

## ***Interactive comment on “Drivers and uncertainties of future global marine primary production in marine ecosystem models” by C. Laufkötter et al.***

### **Anonymous Referee #1**

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**General Comments** This manuscript examines between-model differences in ocean NPP in nine CMIP-5 coupled carbon climate models for the IPCC high emission scenario RCP8.5. To be included in the study models had to have at least 2 phytoplankton and 1 zooplankton Plankton Functional Types (PFTs). Most previous publications emphasize the multi-model mean outcomes and do not explicitly examine the differences between models and their causes. While the objective is highly worthwhile and the results are for the most part informative, the current version of the paper is too long and repetitive, and the key results are difficult to pull out from the mass of detail. Reading the paper is made difficult because in some cases the numbers in the discussion do not match the numbers in the tables or figures, and some tables are discussed in a different order to how they are numbered. Unfortunately only 2 ecosystem models are coupled to the same physical model, making it difficult to separate out effects caused

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by physical processes from those caused by ecosystem/biological processes.

Specific Comments 1. Tables 7 and 8 are referred to before Table 6, and the discussion of Table 8 preceeds that of Table 7. They should all be renumbered accordingly. In addition the numbers quoted in the text often do not agree with those in Tables 7 and 8. For example, on p. 3744:

Line Text Table/Column Table # 18 0.61 0.62 /Correl. 8 19 0.86 0.19 /NStdDev 8 20 -0.69 -0.51 /Bias 8 23 0.5 – 0.75 0.45 – 0.76 /Correl. SiO3 8 25 0.5 – 0.72 0.36 – 0.72 /Correl. Chl 7 6 / 3745 0.17 – 0.69 0.09 – 0.69 /Correl. NPP 7 7 -8.8 – 6.8 -8.9 – 6.8 /Bias NPP 7

2. The idea of ‘Bottom-up vs top-down’ processes was carefully presented on p. 3746-47, but discussions of Figs. 9 and 12 are confusing and do not discriminate between ‘Bottom-up’ (growth rates) and ‘top-down’ (changes in phytoplankton biomass). I think there is too much info on these 2 figures.

3. Eqn (6) on p.3753 is confusing. The authors do not clearly define the numerator term ‘P-dependence’

4. Sections 6.1 and 6.2 – In an effort to make the paper more concise and have less unnecessary detail, I think references to results from ‘older’ (i.e. pre-2010) papers should be minimized. Key points on the current work seem to be getting lost in the details of other references.

5. Sections 7, 8 and 9 seem to repeat certain findings. It would make the paper more readable if these sections were reduced substantially. I think 8 and 9 could be combined into a single section on Conclusions.

6. The analysis here is made more difficult because most of the biogeochemical models are coupled to different climate models. Yet the paper by Kwiatkowski et al. (2014) is only referred to as “... a more solid comparison between differences ...”. It seems to me that a summary of their main findings, relative to this paper, would be a valuable

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addition.

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Interactive comment on Biogeosciences Discuss., 12, 3731, 2015.

**BGD**

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