

RESPONSE TO REVIEWERS

We would like to thank reviewer for his/her comments. We have done our best to address each of the points as detailed below.

Note: All reviewer comments in *italics*; all responses by the authors in normal font.

Anonymous Referee #2

Overall: This paper describes the water insoluble organic mass fraction of sea spray (OMss) as a function of chlorophyll concentration, wind speed, and aerosol diameter. The goal is to be useful in global models containing the organic flux from ocean emissions. The paper is well written and clear, and while the authors acknowledge many of the uncertainties associated with the analysis more work needs to be done on certain points. We applaud the effort to define the OMss as a function of multiple parameters because it is clearly a multivariate system. Using just one parameter may result in overlooking the real cause of the change in OMss fraction. That being said, I believe there is a basic flaw in the logic of using SS particle size to predict SS composition — since these variables are dependent: i.e. isn't this using one particle characteristic to predict another rather than using an atmospheric or oceanic variable to predict particle production? Also, future work should really focus on moving past using only chlorophyll concentration as the overall proxy for all biological activity in the ocean as it is unlikely to be representative of microlayer organic fraction. On the positive side the authors have made a first attempt to combine both meteorological and oceanic data to predict the organic fraction of sea spray aerosols (denoted as OMss). This is an important step because the complex processes controlling OMss are likely occurring in both the ocean and the atmosphere. However, there are sizeable uncertainties in all of the concepts and data that the authors base their parameterization on. In addition, I believe that there are some large mistakes in the method used to derive the parameterization, which I detail below. Consequently, the authors' conclusion that they have identified the three "most important" controlling factors is not merited.

In addition to the updated manuscript changes described below, the conclusion is changed to state that these factors were the most important of the factors that were explored in the study.

Major Points

#1. The conceptual picture presented in Fig. 1 is not as certain as the authors would have us believe. Wurl et al., (2010) (referenced in the paper) contains a sizeable dataset that suggests that for surfactants at least SML enrichment does not depend on wind speed up to 10 m/s (plus numerous references that support this statement). Wurl et al. argue that at moderate wind speeds SML enrichment may actually be increased by an increase in the number of bubbles that effectively pump organic material to the ocean surface where it may quickly form films. This is just a hypothesis, but the point is that our understanding of SML enrichment is much more immature than the conceptual picture introduced by the authors would have the audience of atmospheric scientists believe. Note the manuscript doesn't mention other factors that affect SML enrichment such as trophic level and the specific composition of seawater organics. In

addition, the discussion of SML enrichment is hampered by the fact that actual enrichment factors are never reported. In Fig. 1 enrichment is falsely represented as OC/Na ratios in the SML. These are not enrichment factors; They must be divided by the corresponding ratios in subsurface water to obtain enrichment factors. Properly reporting the range of SML enrichment factors generally observed (from less than 1 (i.e. depletion) to ~5) would give atmospheric scientists a much better physical understanding of the processes occurring here.

We agree with the reviewer that the scientific understanding of the issue of organic enrichment of the sea surface microlayer (SML) is low. However, we believe that there is enough evidence in the literature of the conceptual picture used in this study. A conceptual picture is not a certainty and does not require a high level of understanding of the topic; rather it serves as hypothetical illustration of the processes. We also strongly caution reviewer to base his/her opinion on a single (although recent) study for surfactant enrichment in SML. It has been shown that different measurement techniques (mesh screens, glass plates, rotating cylinders and membranes) can give vastly different results (e.g., Falkowska, 1999). For example, the thickness of a microlayer collected by Teflon plate was found to be inversely proportional to the wind speed, while glass plate showed positive correlation (Falkowska, 1999). Rotating cylinder sampler also suggested linear reduction of SML thickness with the wind speed (Liu and Dickhut, 1998). Negative relationship between particular organic matter enrichment and wind speed was also found for metal screen (Obernosterer et al., 2008). It has also been documented that thickness of the SML collected using the glass plate technique can be affected by the wind speed, temperature, slick presence, glass plate withdrawal speed and the creation of ‘calm spot’ (Carlson, 1982). Furthermore, Wurl et al (2009) reported significant linear anticorrelation between transparent exopolymer particle (TEP) enrichment in the SML and the wind speed, while surface-active substances (SAS) and total dissolved carbohydrates (TDC) did not reveal such a relationship.

It is also incorrect to think that bubble-mediated processes are the only mechanism for increasing SML enrichment. TEP is shown to be positively buoyant and able to ascend the water column (Azetsu-Scott and Passow, 2004). The ascending TEP aggregates represent a mechanism for SML formation under totally calm conditions. Cunliffe et al. (2011) name a number of water column processes (convection, mobile biota, biota attached to buoyant particles, bubbles, buoyant TEPs and diffusion) that regulate the accumulation of material in SML. Wave motion and bursting bubbles, on the other hand, may reduce, disturb or even destroy SML (Wurl and Obbard, 2004; Cunliffe et al., 2011).

The updated manuscript includes a more extensive review of the topic.

#2. The inverse relationship between OMss and U10 displayed in Fig. 2a does not support Fig. 1 due to the following reasons. OMss is calculated from ambient OA measurements. It is highly probable that both continental OA and secondary marine OA are contributing to these ambient samples. This source of uncertainty is raised (though not quantified) by the authors in their caveats and uncertainties section (4), but it is not quantified. In addition and perhaps more importantly, local wind speed data is used to generate this plot. Most of the OA measured at the 2 coastal locations, if it is indeed from primary marine and not secondary sources, must have been generated well upwind of the 2 sites, in some cases probably a few days upwind if there were no rain to wash it out. Therefore it is incorrect to use wind speed data at the point of

sampling rather than the point(s) of OMss generation. Again section 4 raises this possibility but only a weak analysis is presented (for only 1 of the 2 sites!) to suggest that this is not a significant source of error. {Note – that the existence of a correlation may occur in isolated cases when the source and receptor regions are linked by a synoptic system; generalizing this to a global model is not appropriate.} Given these issues I don't believe Fig. 2a supports the Fig. 1 conceptual picture at all. To my mind a more likely explanation for the observed inverse trend is that there are relatively constant sources of OA that are 'diluted' by increases in local (e.g. surf zone) sea spray production as winds increase (recall OMss is defined as the ratio of WIOM to WIOM + sea salt). Since the authors consider the PM1 and PM2.5 size fractions a large proportion of the added sea spray mass will come from large particles that are known to be predominantly sea salt.

We agree with the reviewer that there are several possible sources of uncertainty concerning nature of the organic aerosols that were measured at both sites. To address the potential influence of continental OA and secondary marine OA in the Point Reyes data analysis, we used a strict filter on the wind direction and elemental carbon concentration. It has been previously documented (Ceburnis et al., 2011) that it is highly unlikely that continental OA concentrations be significant when the winds are constantly blowing onshore (with thousands of km upwind fetch) and the elemental carbon concentrations are below 50 ng m^{-3} . Secondary marine OA are inevitably sampled along with primary OA in the filters, but we have attempted to minimize their influence by assuming that 30% of the OA is water soluble (which has been shown to be a characteristic of secondary marine aerosols (Ceburnis et al., 2008). Furthermore, significant contribution of secondary marine OA is unlikely as Rinaldi et al. (2010) has shown that water soluble OA can be formed by atmospheric aging of primary OA rather than from strictly secondary formation. For Mace Head, we believe that the WIOM sampling effectively filters secondary marine OA (Facchini et al., 2008) and that continental OA is minimal due to the wind direction filtering (see Ceburnis et al., 2011 for carbon isotope analysis of this effect).

We also agree with the reviewer that the source region of the aerosols does not necessarily have the same oceanic and meteorological conditions as near the sampling site. An upwind footprint of several days is unsupported if one tries to calculate fluxes into boundary layer and considering negative WIOC gradients from Ceburnis et al. (2008) when footprint was outside biologically active area, effectively removing WIOC emitted far from the site. The low temporal resolution of the measurements (24-100 hours) and their long-term nature (8 years) is expected to yield relationships suitable for global climate models like CAM5 that give an average realization of the atmospheric state and therefore should not be directly compared to the measurements conducted for a short period of time at specific locations. Furthermore, Rinaldi et al. (2009) shows similar chemical composition of aerosols at Mace Head coastal station and open ocean.

The updated manuscript includes additional wind speed analysis of local and upwind wind speed for both sites.

The reviewer's hypothesis of a constant OA source that is diluted by sea salt is plausible, albeit inconsistent with the data. Firstly, it has been well documented by Ceburnis et al. (2008) that Mace Head aerosol measurements at 10 meters are not much affected by the surf zone as the concentration footprint is much larger. A constant OA source diluted by sea salt is also in contrast with evidence that ocean biological activity has a strong effect on the OA concentration (O'Dowd et al., 2004; Sciare et al., 2009). If one assumes a constant OA source scaled to ocean biology, the differences between our explanation of Fig. 2a and the dilution hypothesis are more

subtle. While both our Eq. (1) and the dilution hypotheses have progressively less organic enrichment of sea spray as the wind speed increases, our formulation combined with the Mårtensson et al. (2003) sea spray function predict increasing OA emissions with wind speed (roughly linearly due to the combination of the negative logistic relationship between OM_{SSA} and U_{10} and the exponential positive relationship between sea spray emissions and U_{10}) while the dilution hypothesis would predict OA emissions unrelated to wind speed. Assuming that OA emissions and concentrations are closely related, we can use the high OA concentration episode described by Ovadnevaite et al. (2011) as a case study. The high OA concentrations observed in that study were accompanied by an increase in wind speed and elevated sea-salt concentration. It is difficult to explain these observations with the hypothesis of constant OA emission and sea salt dilution; on the other hand, our parameterization is not inconsistent with these observations as we would predict an increase in OA concentrations along with the high winds and increased sea-salt concentrations.

#3. I don't understand why equation 3 doesn't have a site-specific maximum OM_{SS} factor. In Eq. 1 $OM_{SS}(max)$ was site-specific and very different for Point Reyes (0.24) and Mace Head (0.78). I don't understand how both sites, and indeed all sites across the world, can then have the same $OM_{SS}(max)$ value of 1 in Eq. 3. Doesn't this mean if Eq. 3 was applied to Point Reyes then it would greatly over-predict the measured OA concentrations? The authors could check this by modeling a typical size distribution of sea spray particles being sampled at Point Reyes and then calculating the bulk PM2.5 organic fraction to compare to the measurements. This problem is part of a more fundamental problem of this study, which is that a parameterization is derived from data collected at only 2 coastal sites and then applied world-wide without any validation.

Eq. 3 is not site specific because it is size-dependent and the OM_{SSA}^{max} value basically becomes the OM_{SSA}^{max} for organic enrichment at all the aerosol sizes. With the evidence of progressively higher organic enrichment with decreasing aerosol size (Keene et al., 2007; Facchini et al, 2008), and the possibility of an external mixture of sea salt and pure marine organics (Leck and Bigg, 2007; Hawkins et al., 2010; Hultin et al., 2010; Fuentes et al, 2011) we felt it would be incorrect to set an upper limit based on the PM1.5 measurement from Mace Head or the OM_{SSA}^{max} value measured by Facchini et al. (2008). For this reason, we use the value of 1 (entirely organic) for OM_{SSA}^{max} in Eq. 3 but scale it with the chlorophyll-*a* concentration and wind speed. Using the size distribution of sea spray emissions from O'Dowd et al. (1997), Eq. 3, and typical values of 7 m s^{-1} and 0.5 mg m^{-3} for U_{10} and [Chl-*a*] respectively, the bulk organic enrichment of PM2.5 particles is ~ 3 times less than that of PM1.5 particles. This ratio is very similar to the site-specific OM_{SSA}^{max} values for Point Reyes and Mace Head.

We agree with the reviewer that the world-wide application from a small dataset is a problem inherent to all marine POA emission parameterizations and is the consequence of a lack of measurement stations focusing on marine aerosols. Emission data is even more difficult to validate because those measurements are nearly nonexistent. However, despite the fact that the current marine organic emission parameterizations did not go through a rigorous evaluation in different parts of the global ocean, they still help advancing research frontiers. Several recent papers at ACPD (Westervelt et al., 2011; Meskhidze et al., 2011) show the influence of marine organics on cloud microphysical and radiative effects. Such modeling attempts, highlighting the need for improved understanding of marine organic aerosol, will undoubtedly lead to more available data and improved validation of the emission parameterizations.

#4. Secondly, it is far from clear that small sea spray particles (< 250 nm) are always dominated by organics. Only two studies have found such particles to have high organic fractions (~80%). Both studies are quoted in the current manuscript (Facchini et al., 2008; Keene et al., 2007). However, there are also considerable hygroscopicity measurements that suggest small sea spray particles frequently have only moderate to minor OM_{ss} values (< 40%) (Sellegri et al., 2008; Modini et al., 2010, Fuentes et al., 2010, 2011; only one of these studies is quoted in the manuscript). Given this I don't believe it's reasonable to assume that the maximum organic fraction of sea spray particles is 1. Again the authors raise this point briefly in the caveats and uncertainties section 4, but I'm still wondering how different the estimated emission rates would be if, say, the maximum OM_{ss} value allowed was only 50%, or even 10%?

We do not agree with the reviewer. Studies by Facchini et al. (2008) and Keene et al. (2007) are not the only ones suggesting that small sea spray particles (< 250 nm) can be dominated by organics. Other investigators have also found low concentrations of accumulation mode particles that contain Na (Mészáros and Vissy, 1974; Bigg, 1980; Gras and Ayers, 1983; Jennings and O'Dowd, 1990; Bigg et al., 1995; Leck and Bigg, 2001; Leck et al., 2002; Leck and Bigg, 2005a,b, 2008; Bigg, 2007). Since the accumulation mode is almost always observed in the marine boundary layer, these measurements indicate that a second source, such as growth of Aitken mode particles that do not contain Na (Hoppel and Frick, 1990), can also contribute to accumulation mode aerosol number concentrations (Tyree et al., 2007). The interesting question is how/why the bubble-bursting process would form separate sea salt and organic particles (Hultin et al., 2010), answering of which, however, is outside the scope of the current paper. Finally note that presence of high organic fraction for small sea spray aerosol is not inconsistent with results of (Sellegri et al., 2008; Fuentes et al., 2010) who show considerable increase in accumulation mode aerosol number concentration with the presence of organics. Therefore, assumption of the maximum organic fraction of sea spray particles to be 1 is not inconsistent with currently available data.

The updated manuscript now includes more description about discrepancies in the literature regarding organic enrichment of sea spray. The uncertainties that these different values for OM_{SSA}^{max} may have on the global emission estimate are now included in the updated manuscript.

#5. Finally, I have an issue with the absence of an artificial cut-off for OM_{ss} at low wind speeds. It is true that such a cut-off may not be required since OM_{ss} will presumably be combined with sea spray source functions that will predict no sea spray production during calm conditions ($WS < 3-4$ m/s) (although this point is never explicitly made in the manuscript). Nevertheless, not enough is made of the simple fact that OM_{ss} will not be produced during calm conditions. This is potentially very misleading.

The update manuscript includes additional description of the difference between OM_{SSA} and emission rates and how these are affected by wind speed.

Specific comments:

#6. P10527, L 11: The OA concentration reported is a peak that has only been observed once. Average concentrations are far lower and should be reported (for Mace Head and other sites) to give the reader a more realistic picture of marine OA levels.

It is worth noting that high marine OA concentration events at Mace Head have been observed regularly, but reported only once. In the modified manuscript, we have included the requested description of average OA concentrations.

#7. P 10528, L 28: Provide references for thickness of microlayer films. Also, it should be pointed out that depletion of organics in the SML is also a possibility.

The updated manuscript includes the requested information and references.

#8. P 10528, L 29: Organic enrichment at the air-sea interface is not represented in Fig. 1 because it contains OC/Na ratios in the SML, not enrichment factors. See general comment above.

The updated manuscript includes an updated Fig. 1.

#9. P 10529, L 23: Should read ‘: :an additional: :.’, not ‘: :the mechanism for sea spray generation: :’ Why would bubble bursting stop at wind speeds greater than 11 m/s?

We have made a change in the modified manuscript.

#10. P 10530, L24: Studies that have found lower organic fractions for submicron sea spray particles should also be discussed here. See general comment above.

The updated manuscript includes information about the additional studies requested.

#11. P 10531, L 8: More precise details on the location of the Point Reyes station should be provided since it is not directly on the coast. Are there any non-marine sources (e.g. roads) that could potentially be influencing the results even during on-shore flow?

The updated manuscript includes the requested information concerning the Point Reyes station location.

#12. P 10531, L 22: Why does this avoid potential Na problems? Please explain.

The updated manuscript includes the requested explanation.

#13. P 10532, L 14: The number of OMss measurements used for each of the 2 sites should be explicitly stated somewhere in this section.

The updated manuscript includes these numbers.

#14. P 10533, L 22: I think it is incorrect to use non-weighted ocean chemistry data in this analysis. If high surface Chl a and DOC regions occurred concurrently with low wind speeds (<4 m/s), which is probable in the North Atlantic during summer for example, then these regions would not add primary organic material to the atmosphere because bubble formation and therefore sea spray generation would not be occurring. Only ocean chemistry data in regions where the wind speed is high enough to cause sea spray generation should be used in this analysis.

We disagree with the reviewer that non-weighted ocean chemistry data is used incorrectly as there is no generally accepted non-zero wind speed threshold for sea spray emissions. Also, it is unclear to us whether weighting by wind speed (as the reviewer suggests) is more appropriate than weighting by distance from the measurement site. We acknowledge that the correlations of the relationships between OM_{SSA} and other oceanic and meteorological parameters may be improved by more complex back-trajectory weighting, one that considers entrainment from free troposphere to boundary layer, mixed layer height, as well as temperature and humidity profiles.

#15. P 10535, L 6: Needs to be pointed out that these are weak correlations.

The updated manuscript includes acknowledgement of the lack of strength of these correlations.

#16. P 10535, L 26: Should this be less than 1.5 m/s?

The correct value of > 15 m/s (and < 5 m/s) is included in the updated manuscript.

#17. P 10536, L 4: I don't think it can be claimed that an inverse relationship holds for the low Chl a case, which has a correlation coefficient of only 0.08.

The updated manuscript includes a qualifier stating the weakness of the relationship with correlation coefficient of -0.28 and a p-value of 0.2.

#18. P 10536, L 8: Using a single, anomalously high OA concentration measurement to validate Eq.1 does absolutely nothing to increase confidence in it. I'm sure long term AMS data exists for Mace Head; why not validate the equation against these measurements?

We evaluated Eq. 1 with an anomalously high OA concentration event for the reason that the equation is based on 5 years of long term measurement data. It would be inappropriate to validate the equation with the same data that was used for its derivation. It is a good test to see whether the equation would still hold when validating the predicted OM_{SSA} against such an unusual event. We agree with the reviewer that validation with additional AMS data would be helpful, but this data is not yet available.

#19. P 10536, L 22: What size range is this Eq. 2 valid over?

Eq. 2 is based on measurements for aerosols with diameters from 0.125 to 8 μm in diameter. As the equation changes little for aerosol sizes above and below, we propose that the relationship described in Eq. 2 can be extrapolated to the size range of sea spray aerosol commonly used by global climate models. Discussion about the size range is included in the modified manuscript.

#20. P 10537, L 16: Eq. 1 was site-specific through the factor $OM_{SS}(max)$. Where has this site-specificity gone now Eq. 1 is combined with Eq. 2 to produce Eq. 3? I believe this is a large mistake and will likely introduce significant errors into the OM_{SS} values predicted by Eq. 3 for some sites. However, I don't know this for sure because no effort has been made to validate Eq. 3 against measurements. Even though size-resolved measurements of marine OA are rare this might be done by assuming typical size distributions and calculating bulk OA concentrations.

We disagree with the reviewer. There is no mistake in Eq. 3. We did so to avoid setting size-resolved OM_{SSA}^{max} values for every location as such measurements currently do not exist. See our response to comment #3 above regarding the possible emission of pure organic sea spray aerosol.

We are unable to validate Eq. (3) with emission measurements as these are to our knowledge nonexistent, but Meskhidze et al. (2011) show that the current parameterization improves the seasonality of predicted concentrations and prediction of fine vs. coarse mode marine organic aerosol concentrations at Amsterdam Island.

#21. P 10539, L 19: Two is not a few

The updated manuscript replaces “a few” with “two”.

#22. P 10539, L 24: What were the actual organic fractions observed by Modini et al. (2010) and Fuentes et al. (2011) for that matter? Give the reader a feel for how large this assumption is.

The updated manuscript includes these numbers.

#23. P 10539, L 27: What are the ranges of $WIOC/OC$ ratios that have been observed? Again, the reader needs to get a feel for how large this assumption is. Is this even important? What parameters is the parameterization most sensitive to?

The updated manuscript includes this information and discussion.

#24. P 10540, L 7: The 2 stated references are not enough to support the bold assertion that “: aerosol chemical composition and flux parameterizations derived from coastal measurements can provide a suitable proxy for open ocean conditions and therefore can be successfully used for global emissions assessments.” The Rinaldi reference is specific to Mace Head, so that's ok for the Mace Head data but not the Point Reyes data. The Clarke et al. study is fundamentally different because it scaled coastal measurements to the open ocean using whitecap coverage. It cannot be quoted in this context. Thus the authors have provided no justification for why they consider the Point Reyes data to be unaffected by coastal processes such as surf.

The section has been rephrased in the updated manuscript to improve the discussion/justification for the applicability of coastal measurements to the open ocean.

#25. P 10540, L 16: What about Point Reyes? Also Fig. S3 only contains QuikSCAT data for a single 1x1 degree grid box 24 hrs upwind of Mace Head. Sea spray production should be occurring continuously along the air mass back trajectory if wind speeds are high enough and there are no sinks. Therefore, Figure S3 is too simplistic to claim that the use of local wind speed data doesn't introduce large uncertainties into the approach.

The updated manuscript includes Point Reyes data in Fig. S3. We do not agree with the reviewer that this analysis is too simplistic. It is not realistic to consider no sinks as Ceburnis et al. (2008) gradients showed sinks without even considering wet deposition. As flux calculations suggest a maximum footprint of 24-48 hours, we expect that the majority of sea spray measured at the stations was emitted within the 24 hours upwind trajectory and that the 24 hours upwind wind speed value would be the most different from the local wind speed among the winds within the footprint. If these two values are comparable, it suggests that winds do not change significantly along the back trajectory and that local winds are a reasonable proxy for the winds in the majority of the concentration footprint.

#26. P 10541, L 19: The last 2 sentences completely disregard the fact that sea spray organics are only produced at wind speeds greater than 3-4 m/s when bubble formation begins. They should be removed.

We disagree with the reviewer that these sentences are factually incorrect and need to be removed because the sea spray functions used in this and other studies do not go zero at wind speeds of 3-4 m/s. However, the updated manuscript has been adjusted to clarify to the reader that organic enrichment and emission rates are not the same.

#27. Minor Corrections: Abstract: Page 10526, Line 14: insert "with" "This relationship, combined with the published: : :"

This has been corrected in the updated manuscript.

#28. Introduction: Page 10529, Line 2: Citation needed for the review of previous works.

The updated manuscript directs the reader to the appropriate references.

#29. Data and Methods: Page 10531, Line 15: Analyzed using what technique? Page 10531, Line 20: be more specific about the "problems" with the Na measurements Page 10531, Line 23: Does Point Reyes also have a "clean sector" requirement? Mention that it is discussed in a later section, since the Mace Head clean sector is described here.

These questions have been addressed in the updated manuscript.

#30. Page 10532, Line 3: Make a comment on how accurate it is to assume that 70% of the OC from Point Reyes is WIOC based on measurements at Mace Head. Are there studies at locations other than Mace Head or Amsterdam Island that would support this separation? I see now that this is discussed later, but it might be helpful to mention it here. Page 10532, Line 10: Is there any evidence of Chlorine depletion?

This discussion is included in the updated manuscript. We only used sodium and reference sea-salt composition in determining the sea-salt concentration to avoid any possibility that Chlorine depletion would affect our results.

#31. Results and Discussion: Page 10534, Line 22: Interesting. Showing the interdependence of chlorophyll and wind speed on determining the OM_{SSA} is useful since chlorophyll and wind speed each go through a range of values.

We agree with the reviewer. Another implication of the study is that there is a heightened sensitivity of OM_{SSA} at certain values of chlorophyll-*a* and wind speed.

#32. Page 10536, Lines 21-22: The measurements by Facchini et al. (2008) were taken from the R/V Celtic Explorer and not necessarily at Mace Head. Explain why these measurements were used (matches best with the time period, technique, etc.). This seems like a large part of the parameterization, so it may require further explanation.

The updated manuscript includes additional description about our selection of Facchini et al. (2008) measurements.

#33. Page 10538, Line 9: take out “parts of the” “...fluxes in different oceans...” Page 10538, Line 21: the OM_{SSA} calculated was from WIOM. How much would including the WSOM affect the growth factor and resulting conclusions? Page 10538, Line 23: Start a new paragraph at “Figure 6”. Even though it is related, this paragraph is long and can be split up. The discussion of the figure seems to be a good splitting point. Page 10538, Lines 26-28: Explain this sentence more. Is it related to the figure? Or just a generalized statement based on other work?

The updated manuscript includes these changes and an expansion of the discussion of this figure.

#34. Page 10539, Lines 4-5: “global magnitude” – do you mean global mean? Or magnitude in terms of change? Page 10539, Line 13: Check the parentheses.

These corrections are included in the updated manuscript.

#35. Caveats and uncertainty: Page 10539, Line 18: Good that it is acknowledged. Page 10539, Lines 24-26: I see that the WIOC/OC ratio for Point Reyes is discussed here. It may be useful to mention earlier that there is this discussion later.

The updated manuscript includes the earlier discussion.

#36. Conclusion: Is there a cutoff point (minimum or maximum) for chlorophyll concentration where the parameterization of OMss no longer holds true?

The updated manuscript includes this information.

#37. Figures: Figure 2 – The font in the plots (equations) seems very small. It would help the reader if the font was larger. If this does not fit in the figure, try including the equations in the caption, instead, or even in a table. Also, the 4 points that were excluded, it may be interesting to include them in the figure, just colored differently. Do those anomalously high points say anything about the bounds to which the parameterization must stay within?

The font has been increased in Figure 2 of the updated manuscript.

#38. Figure 6 – Even though it is in the caption, it may be helpful to have description on the color bar. Also, what years is the annual average taken over?

The description has been added to Figure 6 in the updated manuscript. In terms of the year, the climate model CAM5 does not use a specific year. See our response to comment #2.

References (not included in manuscript)

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