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Interactive comment on "A latitudinally-banded phytoplankton response to 21st century climate change in the Southern Ocean across the CMIP5 model suite" *by* S. Leung et al.

Anonymous Referee #2

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The manuscript, 'A latitudinally-banded phytoplankton response to 21st century climate change in the Southern Ocean across the CMIP5 model suite' provides a statistical assessment of ecological changes in the Southern Ocean exhibited in the CMIP5 suite under projected climate change. They propose a latitudal banding into four biogeographical provinces that respond quite differently to regional patterns of climate change. I think the manuscript could make a novel contribution in this regard. However, I also have four main concerns. First, while the authors provide a correlative justification for mechanistic causation in each case, it is unclear how robust each of these explanatory factors might be... without conducting sensitivity tests with each model, how do the authors know for sure that their inferred drives are true the causative one?

It should be made more explicit that 1) these inferred linkages are speculative based on correlation, 2) the magnitude of changes in the proposed factors are hypotheitically large enough to drive most of the change, and 3) discussion of potential alternative explanations. Second, the use of 'sign of change' is potentially very misleading to a community that is accustomed to changes being expressed as integrated anomalies - I am very concerned that readers will interpret a null result that 50% of pixels increase and 50% of pixes decrease as alternatively that the mena value increased (or decreased) by 50%... there needs to be a more thorough statistical justification for this approach based on the result of a null test where nothing changes. Third, how strong are any of these changes? There should be a figure quantifying the magnitude of the change in each band to compliment figure 5 that currently gives the percentage of pixels that agrred on the sign of the change. Finally, the purpose of the section on SAM observations seemed out of context and potentially in conflict with another paper in preparation. I recommend the section be better integrated into the current paper or removed.

Specific comments:

pg 8161, ln 3 - Distribution misspelled

pg 8161 - It is currently not clear how much of the results are novel relative to the Cabre et al 2014 paper. Given the overlap in topic and methods, the analysis overlap and distinct contribution of the present work should be made explicit

pg 8162 - I am not sure if the application of the bootstrapping is mechanistically justifiable. While I appreciate that this method bring correlation analysis to a more sophisticated statistical level, I have a hard time appreciating what the mechanistic significance of the signs being correlated 64% of the time and anticorrelared 36% of the time... Which of the two cases is more relevant to a causal mechanism can only be determined by either direct sensitivity tests or scaling via simplified idealization.

pg 8162 - When assessing sign of change, did the authors apply a magnitude threshold of significance, or just much at the sign independent of magnitude?

pg 8163 - The discussion of all the figure specifications is typically limited to the discussion of the Figures themselves either in the text or captions rather than separately in the methods before the figures are introduced. I reccomend moving this secion.

pg 8166-8167 - The discussion of different mechanisms operating on different timescales is intriguing but speculative, incomplete and not well justified.

pg 8167 - The iron supply mechanism appears suspect - do the authors know that supply increased. Or are they inferring this from concentrations. At least some (all?) of the models considered assumed fixed climatologies of iron supply. My guess is increased salinity stratification leading to shallower mld south of 50.

pg 8168 - In the interannual variability discsussion, the authors should note that the models are 'perfect' integraters in their annual time averages while observations are a collection of snapshots and the implications for interpretation of the bootstrapping.

pg 8171 ln 17, pg 8172 ln 9, pg 8172 ln 13, pg 8174 ln 21, and pg 8180 ln 11 - Justification that something 'comes from theory' or 'accepted theory' or consistent with general expectation from theory' is totally unhelpful. The particular theory of relevance should be cited and described. The authors seem to think that one should trust that all theories are truth... They are not.

Throughout - The nine instances of 'In order to' should be reduced to 'To'

pg 8176 ln 4-5 - The satellite interpretation should be prefaced by' if the models are to be believed ' before saying that the record isn't long enough to see a trend... There may indeed be trends in the satellite record, they just may not be attributed to climate warming as evidenced by the models. The authors are this expressing a severe overconfidence in the models to the detriment of interpreting observations in an unbiased manner.

pg 8177 ln 3 to pg 8179 ln 14 - Instead of reviewing the literature, the authors should only bring in past observational studies to specifically describe how these studies support or refute the expected behavior of the models. In this line, it seems like the authors should be interpreting the models on the same timescale as the observations to illustrate the scope of the signal to noise relative to the 100 year trends.

8180 ln 1-5 - If the authors are concluding that SAM is the central mechanism, the authors should be assessing the biomass and productivity changes as a function of SAM in the present paper rather than citing a manuscript in preparation. I wonder if this whole section on SAM (3.4 Linking CMIP5 model projections to observations and figure 6) should be removed.

pg 8181 ln 7-10 - In concluding that there should be 'at a minimum, one or two representative time series' sites for each of the four Southern Ocean provinces they characterize, the author's are pointing out that the 4 provinces they discern would each require time series sampling for long term monitoring. However, the authors have a lot more specific guidance to offer on this than they currently provide, namely how one should robustly site a 'representative' location for each site... This information is critical for the recommendation to be actionable. Are these sites expected to be spatially fixed? In damping down the noise through the use of ensembles and extremely long time averaging, this work is more of a detectability study than an impacts study akin to what one would expect to derive from satellite or other observed fields with all their caveats. Are observations at time series sites demonstrably robust against all manner of spatial, temporal and causation caveats?

pg 8181 ln 18 - The assertion 'Given the fragility of polar ecosystems' here requires more support. What makes polar ecosystems particularly fragile? Is there some particular fragility that is motivating this work? If so, this motivation should also be brought up in the introduction.

Figure 5 - why aren't all the lines plotted for all the latitudes?

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