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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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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. 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. 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. 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. 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? 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.

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 $\sim$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.

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.

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

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