

# ***Interactive comment on “Seasonal and interannual variability of coccolithophore blooms in the North East-Atlantic Ocean from a 18-year time-series of satellite water-leaving radiance” by L. Perrot et al.***

**Anonymous Referee #2**

Received and published: 19 May 2016

## GENERAL COMMENTS

The paper by Perrot et al. presents a novel analytical method examining satellite reflectance data on the presence and phenology of coccolithophore blooms in the NE Atlantic Ocean (with a focus on shelf seas and shelf break). In parts the paper reads as a methods paper, with some results and new insights gained, though not discussed in depth. At some level the authors need to decide whether the aim of the paper is to validate a new method or to examine the phenology of coccolithophore blooms, and give a clearer focus on that aspect in the revised paper. Although generally well written, there are a number of grammatical and spelling errors which distract from the flow of the paper.

[Printer-friendly version](#)

[Discussion paper](#)



The paper also suffers from unclear units: specifically, what are the units of SPM and calcite, mg L<sup>-1</sup> of what? Carbon? Calcite? Dry weight? Clarifying this is key to the paper as it would make things much clearer as how much agreement might be expected between SPM and calcite and also allow comparison with other studies in the literature.

Another issue is the values used to estimate total coccolithophore biomass (note this is only calcite and does not include organic carbon; i.e. only about half of the full mass of coccolithophores). The authors cite Beaufort et al. (2011), with coccolith values of 15 pg coccolith mass (note this is CaCO<sub>3</sub>) and 60 pg coccospHERE (cell) mass (also CaCO<sub>3</sub>). The value of 15 pg per coccolith is much higher than many other estimates of coccolith mass in the literature; for example 2.3 to 4.6 pg CaCO<sub>3</sub> coccolith-1 (Young and Ziveri, 2000 Deep-Sea Research II 47, 1679-1700), 3.9 pg CaCO<sub>3</sub> coccolith-1 (Balch et al., 1996), or 1.5 to 6.8 pg CaCO<sub>3</sub> coccolith-1 depending on morphotype of *E. huxleyi* (Poulton et al., 2011 MEPS 443, 1-17). Hence, a more reasonable value would be 5 pg CaCO<sub>3</sub> coccolith-1; note that 15 pg per coccolith infers that the cells only have four coccoliths per cell to have a mass of 60 pg CaCO<sub>3</sub> cell-1, whereas coccospHERes are composed of at least 12-15 coccoliths. I would recommend the authors rethink or better justify the values used in the paper.

#### SPECIFIC COMMENTS

pg 1, ln 9: Evaluating rather than evaluate

pg 1, ln 13: Moore (2009), is the right reference? Shouldn't it be Moore et al. (2009)?

pg 1, ln 25: Define nanophytoplankton, what size range is this?

pg 2, ln 9: Make clear to the reader why the emphasis on *Emiliania huxleyi* (i.e. this is the coccolithophore species which most often forms blooms and numerically dominates blooms)

pg 2, ln 10: Winter et al., (2014) has a southern ocean focus, what about citing Smyth

[Printer-friendly version](#)

[Discussion paper](#)



et al. (2004; Time series of coccolithophore activity in the Barents Sea, from twenty years of satellite imagery. GRL 31, L11302) to give a northern perspective as well.

pg 2, ln 13: in rather than on (our studied area)

pg 2, ln 29: Why not explain why coccolithophore blooms tend to have low chlorophyll (i.e. they are not high biomass blooms, rather high concentrations of calcite in the form of detached coccoliths)?

pg 7, ln 24-25: In terms of a correlation, is it not better to give the slope of the line and statistical significance ( $p=?$ ), especially when supporting agreement between two datasets?

pg 8, ln 20: mg L<sup>-1</sup> by pixels - please better explain these units throughout the paper - is it areal total? Why not express as grams or tons?

pg 8, ln 21 (and onwards): missing term in  $4.1 \times 10^4$  rather than  $4.1 \times 104$

pg 8, ln 33: Thierstein et al., 2004 - listed in references as the book rather than a specific chapter; better to cite Tyrrell and Merico (2004) as through the rest of the paper.

pg 10, ln 4: A case-study in the Bay of Biscay?

pg 10 onwards: dates as xth or x (24th or 24 April), please use journal specific units throughout.

pg 11, ln 6: Again report the significance ( $p=?$ ) of the correlation between SPM and calcite, and also the slope of the line.

pg 12, ln 1: 'for forcing light through the water column' - forcing? modelling it through the water column?

pg 12, ln 2: 'suppressed' from the SPM product or deducted?

pg 12, ln 28-29: Low chlorophyll in blooms is not a particularity (peculiarity?) of coc-

[Printer-friendly version](#)

[Discussion paper](#)



colithophore blooms (see previous comments over low biomass in blooms).

OSD

Figures Figure 1. Grid on b-d makes it very difficult to see, what about reducing them to different latitudes? Also lack of mass term for the units.

Figure 3. Again grid excessive and lack of mass for the units.

Interactive comment

Figure 5. Again grid excessive and lack of mass for the units.

Figure 6. Would be better to see the data points per unit of time.

Figure 9. Are the coccoliths and coccospores in units of mg L<sup>-1</sup> or ml<sup>-1</sup>? Also, hard to see the trends in (c) due to overlapping error bars.

Figure 11. What is the y-axis? Is it the areal extent of the bloom?

---

Interactive comment on Ocean Sci. Discuss., doi:10.5194/os-2016-13, 2016.

[Printer-friendly version](#)

[Discussion paper](#)

