

## ***Interactive comment on “The impact of global warming on seasonality of ocean primary production” by S. Henson et al.***

**Anonymous Referee #2**

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### **1 General comment**

My comments are mostly editorial as I believe the paper is well written and provides a general overview of the possibility to extract signals of changes in phenological features from the current set of CMIP5 simulations. I think the authors did an exhaustive analysis of the available data and drew out the only possible conclusion from them, which is that we cannot detect any robust change in bloom features with monthly output data. I appreciated very much the final discussion and I think the paper should be

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accepted with just minor changes that I would like the authors to consider (see section below) but I think this information on the time scale of analyzed data should be given earlier in the manuscript.

For instance Hashioka et al. (2009) and Racault et al. (2012) found that expected changes in model data and Earth observations are of the order of days or couple of weeks, and it may be unlikely to identify this kind of time scales in the standard output files of CMIP5. On the other hand, it is also important to make clear that the solution is not to store model outputs at daily frequency, because all participants to CMIP5 know well how expensive in terms of resources it is to store and post-process data at this resolution. Phenology and/or other indicators of plankton status such as biomass stoichiometric ratios (e.g. Patara et al., 2012) do come at higher computational and storage cost and a careful design of the output that is required should be addressed when designing the next set of experiments for climate change studies.

Therefore, I would suggest the authors to be more perspicuous also starting from the abstract, because the message they seem to be giving is that studying phenology is by no means an indicator capable of capturing changes in the global plankton ecosystem. I think this kind of consideration should be made explicit for instance in the Method section, where the quality of the data used for the analysis is initially assessed. Some recommendations on the set of indicators that CMIP5 modellers could put in their output should be also included, as a complementary information to the (unfortunately always necessary) plea for maintaining long-term Earth Observation instruments.

### **2 Specific comments**

**P1425\_L16-19** Since this work is an update of the study done in Henson et al. (2010, see also the comment below for page 1432), the readers should be informed of some of the major difference in RCP8.5 with respect to previous IPCC scenarios.

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**P1426\_L3-6** The VGPM is a model that uses a combination of satellite observations and general circulation model output. This should be made clear that such a comparison is a model-to-model comparison, because it has been demonstrated that the skill of satellite models is rather equivalent to the one of global biogeochemical models when assessed against in situ primary production data (e.g. Friedrichs et al., 2009; Vichi and Masina, 2009; Saba et al., 2010).

**P1427\_L14** I disagree that VGPM estimates can be called "observations". The error of the VGPM model should be considered when assessing the quality of the CMIP5 ESMs in the Taylor diagram.

**P1427\_L17** Figure S2 is not informative and should be plotted again with a discrete colorbar. It is not possible to identify the months with a continuous color labelling.

**Fig\_2** Fig. 2 is very difficult to read and it is not of much complement to the description. The plots are very small and the use of a continuous colorbar (which is saturated in many cases) make the interpretation of the differences between the models almost impossible. It should be considered to separate the three variables in three different figures. The authors did a great job in describing the major features, though I would suggest them to make this description a bit more organized by introducing sub-sections with the major basins (North Atlantic, Southern Ocean, etc). I wonder if the authors can include a statistical significance of the trend, which would help the interpretation of the figure by introducing areas with no color in correspondence with the lack of significance. As it stands now, the choice of the white color is rather arbitrary. The manuscript would be very much improved if this significance is introduced and for instance the colorbar is discretised with no more than 6-8 colors. Another option that may be considered is to integrate all information in a map showing the number of models with positive/negative trends. This map could be produced with a coarser resolution and would be much more informative for the readers.

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**P1429\_L4-16** Three models out of 6 show a possible increase in the equatorial Pacific (or surrounding flanks), and in some cases an increase in amplitude of PP. This feature is not commented, while it has been previously studied (Steinacher et al., 2010; Vichi et al., 2011) and it is consistent with the projected trends in the transport of the Equatorial UnderCurrent (Sen Gupta et al., 2012).

**P1431\_L18-25** The increase in dissolved iron is an interesting feature that is not completely clear to me. I suspect that this kind of effect may just be a consequence of the Liebig principle which is parameterized in most (maybe all) of the biogeochemical models used in the analysis. This could be thus a consequence of the choice of the Michaelis-Menten constants for iron and nitrogen. I would indeed expect such a response in areas where iron supply is provided not from preformed iron below the thermocline but from atmospheric deposition. However this is not the case for the Southern Ocean. In my view this part deserves a more thorough explanation and discussion.

**P1432\_L5-8** Since this exercise has been previously done, it would be good to provide some considerations on the use of different climate change scenarios and whether they may have some impact on the results.

**P1434\_L20-23** It is not clear why there should be a discrimination between micro and macro-nutrients in the Southern Ocean when the controlling process is the same, i.e. that shallowing of the mixed layer (this is related to the comment above at P1431)

**P1435\_L1-13** This is an interesting discussion that I tend to support. However I wonder if these conclusions can be derived from the presented results alone. For instance data on carbon fluxes and particulate sinking rates have not been considered in the analysis. I'm not saying that it should be done but I think these statements should be complemented at least by reference to analysis done in other publications.

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**P1436\_L14-25** I am a bit puzzled by the lack of relationship between the number of years required to detect the climate signal and “biomes” (I would have chosen a different word but I see it has been accepted in a previous publication) where it is known that climate variability is large (as for instance the North Pacific and North Atlantic). When the authors speak about natural variability do they intend climate variability or natural interannual variability in species competition and blooms? I think it is the second but then I am missing a brief comment on the role of natural climate variability (NAO, PDO, etc).

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