final results are compared. It seems a strange "incestuous" approach. Eventually the initial conditions of the model can be extracted from the stock assessment, but then it should run without input from it and their results compared.

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. These fish landings and biological sampling need to be detailed and shown. To propose a new model and persuade fisheries scientists and managers that it can be used in a management framework, the conventional cost function is not sufficient (especially of course when the properties used are model predictions of the current stock assessment. . .). As said above, the manager 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, and preferably catch per unit of effort (by size/age).

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. Why are these two model outputs so similar? Even when data assimilation is not used?

The optimistic hypothesis is that both the regional stock assessment and this new approach introducing some detailed spatial larval processes linked to the environment are good enough to capture the real dynamics of the stocks. The pessimistic is that both modeling approach are finally very close with similar population structure after the larval phase, same growth, fecundity, mortality, and are mainly driven by catch (since F is provided by stock assessment model and C (and M?) are tuned to fit the observed catch), while larvae recruitment finally would bring limited information?

Whatever the answer, it needs to be investigated in more details, and the real skills of the model demonstrated.

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 Estimation (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.

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