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Biogeosciences Discussions

Interactive comment on "NW European shelf under climate warming: implications for open ocean – shelf exchange, primary production, and carbon absorption" *by* M. Gröger et al.

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J.Holt

This is an interesting and important approach to downscaling climate change to a regional scale, one which others working in this area should take note of, particularly the removal of the need for open boundary conditions and use of a full transient simulation rather than a time slice (as is common in many studies). Both relate to the adjustment time to oceanic conditions, which can be many years on these shelves. There are, however, several issues related to the conclusions that the authors should consider, which I hope will be taken in the spirit of constructive criticism.

Authors

We appreciate the interest in this study and thank for the significant and constructive comments from which a revised version will clearly benefit.

J.Holt

The general observation that the European shelf receives much of its nutrients from the NE Atlantic is well known (e.g. Hydes et al., 2004) and we found a similar effect to that seen here in our model simulations (Holt et al., 2012). These also use an A1B scenario, but with a time slice approach in an ocean-margin model. They show a 20% reduction to N inflow on shelf, but only a 5% reduction in primary production across the shelf. This raises two important questions. The on shelf nutrients are sensitive to deep winter mixing in the North east Atlantic, and this is likely to be very sensitive to both changes in atmospheric forcing (e.g. from different atmospheric models) and the mixing scheme in the ocean model. These issues of structural uncertainty are crucial to the wider interpretation of this work.

Authors

We agree that deep mixing in winter is most important in this issue as consumption of nutrients by phytoplankton growths is limited by light so that nutrients can accumulate. Of course the atmospheric forcing is important in this respect as well because to a certain degree it influences mixing. A drawback of our expensive model is that we only could use one atmospheric model and one warming scenario. However, the increasing stratification in the mid to high latitudes in the course of the 21st century is seen in many models (see also our reply to referee 1). We will give a more comprehensive overview the these structural uncertainties in a revised version.

J.Holt

This aside, it would be interesting to know how the deep winter mixing and surface nutrients change over a wider area than shown in figure 6?, bearing in mind the general pole-ward flow here and also that some water comes on-shelf further south than is shown.

Authors

This is an important point. In the next version, we will show that winter deep mixing weakens for a very broad region of the NE Atlantic.

J.Holt

I'm not sure I agree with the description of this process as a 'stratification feedback' and the explanation for there being a stronger effect on–shelf than in the open ocean needs further exploring or reconsidering. Local stratification effects should be marginal in a sea that is only seasonally stratified, since they will only effect summer mid water production rather than seasonal nutrient resupply.

Authors

There are two stratification effects: 1)increased stratification INSIDE the North Sea during summer. 2) stratification during winter combined with a thinning of the mixed layer OUTSIDE the North Sea along the shelf edge and continental slope. This reduces lateral nutrient advection into the North Sea thereby declining its total nutrient inventory. The resulting relative decline of production in the North Sea is larger than the relative production decline averaged over the North Atlantic. We will make this more clear in a revised version.

J. Holt

It's good to see the on-shelf mixed layer depths are unchanged in figure 6. Changes to the duration of the stratified period are likely to be a positive effect on PP. Again it would be good to see the change in PP over a wider area. Is it anywhere positive?

Authors

The thermal stratification, i.e intensity and duration of the thermocline throughout the

year shows no significant changes in our scenario simulations in the North Sea. Along the continental slope and in the North Atlantic the pycnocline becomes more intense at a depth around 700m which weakens upward mixing of nutrients in winter. The change in the nearby NE Atlantic is responsible for the profound productivity changes in the North Sea. As suggested we will show the changes in the MLD, PP and nutrient concentration for a much broader area in a revised manuscript which will clarify this. Changes in PP are nowhere positive except for those regions that are affected by a retreat of sea ice.

J.Holt

This brings me to my second point. In our studies we found several processes that acted to mitigate against the effects of reduced oceanic input (hence only a small reduction in overall pp). These included changes to light field, temperature effect on nutrient recycling, wind, length of growing season etc. I'm wondering how well some of these are accounted for in an ecosystem model designed for global biogeochemical cycling? If these local mitigating effects are not included here, then is this work in fact putting an upper bound on the oceanic control? Again these issues of structural uncertainty are important for the interpretation of this work.

Authors:

The availability of photosynthetic active light increases slightly in the North Sea (by <5%). Only in the southern North Sea, where river inputs were reduced light intensity increases by up to 15% (less self shading by phytoplankton). Our carbon cycle model HAMOCC has no temperature dependency of remineralization which would of course stimulate PP again by accelerating nutrient recycling in a warming climate. However, the temperature effect on growth rates is included in HAMOCC which feeds back positively on growing season and on photosynthesis. This may indicate that the strong mitigation effects found in Holt et al., 2012 could primarily be a consequence of the temperature effect on remineralization which is not included in our model. The length of the growing season is prognostically calculated from the aforementioned processes. We will include these topics in a new discussion section (also requested by referee 2) to be introduced in the next version.

J.Holt

On the shelf carbon pump question – I agree the transport of DIC to deep water is crucial. Interestingly our experiments with tracer transport (Holt et al., 2009) and ecosystem models (Wakelin et al., 2012) also suggests the pump is 20% efficient, although the details differ and we lack the benefit of the larger area considered here. Some consideration of the hydrography around the Norwegian coast would be interesting. There's permanent salinity stratification in the Norwegian Trench, does deep winter mixing occur near the Norwegian coast in the Norwegian sea, if so this may answer a question we were unable to in Holt (2009) due to our limited area model. This then points to the shelf edge region as being much more important than the North Sea for the shelf sea carbon pump.

Authors

Indeed we see this is an important point in our simulation. In the southern part of the Norwegian coast the water column is stratified. But when the water leaves the North Sea winter mixing occurs up to several hundred meters on the slope. Hence, it appears that the shelf edge region and the neighboring NE Atlantic controls the shelf pump two fold: by controlling the nutrient and DIC transport onto the shelf and 2) by the fact that carbon loaded waters of the North Sea are injected well above the pycnocline in the open ocean.

J. Holt

A final (minor) point, the discussion of internal tides in the introduction is possibly misplaced, these are unlikely to be resolved at 10km; the relevant ocean –shelf exchange processes are more likely to be wind and slope current driven Ekman transport.

Authors

Internal tides can be at least recognized in the model. They are strongest along the northern Biscaya shelf (see attached Figure 1 in the supplementory material). We don't know, however, whether the modelled amplitudes are realistic given the coarse spatial resolution of the model. In the North Sea such oscillations rarely exceed a few meters. To assess whether these oscillation take notable influence on vertical tracer fluxes would require a more thorough analysis which goes beyond the purpose of this study. Thus in the next version we will not mention the process of internal tides as they are probably not sufficiently resolved by the model.

Again I hope these comment will be taken in the constructive sprit that is intended: this is an impressive effort.

Jason Holt, National Oceanography Centre, Liverpool, UK

Interactive comment on Biogeosciences Discuss., 9, 16625, 2012.

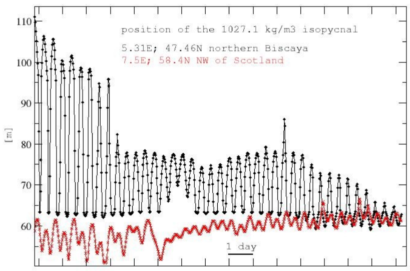

Figure 1: Vertical movement of the 1027.1 kg/m3 isopycnal at two stations along the NW European shelf. The timeseries were extracted from experiment CWE and covers the period from the 16th to 31st of August in 1995.

Fig. 1.