RESPONSE TO REVIEWERS
(R = reviewer; *A = authors*).

We thank both reviewers for their thorough and constructive comments on our manuscript, and reply below to their criticisms and suggestions, which surely will help to improve the revised version of the paper.

RESPONSE TO R1

General comments
This paper explores, through an experimental approach (four experiments performed in oligotrophic Mediterranean coastal and open stratified waters), the influence of a static (three fixed-depths) and dynamic (experimental vertical movement) gradient of solar radiation exposure within the upper mixed layer (UML) on two groups of response variables: one acting as phyto- and bacterioplankton physiological indicators (e.g. algal pigments, Fv/Fm, proportion of bacteria with intact membrane integrity) and the other group with biogeochemical relevance (e.g., primary and bacterial production, gross and net biological DMS production and photolisys). The response variables were measured by means of properly applied and described methods. Authors posed two interesting questions falling within the scope of BG, one photobiological and the other biogeochemical-methodological, which were quite well elucidated through a nice integration (data analysis, presentation, and discussion) of the experimental results obtained from the four experiments. I find that paper is well written and contributes novel information on how dynamic solar exposure within UML slightly disrupted the photoinhibition and photoacclimation processes associated to vertical gradient of ultraviolet radiation in marine UML. Nevertheless, I have found some deficiencies, indicated in the detailed comments below, which can be easily remedied to strengthen the final version of the paper.

Specific comments

Title
R: I propose (optionally) the following title because, as acknowledged by authors in conclusions, "the irradiance dose-response in mixing bottles was distinct (though subtle) in each of the processes measured. . ."; besides, it may reinforce the idea of static vs. dynamic light field: "Subtle differential response of planktonic primary, bacterial, and dimethylsulfide production rates to static vs. dynamic incubations in upper mixed-layer summer sea waters"
A: We have made the proposed change.

Abstract
OK, it provides a concise and complete summary. But, in order to improve consistence throughout the paper, introduce the word "subtle" before "disruption" in pg. 8853, line 15.
A: We have made the proposed change.

Introduction
R: Good review of the scientific background, and interesting questions posed. However, I do not feel entirely comfortable with the statements in pg. 8855 lines 18 to 21. I think that mixing treatment resembles more realistic conditions than fixed-depth incubations within UML (because water and organisms indeed experience vertical movement and dynamic light exposure in real UML), even though the experimental mixing times were faster than current mixing times, according to

calculations and statements in pg. 8861, lines 10 to 11. Hence, I would include this point of view in the Introduction (e.g., in pg. 8855, after lines 18 to 21).
A: In fact, it is very difficult to assess the actual mixing rates and even more to simulate them experimentally. The best we were able to do was to generate a fixed-period oscillation motion, slow but still too fast when compared with the likely in situ motion, thought to be slower and more stochastic in both speed and direction. We agree with the reviewer that ours is a step forward (from fixed-depth incubations) towards a realistic simulation of vertical mixing. In lines 18-21of the revised manuscript we state that both the dynamic and the static treatments represent a perturbation. They possibly lay at the dynamic and static end of the conditions found in the upper mixing layer (UML), and the prevalent conditions are somewhere in between.

> R: Pg. 8855 lines 18 to 21: I agree with every idea of the authors' reply but I continue thinking that they also should recognize in the ms. that experimental mixing represent conditions nearer to reality than the experimental fixed-depth treatments (because of water movement within UML), even though actual mixing rates in UML were distinct than those simulated by experimental mixing... The results found from fixed-depth treatments would serve rather as "experimental controls" for the comparison with the planktonic responses to UVR found under mixing.
> *A: We have modified the last paragraph of the Introduction acoordingly.*

Methods
Good description of suitable materials and methods. Some caveats concerning descriptions are indicated below:

R: Pg. 8856, line 4: Was temperature of samples controlled during their transport to lab and the pier? Make a statement about it.
A: In C1 and C2 the samples were transported within 1h to the lab, and kept there at room temperature, which was within +/- 1 degree of SST. The temperature was not controlled during the transport of the filled bottles to the pier and the setup of the experiment, which occurred within 30 min after the bottles had been filled. The manipulations were done as carefully as possible. In O1 and O2 the carboys were kept in a thermostated water bath until the bottles were filled, and a shorter time elapsed until the incubations were set in the water.These details have been added to the first paragraph of the Methods section.

R: Pg. 8856, line 10: Describe here the incubation bottles (were the 2.3L Teflon bottles?) and number of replicates per treatment.
A: All the incubation bottles were made of polytetrafluoroethylene (Teflon), as stated in the second paragraph of the Section 2.1. The volumes are specified in the same paragraph where the corresponding measurements are described. The number of replicates is specified in the last paragraph of 2.2.

R: Pg. 8856, lines 12-16: Mixing times were distinct between coastal (C1, C2) and oceanic experiments (O1, O2). This introduced a different fluctuating light regime between these environments, even though in C1 and C2 the bottles were incubated at shallower depths to approximate the equivalent in situ optical depths. This could make the two types of environments less comparable between them, and perhaps making less appropriate the pooling of results from all experiments. This would deserve some discussion, particularly regarding to the apparent different behavior shown by C1 for most of variables (even different to C2).
A: We agree in that experiment C1 displayed a different behavior, especially in terms of primary and bacterial production (but not so much in terms of gross DMS production). However, note that the initial samples in experiments O1 and O2 came from different light histories and their

experimental response was also slightly different. We decided to pool all the samples together to give more statistical power to our inferences or, in other words, to focus the discussion on the common trends. Discussing the differences between individual experiments can be interesting, but one can get lost in subtleties caused by a number of unknown, uncontrolled factors, that are inherent to working with natural samples.

> R: Pg. 8856, lines 12 to 16: I agree in that discussing the differences between individual experiments (e.g. C1 vs. whatever other) is out of focus of the paper and may lead reader to (perhaps) futile questions difficult to solve. However, the major differences found between C1 and the rest of experiments, at least for PPp, BP (but not so much for the light exposure incubations, fig. 5B), and Net bio. DMS production, deserve some justification to be included in the pool of all experiments. This should include not only the different light history (already justified in the ms.) but also the different mixing time imposed (and hence a different fluctuating light regime) with respect to the oceanic experiments. Besides, **I think that this concern may satisfactorily be solved by showing statistics results, both including and excluding C1 (as was displayed in fig. 4A, Ppp), for the response variables for which C1 behaved more divergently**.
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> *A: We have added the statistics excluding exp. C1 for LIR (Fig. 5) and $NP_{bio,DMS}$ (Fig. 6). In the latter case excluding C1 did not modify the results of the multiple comparisons, as explained in the figure caption.*

R: Pg. 8857, lines 24 to 26. Describe better how this calculation was made e.g. were expressed as rates per hour or per (incubation) period? How was integrated the 2 h of incubation under dark in the presence of tracer with the prior incubation time under light?
*A: The rates were expressed in pmol leu/h. We assumed the first dark incubation (done after the first 2h of light exposure) to represent the 2h period, and the second dark incubation to represent the subsequent 4h period. If BP1 is the rate in the first period and BP2 that in the second period, BP_final = (2/6)*BP1 + (4/6)*BP2. We are aware that this is a subjective approach, but we found it more consistent with the treatment of C1 and C2 experiments. This will be more clearly stated in the revised version.*

Pg. 8857, lines 24 to 26: I may agree with how time-weighted of BP rates were calculated, i.e. how much weight is given to each of the two incubation periods. However, I disagree with the reason given by authors in their reply to justify the calculations, i.e. that second dark incubation represents the subsequent 4h period. My disagreement is based in that these incubations were performed during the entire light exposure, i.e. 6h, to be consistent with that stated in pg. 8857 lines 25 to 26. Therefore, this issue should be better clarified and justified in the final version.
A: We agree with R1 that the LIR measurements done at the end of the exposure correspond to a 6h incubation period. Yet, applying a different time-weighting scheme (e.g. ¼ + ¾) did not affect the conclusions nor the statistics. The calculations are now described in the first paragraph of Section 2.2.
Besides, authors should be aware (and reflect it in the paper) that the 2h of dark incubation with the tracer represent an important share of the entire incubation period, and a time when net repair of photodamage can be operating.
A: In fact, this is what we tried to convey in Pg. 8865, lines 23-28. These lines have been slightly reworded.

Pg. 8859, line 22: Replace "...a photolysis rate constant (...) was used at each experimental location to correct..." with "...a distinct photolysis rate constant (...) for each type of experimental location (i.e. coastal or oceanic) was used to correct..."
A: Replaced.

Statistical analysis

R: Pg. 8860, line 2: How the integration was calculated? Please, detail further.

A: The integration was calculated as the sum of trapezoids formed by 'depth' (in the vertical axis) vs 'rate' (in the horizontal axis) data points. This is now detailed in the text.

R: Pg. 8860, line 4: Please, report n or degrees of freedom.

A: Done. For the comparisons among experiments, df = 3.

R: Pg. 8860, lines 5-6. It would be worth to use modern robust statistical methods instead or complementarily to classical non-parametric statistic to corroborate differences among treatments when assumptions for parametric tests are not met (e.g. ANOVA based on percentile bootstrap method; see Erceg-Hurn & Mirosevich 2008, Rose et al. 2009).

A: We have conducted additional statistical tests as suggested. The "bootci" function as implemented in Matlab 2009b has been used to generate confidence intervals for the mean in each treatment normalized to the vertical integral (=100%), with n = 1000. The 95% CI's are summarized in the table below (for process rates only). In our view these tests do not add much information to the simple ANOVA / Kruskal-Wallis tests performed before, and we will only include them in the article if the reviewer and/or editor judged it essential. Note that if the 95% intervals are computed using the median instead or the mean in each bootstrapping resampling, they are equal to the range spanned by the original data. If ANOVA (or Kruskal-Wallis) tests are performed on the larger data matrix generated through bootstrapping, highly significant differences between treatments are always obtained due to the large "sample" size. Perhaps the utility of the CI's would be to test in a more robust way whether a treatment differs from the vertical integral (i.e., if the CI includes 100% or not).

Treatment	PPp	LIR	GP_{DMS}	$NP_{bio,DMS}$
	All experiments			
Surface	74—88	89—102	131—183	151—283
Middle	92—107	100—116	99—129	96—130
Bottom	97—121	106—139	25—80	34—58
Mixing	87—120	104—133	64—110	62—156
	Experiment C1 excluded (CI calculated only for n > 2)			
Surface	82—90	88—94		150—203
Middle	105—107	108—118		103—137
Bottom	96.6—97.3	114—145		39—63
Mixing	83—90	99—139		42—83

Results and discussion

R: Good description of results and discussion, conclusions, and the arrangement of the sections. Nevertheless, I miss a discussion about broader ecological implications of the results. I feel the valuable responses to solar radiation found, particularly of variables with biogeochemical relevance (e.g. primary and bacterial production, DMS production...) through the depth gradient (fixed incubations) and the subtle effects of mixing, deserve a more extensive discussion focused on their implications in the context of global warming, and within theoretical frameworks of (controversial) CLAW hypothesis, summer DMS paradox, and Earth-system theory (after Vallina & Simó 2007; Quinn & Bates 2011, Lana et al. 2011, 2012, Galí et al. 2013). Thus, as an example, the results found at surface and middle static incubations jointly may mimic the scenario of expected prolonged shallower stratification due to global warming, confining plankton long within hypothetical

photoactive UVR damage layer (Fig. 1). In this way, (i) the maximum PPp found at middle depth, offset by mixing (resembling values from surface depth, i.e. subjected to inhibition), (ii) the absence of significant variation with depth (and mixing) of LIR measured under complete light exposure in presence of tracer (Fig. 5B) that may be judged as very realistic measurement of bacterial activity, and (iii) the sharp vertical gradient of gross and net biological DMS production (increasing with irradiance, but largely compensated by DMS photolysis), with a neutral or slight reduction due to mixing, are results that, in overall, give room to discuss in the context of shallower stratification (global warming) and the CLAW hypothesis, particularly after controversies introduced by Quinn & Bates (2011). In this line, not only DMS but also organic matter (dissolved and particulate, of biological origin) or even (volatile and non volatile) photodegradation products of DMS (also of biological origin) can affect the formation of cloud condensation nuclei (by bubble bursting at the ocean surface) at large scale. The underlying idea of this claim is that the integrated operation of biotic and abiotic variables can reinforce regulation of Earth-system (after Cresser et al. 2008, Kleidon 2010, 2012). I strongly encourage authors to include some of these aspects in discussion to reinforce the implications of their results.

A: We thank the reviewer for the comprehensive discussion of the implications of our results. We agree that these implications are very interesting regarding climate change/regulation, but we do not want to overextend the significance of our data. In our view, our results only allow to assess short-term responses of the plankton community. In the mid term (days, weeks) we would expect that the plankton thriving in a given water mass will be replaced by better adapted communities. In the longer term, biogeochemical regimes (provinces) will change their distribution. That is, performing time-for-space substitutions might be more useful than manipulating a given community (necessarily in the short term) to make predictions into the longer term.

> I disagree with authors' reply in that their results only allow to assess short-term responses of the plankton community to avoid dealing with regulation/climate change implications. I think that their results deserve the inclusion of some broad-scale implications, as pointed out in my former comments. I am aware of the risk to fall in speculative discussion, and I agree and recognize the reasons given in the authors' reply to make them reluctant for including implications of this type. However, I think that the point of their experiments and results would be to go somewhat further than a mere description of concrete experiments, even though the study of short-term planktonic responses is an interesting theme "per se". I think that some of their short-term results are extrapolable to scenarios of shallowing stratification, which may follow an intermittent or fluctuating pattern (i.e. transitorily returning to original mixing conditions) rather than a permanent modified (shallowed) mixing regime, un-der global warming, at least at early stages. This could partially refute the argument of the replacement by a better adapted plankton community as basis to reject the inclusion of mid-term implications. Following a phylosophy of science perspective, you may extrapolate the short-term results at future scenarios, "ceteris paribus". The latter could refer here, for example, to unchanged plankton community, plausible with still non-permanent changes in mixing regime. Definitely, I insist encouraging authors to include some of these implications (and mentioning warnings, caveats, cautions) to provide the paper a broader perspective, if editors agree with this idea.
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> *A: The results presented here on the enhancement of DMS production by increased UVB exposure, agree with those recently reported by our group using a variety of approaches, from light spectrum manipulation with optical filters (Galí et al. L&O 2013) to the study of diel cycles at sea (Galí et al. GBC 2013). They all converge with Vallina and Simó (2007) at pinpointing solar radiation as the main driver of DMS production and concentration in the surface ocean, and suggest mechanistic bases for this relationship: in the short term (hours) sunlight directly affects the cellular machineries of DMS-producers and DMS-consumers, and favors DMSP-to-DMS conversion pathways; in the longer term (days to weeks to*

months) sunlight shapes the seasonality of the dynamics in upper ocean physics and plankton succession, favouring DMSP producers. As a resulting emergent property, DMS tends to increase in summer even in regions where phytoplankton biomass is at their annual minimum. This phenomenon was termed the 'DMS summer paradox' (Simó and Pedrós-Alió 1999) and suggested to be at the base of a 'seasonal CLAW' hypothesis by which plankton respond to higher summer irradiances by increasing the production of cloud-brightening DMS (Vallina and Simó EC 2007). Whether this seasonal feedback will also operate efficiently at the longer time scale of anthropogenic global warming or Earth climate cycles cannot be easily predicted from short term observations. Indeed, projections point to an enhancement, expansion and longer duration of stratification by global warming, with shallower mixed layers during longer periods (Sarmiento et al. 1998), which would result in increased exposures of plankton to UVR (Díaz et al. 2000). Our results suggest that this should lead to increased DMS concentrations/emissions and hence to a potential feedback that would buffer global warming. However, likely phenomena such as the substitution of plankton species and communities towards the ones more adapted to the evolving conditions, and the development of protection strategies against environmental stress, hamper the straightforward applicability of our short term observations to long term trends.

Similar considerations have been added at the end of Section 3.5.

Diaz, S. B., J. H. Morrow and C. R. Booth (2000) UV physics and optics. In The Effects of UV Radiation in the Marine Environment (Edited by S. de Mora, S. Demers and M. Vernet), pp. 35–71. Cambridge University Press.

Sarmiento, J. L.,Hughes, T. M. C., Stouffer, R. J. & Manabe, S. (1998). Simulated response of the ocean carbón cycle to anthropogenic climate warming. Nature 393, 245–249.

Vallina, S.M., R. Simó (2007). Re-visiting the CLAW hypothesis. Environ Chem 4: 384-387.

Tables and Figures

R: Table 1: Include between parentheses absolute or percentage values of biomass of the "dominant phytoplankton (biomass)" within the field "Initial sample characteristics".

A: We prefer to disregard this suggestion because the calculation of group-specific biomass is only approximate, and allows only qualitative assessments of dominance. The calculations were made from phytoplankton counts, roughly estimating average biovolumes and then following Simó et al. (2009, AME; table 1), as indicated now in the caption of Table 1.

R: Fig. 3: Display axe title and units; also report R-squared and significance (p-value) of regression.

A: Changes made.

R: Figs. 4 and 5: Why ANOVA results (p-values) and multiple comparisons are not included in Figs. 4E to I, and 5B to C)? If all variables (except DMS production rates) were measured in duplicate incubation bottles, as stated in pg. 8859 lines 24-25, there are enough variability to perform the statistics...?

A: In fact, considering replicates in the ANOVA will increase statistical power, and indeed gave more significant differences among treatments for some variables. Unfortunately, pigment concentrations and particulate absorption coefficients could not be measured in duplicates, as reflected now in the text (see the last paragraph of 2.2.). We faced another problem when trying to include replicates in the statistical analysis: how can "analytical replicates" or "subsamples" be

included in the Kruskal-Wallis tests at a hierarchical level below that of truly different samples (C1, C2, O1, O2)? For these reasons, we decided to analyze all the variables in the same statistical framework.

R: Fig. 7: Report values of R-squared, slope and significance of regression, for fixed depth and also for mixing (either alone or fixed-depth + mixing) incubations, in order to show to which extent mixing treatment disrupted photoaclimating and photodamage processes.
A: In fact "showing to which extent the mixing treatment disrupted photoaclimating and photodamage processes " was the purpose of Fig. 8. We will calculate the confidence intervals of linear least squares regressions if the reviewer judges it appropriate.

> R: Fig. 7. Yes, please, I prefer to see R-squared, slopes and significance of the regressions (fig. 7), even though fig. 8 deals with a similar purpose.
> *A: After considering this option, we decided to leave this information summarized in Fig. 8 (adding the significance of the correlation coefficients). The reason is that the correlation coefficients (and their significance) are better than slopes for answering our question: are the processes more linearly related to dose or to irradiance?*
> *A table reporting all the proposed statistics (R^2, p, regression slopes) for the combination of 4 process rates, irradiance or dose, and radiation band (UVB, UVA, PAR) wold be far to extense: (3 statistics) x (4 processes) x (irradiance, dose) x (3 bands) = 72 entries.*

Technical Corrections
R: Pg. 8854, lines 22-23: Replace citation Helbling & Villafañe 2013 with Helbling et al. 2013. *A: Changed.*
Pg. 8859, line 6: Is it "cell-permeant" instead of "cell-permanent"? *A: Changed.*
Pg. 8859, line 6: In ". . .SybrGreen I (Molecular Probes, Eugene, OR). . ." include the acronym SGI within those parentheses. *A: Changed.*
Pg. 8867, line 25: What is the difference between DMSPt and DMSP (DMSPt was not defined).
A: DMSPt is 'total DMSP', this is now described in the text. Several studies distinguished between the particulate (DMSPp) and dissolved (DMSPd) pools, but recent works have shown that the distinction is methodologically tricky.

RESPONSE TO R2

General comments
This study aims at estimating how dynamic light exposure affects phytoplankton and bacterioplankton physiology, as well as DMS production compared to static light conditions (which are generally used). They mainly conclude that dynamic light significantly affects the responses of bacterioplankton, and does not significantly affect primary productionn or gross DMS production, compared to static light conditions. They also discuss the link between UV exposure and DMS(P) concentrations. The manuscript is well written, well structured, and informative. I have only a few suggestions/questions before recommending it for publication. In particular, I feel that the part on the DMS(P) cycling could be discusssed more in detail.

Specific comments
Abstract
R: Somewhere in the abstract it should be indicated that this study deals with short-term effects. This should also be reminded when needed in the text (e.g., when comparing with results from the literature).

A: We have slightly modified the first sentence of the abstract: "Microbial plankton experience SHORT-TERM fluctuations in...". In the last paragraph of the Introduction we have also underlined that the questions we aim to answer belong to the short-term: "The experimental design was aimed at answering two questions REGARDING THE SHORT-TERM RESPONSE OF PLANKTONIC ACTIVITY TO DYNAMIC LIGHT EXPOSURE: ..."
In our view, it is quite clear throughout the article that the study deals with short-term effects. Yet, in response to the petitions of R1, we have included a paragraph at the end of section (3.5) discussing to what extent our findings (regarding DMS cycling) can or cannot be extrapolated to longer time scales.

Introduction
R: P. 8855, l. 9-11: A review of the effects of UV irradiation on DMS(P) (in situ and in cultures, e.g., Hefu and Kirst 1997, van Rijssel and Buma 2002, Sunda et al. 2002, Slezak and Herndl 2003, Harada et al. 2009, Archer et al. 2010) could be included here, to emphasize this part of the discussion later on.
A: We have added a paragraph (las but one of the Introduction) reviewing these aspects as suggested.

Material and Methods
R: p. 8858, l. 17: What does the value of 2.303 represent in this equation?
A: The value is ln(10), which is needed to convert the log10-based absorbance to e-folding absorption coefficients.

R: p. 8858, l. 26-29: FSC, which is largely determined by the cell's size and shape, should be used to evaluate the cell size instead of SSC, which depends on internal and external structure and refractive index (Collier, 2000).
A: This is not correct… for cells in the µm range. We have empirically found in that case that SSC is better related to cell size than FSC. In most cytometers the relationship FSC-cell size breaks down at the 2-3 µm size range. It is not only us, others also use SSC to discriminate algal populations by size instead of using FSC (look at Marie & Partensky 2006 as an example, or Fig 1 in Calvo-Díaz and Morán (2006)). In Marie et al. 2005 the recommendation is to use "either SSC or FSC". Others have also found SSC to be a good predictor of bacterial cell size (Felip et al. 2007, Gasol & del Giorgio 2000, Veldhuis & Kraay 2000). Recent flow cytometry papers by a reference researcher (H. Shapiro) indicate that SSC is better related to size than FSC (See Fig 1 in Tzur et al. 2011).

FSC is also known to be dependent on the nature of the sheath fluid (Cucci & Sieracki 2001) which forces users to run samples with filtered seawater as sheath fluid, something unpractical. SSC is much less sensitive to the lack of match between sample and sheath salinity.

Cucci, T. L., and M.E. Sieracki. 2001. Effects of mismatched refractive indices in aquatic flow cytometry. Cytometry 44: 173-178.

Marie, D., N. Simon, and D. Vaulot. 2005. Phytoplankton cell counting by flow cytometry, p. 253-267. In R.A. Andersen [eds.], Algal Culturing Techniques. Academic Press.

Marie, D, Partensky, F. 2006. Analyse de micro-organismes marins, p. 211-233. In La cytométrie en flux. Lavoisier.

Gasol, J. M., and P.A. Del Giorgio. 2000. Using flow cytometry for counting natural planktonic bacteria and understanding the structure of planktonic bacterial communities. Sci Mar 64: 197-224.

Felip, M., S. Andreatta, R. Sommaruga, V. Straskrábová, and J. Catalan. 2007. Suitability of flow cytometry for estimating bacterial biovolume in natural plankton samples: comparison with microscopy data. Appl Environ Microbiol 73: 4508-4514.

Tzur, A., J.K. Moore, P. Jorgensen, H.M. Shapiro, and M.W. Kirschner. 2011. Optimizing optical flow cytometry for cell volume-based sorting and analysis. PLoS ONE 6: e16053.

Veldhuis, M. J., and G.W. Kraay. 2000. Application of flow cytometry in marine phytoplankton research: current applications and future perspectives. Sci Mar 64: 121-134.

Calvo-Díaz, A., and X.A.G. Morán. 2006. Seasonal dynamics of picoplankton in shelf waters of the southern Bay of Biscay. Aquat Microb Ecol 42: 159.

Results and Discussion

R: p. 8860, "Oceanographic settings": It would be interesting to display the concentrations of the major nutrients, and to discuss their potential role in influencing the high DMSP:Chl a ratios (besides the presence of strong DMSP producers). Higher irradiance (and UV penetration) at the open ocean stations could also explain the higher DMSP:Chl a ratios compared to the coastal stations.

A: Nutrient concentrations (phosphate, nitrate + nitrite and silicate) have been added to Table 1. A brief discussion on the potential effects of irradiance and nutrients on DMSP:Chl a ratios has been added to section 3.4, fifth paragraph.

The average UV dose in the mixed layer should not have been very different between the coastal and oceanic settings, given the ranges in UV transparency and mixed layer depths we encountered. As shown in Table 1, UVR and PAR irradiance at the water subsurface were not too different between C1/C2 and O1/O2. Thus, the differences in DMSPt/Chla should mostly be the result of differences in phytoplankton taxonomy and, as pointed out by R2, nutrient status (see fifth paragraphof section 3.4).

R: p. 8862, l. 6-9: The comparison to the study by Sommaruga et al. (2005) is not clear. What did these authors show?

A: Reworded. Sommaruga et al. showed that different picophytoplankton are not equally sensitive to UVB/UVA/PAR exposure. Prochlorococcus cell counts decreased strongly after UVR exposure due to cell death, whereas Synechococcus counts did not and picoeukaryotes showed an intermediate response.

R: p. 8863, l. 15-22: Given that the Dt:(Dd+Dt) index can vary within a few minutes (as pointed out by the authors), it can not be used with confidence and I suggest to delete this paragraph.

A: We have deleted this paragraph and the corresponding figure panel (former Fig. 4H).Actually, panel G has been used to show a new variable: DMSPt/Chla.

R: p. 8866, "Response of gross DMS production": That part of the discussion would benefit from a more detailed review of the literature, with comparisons of your results with previous results dealing with phytoplanton and UV exposure (in situ and in cultures, e.g., Hefu and Kirst 1997, van Rijssel and Buma 2002, Sunda et al. 2002, Slezak and Herndl 2003, Harada et al. 2009, Archer et al. 2010).

A: We have expanded section 3.4. The fourth paragraph has been re-written, and a new paragraph has been added after it.

R: p. 8867, l. 15-18: I don’t understand this assertion. Please clarify. Also remind the reader that this assumption may be valid only for short-term exposure.

A: Rephrased.

R: p. 8867, l. 24-25: "higher amounts of DMSP were lost as DMS (and perhaps as DMSP) at higher irradiance." Did you mean "higher amounts of DMSP were lost as DMS (and perhaps as DMSO)"?
A: Changed.

R: p. 8868, l. 2-8: It seems contradictory to me that DMSP could be cleaved into DMS by "OH radicals, without the need of DMSP cleavage enzymes", and that "intracellular DMSP pool escapes as DMS without reacting with intracellular oxidants". If DMSP is cleaved by reaction with OH radicals, then DMSP reacts with intracellular oxidants. As far as I remember from D.J. Kieber's presentation at the 2011 ASLO meeting ("Direct DMS and DMSO production from DMSP reactions with reactive oxygen species"), DMSP could indeed be cleaved directly into DMS and DMSO, and the produced DMS could itself be oxidized into DMSO. The presence of the DMSO reductase in phytoplankton could further increase the antioxidant capacity of the DMS/P/O system by fueling a coupled DMS/DMSO antioxidant cycle. Please clarify this part.
A: This paragraph has been reworded following R2's suggestions.

Technical corrections
p. 8858, l. 17: "where Afilter(λ) is the measured absorbance". *Changed*
p. 8860, l. 10: "The sampled upper mixed layer (UML) was in all cases"... *Changed*
p. 8860, l. 20; p. 8867, l. 25; and Table 1: DMSPt has not been defined. *Changed*
p. 8881, Fig. 3: The legend of the X-axis is missing. *Corrected.*