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Biogeosciences Discussions

Interactive comment on "Oceanic controls on the primary production of the northwest European continental shelf under recent past and potential future conditions" *by* J. Holt et al.

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We welcome the positive response to this paper from the referees. They suggest several ways in which we could improve it by providing additional material, which we largely agree with. We aim to accommodate these suggestions, accepting some limitations on time, resource and the need to keep the paper focused and of an appropriate length.

Additional analysis

Referee 1: A detailed analysis...; ...what about a decrease...; ...focus on the first of these drivers...; ...like to see a better balanced budget...; provide some estimate to strengthen hypothesis...; Referee K W.-A.: Little discussion of benthic component...

The suggestions to provide more information on the internal cycling, the other drivers of climate change, to quantify the effects of increasing temperature and stratification, and to develop the budget section, would certainly improve the paper. Moreover, the referee is correct that the benthic component has been slighted here. We aim to address these issues in two ways. First we aim to expand eqn 1 to describe pelagic inorganic and organic nitrogen, and benthic nitrogen budgets. This provides a closed budget of the complete N system. Mean values of these terms will then be included in table 2. This we will split into 2 on-shelf regions: The northern regions (2, 3, 7, 8, as labelled on fig 1) and the southern/western regions (1,4,9,10). We will also add experiment A1Bb to this table. It is not practical to include and discuss results from all 10 on-shelf regions. We feel dividing the results in two is an appropriate level of detail, as it divides the regions between those where ocean-shelf transport is more important, showing a decrease in netPP, and those where it is less important and the netPP increases. For example in the latter case, the importance of changes in the benthic system is now clearly shown.

Second, we aim to add a new table showing a range of metrics and their fractional change between A1B and CNTRL. These are:

•Winter DIN (as in fig 7);

•Excess production: annual N uptake minus winter DIN (quantifying the results in fig 7);

•Recycled pelagic fraction: mean annual N remineralisation dived by mean total pelagic nitrogen (TPN);

•Recycled benthic fraction: mean annual benthic DIN efflux dived by mean total benthic nitrogen;

•Diffused fraction: mean summer (JJAS) diffusive N flux across the 10% light level, divided by TPN;

•Phytoplankton growing Season: the length of the period when 15 day mean netPP is greater than 0.1gCday-1

These will be shown for all 13 regions and allow us to show (in summary) how the changes we focus on in this paper (ocean-shelf exchange) compare with those from other drivers.

In addition we will expand the discussion on temperature and the potential for stratification to decrease under future conditions and what effects this might bring. But we also note that the non-linearity in the equation of state means that warming conditions will generally favour increasing density stratification even if temperature stratification is unchanged.

Validating ocean-shelf exchange

Referee K. W.-A.:...no quantitative comparison [of cross shelf circulation] is given...

The question of validation of the ocean–shelf exchange circulation is of great interest, and a way we can again improve the paper. We have recently started a 4 year programme to investigate this issue with a series of targeted observations and model simulations (see www.smi.ac.uk/fastnet). For the present work, the salinity observations provide a useful guide to the general uncertainty using the 'LOICZ methodology' of considering the fluxes into and out of a well mixed sea region. While the validity of this assumption is open to some question, it is adequate for an approximate bias estimation. The mean on-shelf salinity is given by

$S= (So.Q+Fs)/(Q+Qr)$

Where $S=S(So,Q,Qr,Fs)$ is a function of 4 quasi-independent variables: oceanic salinity, ocean-shelf flux, river flux and atmospheric input respectively. Hence perturbations in S can be written:

$\delta S= \delta So.dS/dSo+ \delta Q.dS/dQ+$ *etc.*

This gives:

$\delta Q/Q= \delta S/(So -S)- \delta So/(So-S).S/So,$

for the case $Q \gg Qr$ and $SoQ \gg Fs$.

Using the approach described in section 2.2 for the whole shelf and region 12 (southern section of NE Atlantic) to obtain mean errors, this gives a mean bias of -18.6% for the ocean-shelf exchange. This analysis is applicable on time scale over which the shelf can be considered well mixed – i.e. decades. Hence, it is appropriate to estimate a mean bias rather than provide information on errors in variability. This analysis will be included in section 2, and in the discussion where the results can be compared with Huthnance et al (2009, Ocean Sci.). For example the exchange considered here (of 2.6Sv, adding inflow and out flow separately) corresponds to 1.3m2s-1 of exchange per length of the slope. This falls within the range estimate for slope current (Ekmann drain) of 0.5-2 m2s-1 but is a significant underestimate (by a factor of 2) when other (more variable) process are added together; the salinity comparison suggest these are not necessarily additive (as is also noted in that paper).

Single v's multi forcing data sets

Referee 1: To use only one prediction of climate change is not a very robust approach....; this should already be reflected in the title...yes it is rather a sensitivity study...Please add a paragraph discussing

We agree with the reviewer that using a single forcing data set is not the most robust approach to addressing the downscaling of future climate simulations. As we state, we are concerned with the system's response to changes in external conditions in the context of potential future change. Here we use a single, self consistent forcing set that is physically plausible but cannot comment on 'likelihood' of the details. However, the type of future climate signal seen here is representative of other models (i.e. a warming temperature and an increase in oceanic stratification), although the magnitude of the change and other factors such as wind speed and precipitation will be much less certain. Accepting this shortcoming, we feel this experiment design is still very informative, and this is borne out by the reviewer comments. We are currently working on an ensemble approach (with 40-50 members exploring atmospheric forcing and parameter uncertainty see http://www.equip.leeds.ac.uk/research/marine-ecosystems), but this will not be ready for the present paper. Hence here we propose to modify the title to clarify the scope of this work and include a discussion on the issues arising from only using a single forcing set compared with multiple forcing sets.

Baltic boundary conditions

Referee 1: There are also periods with net inflow into the Baltic...

We agree there will be some impact of the Baltic boundary conditions on the results presented here; this is noted in section 2, as a possible cause of the positive salinity bias seen in table 1 for the Norwegian Trench, Skagerrak and Kattegat. However, the circulation pattern means these effects will be limited to the Norwegian Trench and to a lesser extend the eastern side of the northern North Sea, so are unlike to have a large bearing on the results we present.

Clarification of validation procedure

Referee 1: I have problems with the validation...; Referee K. W.-A.: is the model representing the benthic realistically...

As requested we aim to clarify the model validation procedure. All the data available for each month are averaged onto the model grid to give 12, 3D monthly climatological mean fields for each variable. Corresponding mean annual cycles are calculated from the model results. For each month these fields are differenced to give 12 3D fields of model minus observation. These difference fields are then used to calculate the statistics shown in table 1. The bias is the average value across the region and for all months. The cost function is based on the RMS error of these and the standard deviation of the monthly mean observed field in each region (i.e. without removing the seasonal cycle).

The benthic component of the model was validated during the original development of ERSEM (Blackford 1997 J Sea Res; Ruardij et al 1995 N J Sea Res). Lack of data means we are unable to go further at this stage. In the UK NERC is planning a new shelf seas biogeochemistry programme, which hopefully will address this http://www.nerc.ac.uk/research/programmes/shelfsea/events/documents/shelf-sea-biogeochemistry-ao.pdf

Amalgamation of model data

Referee K. W.-A.: Can the authors comment on whether the regional, depth and temporal amalgamation of model output is a good way to summarise results.... I am bemused by Shutlers et al 2011 finding....; ...in the context of the regional spatial...

There are two issues regarding the need to amalgamate model information. The first is reliability of information and the second practical considerations of distilling complex 5D fields into results that can be effectively interpreted. A more detailed discussion on the issue can be found at: http://www.meece.eu/documents/deliverables/WP3/D3.2.pdf

The issue of reliability arises because several grid cells are required to resolve any feature (say 10 cells), so results are generally more reliable when taken over a larger number of cells, if the small scale features being averaged-out are less well modelled. For example, the difference between one cell and its neighbour is highly dependent on the solver used and so there is little useful information at the grid scale. Similarly poorly resolved small scale features can give misleading results. This is not to say that the model has too fine a resolution, just that we should not pay too much attention to the details at the fine scale. The analysis in Shutler et al (2011; JMS) is not directly comparable to the case of spatial averaging, but is still informative as it is a quantitative assessment of the relation between model skill and scale. In that case wavelet analysis is used and a clear improvement of skill is seen at scales of several grid cells when compared with ocean colour measurements of chlorophyll. However, care is needed because important fine scale/high frequency information can be lost with aggregation.

The report cited above shows the example of temporally averaging the spring bloom, and similarly in the results presented here the small positive change in nutrient uptake in region 10 (figure 7) hides both positive and negative values in netPP change seen in figure 4. While some of the differences in fig 7 are small, 8 of the 13 regions show a statistically significant difference (indicated by bold figures in table 1 and on the new table described above).

Generally we feel amalgamation into large regions is appropriate for this type of shelf-scale integrative study. Naturally a more process oriented study would be able to give more detail in a smaller region. We are reluctant to present 1 or 2 regions in more detail here simply because it gives excessive prominence to the finer space/time scale detail than is the focus here. The data from the reanalysis forced simulation is already available for the interested reader to explore themselves at monthly frequency (see footnote 2). We aim to make the remaining simulations and some higher frequency data available via www.bodc.ac.uk in due course.

Model description

Referee K. W.-A.: scant detail is provided in the current publication

To address the issue of the model description, we will expand section 2 to provide a more comprehensive description of the model configuration and forcing, and how it differs from the primary model references. There are some references to not yet available material, and some omissions, for example, here we do include tidal but not wave re-suspension to couple benthic and pelagic models, which will be addressed. Because the changes from the primary references are not are not so many, we do not feel an appendix is necessary. The configuration files will be published alongside the model data at www.bodc.ac.uk, and the POLCOMS model code is now available under a GRL license.

Seasonality of inflow

Referee K. W.-A.: It would be interesting to quantity the winter ingress of surface waters...

Yes the main influx of nutrient is seasonal – occurring in winter as the inflow waters are depleted of nutrients during the summer. i.e. DIN Flx in in table 2 is predominantly in winter. A comment to this effect will be added.

Other points

Referee 1: ...only the atmospheric forcing are used...

In addition to 6hrly atmospheric data the model is also forced by ocean boundary conditions: tides, residual current, T and S (5 daily), and monthly climatological nutrient data is used for oceanic boundary conditions for the ERSEM model. This will be clarified as noted above.

Referee 1: It is perhaps unbalanced...

Agree, will reword

Referee 1:The dependency....is also discussed by...

Reference to Kauker and von Storch (2000) and Hjollo et al (2009) is duly noted and will be added to discussion.

Referee K. W.-A.: How deep are....

A range of depth levels will be added for section 2.2: it ranges from 2m in 10m water depth to 37m in 4000m water depth

Referee K. W.-A.: Primary production is depth integrated...

The units in table 1 netPP should be gCm-2.

Referee K. W.-A.: Is the bias correction....

Yes the bias correct is applied to all IPSL forced runs – this will be clarified.

Referee K. W.-A.:Annual netPP in Celtic Sea....

A range of in-situ PP values is not available for the Celtic sea. We may be able to add a value for the English Channel for the L4 station.

Referee K. W.-A.: Is SeaWiFs comparison...

The SeaWifs comparison comment relates to both ERA40 and CNTRL.

Referee K. W.-A.: is simulation A1B capturing on/slop circulation....

In fig 9, only the difference between A1B and CNTRL is shown, but the caption is in error, so yes A1B has similar on-off shelf circulation.

Referee K. W.-A.: Ecosystem boundary conditions use...

We don't find a issue associated with mis-match between nutricline and thermocline in the nutrient boundary conditions in this case. This may be because we are using an 'upwind-advective' rather than a 'clamped value' boundary condition. However, we do find it necessary to set the inflowing POM values to zero, rather than zero-gradient, to prevent instability.

Referee K. W.-A.: Please state what the relation...

The pertinent relationship in figure 7 is increasing winter DIN is followed by increasing DIN uptake

Referee K. W.-A.:...do you mean table 2...

Yes – Ns should refer to table 2 and not table 1.