Referee 2:

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.

The paper also suffers from unclear units: specifically, what are the units of SPM and calcite, mg L 1 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 CaCO3) and 60 pg coccosphere (cell) mass (also CaCO3). 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 CaCO3 coccolith-1 (Young and Ziveri, 2000 Deep-Sea Research II 47, 1679-1700), 3.9 pg CaCO3 coccolith-1 (Balch et al., 1996), or 1.5 to 6.8 pg CaCO3 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 CaCO3 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 CaCO3 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.

Response:
As noted also by Referee 1, there are two main questions which are addressed in this paper: Identification and quantification of coccolithophore blooms from satelllite radiance, and application to a 18-year data set of satellite radiance. In the 'results' and 'discusssion' chapters both issues were mixed although we are aware that the main interest of this article deals with the phenology and the variability of the coccolithphore blooms, not with the methodology. We have made major modification and reorganisation in the 'results' and 'discussion' chapters, as well as in the abstract, in order to make clearer these two aspects of the paper, giving to the second point the priority in the discusssion.
We also mentioned in the discussion some new results, based on turbidity profiles in the Bay of Biscay in May, corroborating the representativeness of the satellite products as indicators of the presence and abundance of coccolithophore blooms (see response to Referee1 for more details).

Calcite is expressed in the paper in mg.L-1 of CaCO3, and the non-algal SPM concentration (defined in the satellite algorithm as the particles not related to the bloom) is expressed in mg.L-1. Non-algal SPM corresponds mainly to calcareous matter in the case of coccolithophore blooms.
The references of coccolith mass in term of calcite proposed by the referee are taken into account in the manuscript.

SPECIFIC COMMENTS

pg 1, ln 9: Evaluating rather than evaluate:
Response: modification has been made.

pg 1, ln 13: Moore (2009), is the right reference? Shouldn't it be Moore et al. (2009)?
Response: The reference has been corrected: "developed by Moore et al. (2009)..**"**

pg 1, ln 25: Define nanophytoplankton, what size range is this?
Response: The information has been added: "nanophytoplankton size-class **(3-20 μm)"**

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):
Response: Some information about the fact to focus on *Emiliana huxleyi* has been added in the

Introduction: "Coccolithophore are mainly represented by *Emiliana huxleyi* which is the most numerically important coccolihophore in the ocean. (Paasche, 2001)."

Winter et al., (2014) has a southern ocean focus, what about citing Smyth 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.
Response: Yes, this reference is pertinent in this part of Introduction. Sentence has been added to complete this paragraph: "**Times-series of satellite imagery between 1978 and 2004 show an advancing of coccolithophore blooms from Barents Sea to sub-Artic seas (Smyth et al., 2004)"**

pg 2, ln 13: in rather than on (our studied area): Response: modification has been made.

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)?
Response: The sentence is completed: **" ii- detect coccolithophore blooms based on other proxies than chlorophyll as this phytoplankton is not associated with high-biomass blooms ($<1\mu g\ L^{-1}$, Tyrrell and Merico, 2004)."**

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?
Response: Information about the correlation, which is significant, is added: $r^2=0.89$ with $p<0.001$ and slope=0.83.

pg 8, ln 20: mg L-1 by pixels - please better explain these units throughout the paper - is it areal total? Why not express as grams or tons?
Response: The indication "mg.L-1 by pixels" in the text is unclear: SPM_{fc} describes in maps (Figure 1, 3 and 5) the concentration of suspended particulate matter corresponding to coccoliths, for each pixel. This concentration is expressed in $mg.L^{-1}$ for each day and each pixel between 1998 and 2015. The quantitatif budget of SPMfc is then computed for each day and corresponds to the sum of SPMfc concentration of all pixels for each day.
Fortnight seasonal variabilities of SPMfc for the global area and the four sub-areas are based on these daily SPMfc budgets expressed in mg.L-1, in the view to analyze the variability of the productivity described by the "mass" of blooms in parallel with the variability of number of pixels of blooms (which describes the surface of blooms).

pg 8, ln 21 (and onwards): missing term in 4.1 104 rather than 4.1 x 104: Response: modification has been made in the text.

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.
Response: Modification is made to complete the reference, and the reference of Tyrell and Merico is added: "**(Rost and Riebesell, 2004; Tyrell and Merico, 2004)."**

pg 10, ln 4: A case-study in the Bay of Biscay? Response: The title has been changed.

pg 10 onwards: dates as xth or x (24th or 24 April), please use journal specific units throughout.
Response: Modifications have been made throughout the text.

pg 11, ln 6: Again report the significance (p=?) of the correlation between SPM and calcite, and also the slope of the line.:
Response: The correlation between SPM concentration and calcite ($r^2=0.92$) has a significance $p<0.01$ and the slope=5.33.

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

<u>Response:</u> The sentence has been modified: "**The SPM product has been designed for providing light attenuation in biogeochemical modelling**,.."

pg 12, ln 2: 'suppressed' from the SPM product or deducted?
<u>Response:</u> In this paper, coccolith signal is deducted from SPM product. For a future work focusing only on non-biogenic SPM signal, the coccolith signal should be "suppressed", or "removed", from SPM signal.

pg 12, ln 28-29: Low chlorophyll in blooms is not a particularity (peculiarity?) of coccolithophore blooms (see previous comments over low biomass in blooms).
<u>Response:</u> Sentence has been modified: "**One particularity of coccolithophore blooms is their low biomass in blooms (Tyrrell and Merico, 2004),** thus pixels where both SPM and **Chl** signatures are high are not selected for the assessment of the mean spectrum".