Dear Scott C. Doney

Thanks again for this positive and helpful review of our manuscript. Please find below a reply to the issues you raised and the way we will address them in the revised version of the manuscript.

Sincerely,

Damien Couespel, Marina Lévy and Laurent Bopp

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One issue that would be good to address in a little more detail is the difference between the response of new and regenerated production (around Line 210). In the chosen model, ~2/3rds of the NPP decline is due to new production that is directly linked to nitrate supply; previous model studies have indicated substantial variations across models in the temperature sensitivity of NPP under climate warming scenarios that can reflect direct phytoplankton physiological effects as well as changes in the efficiency of nutrient recycling and export (e.g., Laufkötter et al., 2015, doi: 10.5194/bg-12-6955-2015; Laufkötter et al. 2016, Biogeosciences, doi:10.5194/bg-13-4023-2016). It would be useful to know the temperature sensitivity of some of the biological terms in the model, for example. It would also be useful to present briefly some results on the baseline f-ratio in the control simulations and change in f-ratio across the climate change scenario.

> In our simulations, none of the biogeochemical / biological processes depend on temperature. Thus, the direct effects of warming on phytoplankton physiology are not considered. We agree with the reviewer that this point is a caveat of our study as direct warming effects are key drivers affecting future NPP changes and a large source of uncertainty in explaining contrasted model responses (Laufkötter et al., 2015, Olonschek et al. 2013). Our bias here is to use a very simplistic biogeochemical component, in which all biological terms are independent of temperature. We will emphasize this point in the methods section and in the conclusion (somewhere between lines 337 and 351).

As suggested, we will also compute and discuss the f-ratio and its evolution across the climate change scenario.

The Results section continues with an analysis of the physical transport differences in the climate response across resolution, linking back to nutrient supply. The issue of changes in circulation is an important aspect of the results. In the discussion of the meridional overturning circulation (MOC) (around line 275) it would be good to clarify the differences in the MOC in this simplified geometry model versus more realistic simulations of the North Atlantic. While the simplified geometry model does include some deep convection at the northern boundary, the overturning circulation is shallow (<1000 m) and weak (only a few Sv in control simulation). Also, in full ESMs, the North Atlantic deep water formation rate and MOC are affected by freshwater export from the Arctic, a process not captured in the simplified model. It would be good to clarify what can be done with the simplified model versus those processes that would require investigation in a more detailed model. A more minor point is that the experiments appear to assume that the seasonal wind stress patterns are constant under climate change, a topic perhaps worth noting in the discussion.

> We agree that a deeper discussion on the MOC mean-state and projected changes between this simplified geometry and more realistic simulations would be of interest. Roughly, we see 3 major differences that will need to be better discussed in the revised manuscript: 1) closed boundaries (and no topography), 2) no changes in wind stress, 3) no changes in freshwater input.

- Closed boundaries do not allow the inflow of water masses from outside the domain as done in more realistic ocean and climate models:

    - Closed boundaries in the north and in the east prevent any input of water masses from the Arctic and the Nordic Seas. This results in a much simplified temperature, salinity and density vertical profiles (our model only have one homogeneous water mass below 800 metres). In particular Nordic Seas overflows are important for a better representation of the MOC (Zhang et al. 2019), which might be a starting point for explaining the shallower MOC in our simulations.

    - As mentioned by the reviewer, closed boundaries also prevent any input of fresher Arctic waters. The overturning circulation may be more sensitive to changes in these freshwater inputs than changes in precipitations, run-off or ice melting in the considered domain (Bras et al. 2021).

    - Closed boundaries and the simplified geometry may also prevent any latitudinal shift of the Atlantic Meridional Overturning Circulation source regions (Lique et al. 2018).

    - Closed boundary at the southern border of the domain also prevent the simulations from representing all the effects coming from the Southern Ocean.

- As noted, the seasonal-varying wind stress patterns are held constant under the idealized climate change simulation. Shifts in the wind stress patterns are however important consequences of global warming resulting in a poleward shift of the oligotrophic gyres (Polovina et al. 2008, Yang et al. 2020). However, in the North Atlantic ocean, the impact is weak when compared to temperature changes which are though to be the main drivers of MOC slowdown (Saenko et al. 2005, Gregory et al., 2005; Weaver et al., 2007; Marshall et al., 2015).

As a conclusion, we will add in the discussion section that this simplified model allows to investigate the resolution sensitivity of a warming-induced AMOC decline related with the reduction of the formation of a unique deep water mass - although the link between the two may be more tenuous than previously thought (Lozier et al. 2012). The AMOC response driven by freshwater input and wind stress pattern changes or related to changes in other oceanic regions and water masses would require more realistic configurations.

Specific issues in text.

Line 177
"u cot N ds"
The "cot" probably used be the command "\cdot" in Latex.

> Right, this will be rectified

Line 190
In Equation 4, the second N_CC probably should be N_CTL

> Right, this will be corrected

Line 298
"eddy parameterization coefficients (kredi and kgm)."

I think there is a formatting issue here with the subscript. Also, would be good to relate back to terms such as "isopycnal" and "bolus" diffusivity that may be more understandable to the reader rather than model coefficient names, since the specific GM parameterization equations were note presented.

> Indeed there is a formatting issue, thanks for noting it. As suggested we will used the subscript iso and bol that are more reader friendly.

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