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Interactive comment on "Factors controlling coccolithophore biogeography in the Southern Ocean" *by* Cara Nissen et al.

Anonymous Referee #1

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GENERAL COMMENTS

The modelling study of Nissen et al. provides an important and interesting examination of the biotic and abiotic population and biogeographical drivers of coccolithophores, in the context of other phytoplankton groups, in the Southern Ocean. The paper is well written and clear to follow in all aspects, with the results neatly summarised in the main figures and text. There is also an appropriate level of appreciation of the limits of the model output and field data (though a few omissions which should be addressed, see comments below). I have only minor comments to make:

1. Non-grazing mortality – It is not explicitly discussed in the paper as to what the authors consider this to be. Viral lysis is seen as a major mortality pathway for coccolithophore (bloom) communities and so is this what the authors mean by this terminol-

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Discussion paper

ogy? How is it parameterised and does it fairly represent viral mortality or (e.g.) programmed cell death? Not representing (or discussing) such a major mortality pathway seems like a limitation of the study, but a necessary limitation due to the uncertainties around viral mortality dynamics and its role in the Southern Ocean. The authors should include viral mortality in their discussion over model limitations, as well as directions for future field observations.

2. Importance of bottom-up and top-down controls – The conclusion that both types of controls need to be considered when examining phytoplankton (and coccolithophore) population dynamics and biogeography is very important point to be made. However, the statement is not limited to the Southern Ocean and is relevant across the full biogeographical range of coccolithophores.

3. Coccolithophores/Emiliania huxleyi – Do the authors consider they have parameterised their model to describe the whole coccolithophore community, or rather that they are limited to E. huxleyi dynamics in the Southern Ocean? For this region it is relatively simple as E. huxleyi dominates (to almost monospecific levels depending on latitude). Within the authors recognised limitations, discussion of this point should be considered, especially if there are aspirations to expand such modelling efforts to low-latitude highly-diverse coccolithophore communities. Related to this point, the 400% overestimation of coccolithophore biomass (Pg 19, Lns 25-26) applies to the whole coccolithophore diversity, and in diverse communities would indeed lead to significant issues, however in the E. huxleyi dominated Southern Ocean such issues are far less extreme. There are also numerous estimates of E. huxleyi cell biomass (and even B/C biomass) which are in agreement (and don't vary by 400%).

SPECIFIC COMMENTS

Pg 1, Ln 16: Please specify 'Ocean Acidification' rather than just 'acidification'.

Pg 1, Ln 22: It is not just the ratio of calcifying to silicifying phytoplankton that is crucial to consider, it is the ratio of calcifying to non-calcifying (organic only) phytoplankton.

Pg 2, Lns 4-5: It should be recognised that all these references are model based estimates rather than field estimates, and also take varying ways to parameterise coccolithophore production. See also pg 19, ln 21 – here it should also be recognised that these low estimates of coccolithophore NPP are derived from model studies with diverse parameterisations of coccolithophore calcification.

Pg 2, Lns 10-11: Cell densities of 2.4 x 103 cells mL-1 have to be for the Patagonian Shelf bloom and are really (really) high whilst cell densities elsewhere in the Atlantic sector of the SO are much (much) lower. The authors should make it clear that these high numbers are from bloom waters.

Pg 3, Ln 14: Please make clear that zooplankton grazing includes both micro- and macro-zooplankton (rather than just the latter).

Pg 4, Lns 6-7: 'Coccolithophores grow well at high light intensities and at a range of different temperatures, but have been shown to be light-inhibited at low light levels' – does this statement fit coccolithophores as a group or just E. huxleyi?

Pg 5, Ln 19: What is the justification (reference) for using such extremely low carbon to chlorophyll ratios (3 to 5)? These lead to extremely chlorophyll-rich phytoplankton cells whereas ratios are typically 10 to 20 times higher. Are these based on Southern Ocean studies?

Figure 2: Colours seem to have changed on panel (a) – blue looks olive green and grey looks to be light green?

Pg 14, Ln 4: extra 'a' in this sentence.

Pg 20, Ln 30: A key statement – 'coccolithophores appear to be of minor importance for global oceanic organic carbon fixation'. Many in situ studies agree with such small contributions to phytoplankton biomass or primary production in the Southern Ocean (including those already cited in the paper: Smith et al., 2017; Charalampopoulou et al., 2016; Poulton et al., 2013; Hinz et al., 2012).

Pg 24, Lns 22: ‘Based on our findings, future SO in-situ studies should consider both bottom-up and top-down factors when assessing coccolithophore biogeography in space and time’. This statement should not be limited to just the Southern Ocean.

Pg 25, Lns 19 and 22-23: As well as multiple trophic levels (and trophic cascades), what about non-grazing mortality (i.e. viral mortality?). This is not discussed anywhere in the paper and the omission of viral driven population dynamics needs to be addressed in the limitations.

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