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
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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.

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 CaCO\(_3\)) and 60 pg cccossphere (cell) mass (also CaCO\(_3\)). 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\(_3\) coccolith\(^{-1}\) (Young and Ziveri, 2000 Deep-Sea Research II 47, 1679-1700), 3.9 pg CaCO\(_3\) coccolith\(^{-1}\) (Balch et al., 1996), or 1.5 to 6.8 pg CaCO\(_3\) 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\(_3\) 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\(_3\) cell\(^{-1}\), whereas cccosspheres 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