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Interactive comment on "Climate change impacts on net primary production (NPP) and export production (EP) regulated by increasing stratification and phytoplankton community structure in CMIP5 models" *by* W. Fu et al.

Anonymous Referee #2

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The paper compares 9 CMIP5 ESM in terms of NPP, EP, surface nutrients, and stratification. There is some very detailed comparison of these various fields.

Several recent studies have done similar comparisons (e.g. Bopp et al 2013, Cabre et al 2013) and there have been numerous studies that have considered projected NPP, EP and nutrient changes as function of warming and stratification (e.g. Bopp et al 2001, Bopp et al 2005, Dutkiewicz et al 2013, Marinov et al., 2010; Taucher and Oschlies et al 2012), and several that have also considered changes in phytoplankton community structure (e.g. Bopp et al 2005, Dutkiewicz et al, 2013, Cabre et al, 2015). Thus almost

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everything this manuscript addresses has been discussed before. This manuscript has some more specific numbers for the variability between models (though similar comparison also have noted this variability though the numbers are a bit different depending which models they included). I struggled therefore the find something new (and useful) in this manuscript. The pieces I did find were:

1) models with largest increases in stratification have strongest changes in NPP and EP (Bopp et al, 2013 had something similar but using changes in SST rather than stratification).

2) models with largest increases in stratification also showed the largest biases for the contemporary period (suggesting potential overestimating climate impacts).

3) Models with dynamic phytoplankton communities show larger decline in EP than NPP (but this could be anticipated any of the previous work that has suggested shifts from large to small phytoplankton and if they parameterize large as having larger impact on export).

The second point is potentially exciting. A careful analysis of the differences in stratification helped identify this. I recommend rewriting a significantly shortened paper which highlights this aspect over a long-winded summary of the detailed comparisons. For instance the current discussion makes no mention of the point on this stratification/bias issue, but includes a long list of numbers (not particularly useful as it depends on which set of models one looks at – see e.g. Bopp et al 2013, Cabre et al 2015) and focuses on things that have already been addressed elsewhere in the literature (NPP varies more than EP – Bopp et al 2013; shifts in community structure – Bopp et al 2005 (and many others)). A long discussion about how CMIP5 models are far from perfect seems irrelevant in the face that several other studies have said similar things.

I suggest shorting to less figures and removal of details that can be referenced to other studies. Details of the nutrient changes I found somewhat less interesting – nutrient supply rate changes are what is important. I was convinced that there was useful

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information that came from this part of the analysis. This also applies to Fig 3.

Besides a much shorter paper, I suggest much greater care on the discussion which I found to delve into speculation and grandiose statement that I do not feel are supported by (or even relevant to) the paper. For instance:

Pg 12870: lines 10-20. I find this discussion potentially dangerous. I will agree that changes in EP is a better metric for climate impacts on carbon cycle; but disagree that it is best metric for "marine ecosystems" or food chains and fisheries. Community structure changes are also very important for marine systems and can potentially not be captured in EP. Additionally EP is possibly worse in parameterization than NPP in models. Before arguing this too fully it would be worth looking at how each of the models determines EP (Martin curve, explicit particle sinking) and how they parameterize how much is exported relative to community structure.

Could models have more similar changes in EP because they are all so crude in how they parameterize EP?

Since the models are so crude in parameterizing the complex processes involved in EP (role of bacteria, Archea etc etc): should EP be sold as a "best" metric for any impact of climate change? This goes back to my point (3) above.

On a final note to the community at large: How much more useful (as opposed to "details" on models we know are flawed) information can be wrung out of CMIP5 comparisons?

Interactive comment on Biogeosciences Discuss., 12, 12851, 2015.