Actions taken to accommodate the comments of Referee #1, Dr. P. JLeB Williams

We thank Dr. Williams for reporting his opinions and recommendations on our paper. We are very grateful for the thoughtful comments and have sought to address them in the best way we can. We also apologize for the many errors in the original version of the manuscript, which Dr. Williams helped sort out. We think the paper has substantially improved thanks to key points raised in the recommendations.

Reviewer: The authors examine the effect of UVB radiation on net community production (NCP) by comparing rates in quartz and presumably (but not specified) borosilicate glass bottles.

Authors: Yes, the Winkler bottles were borosilicate.

Action: We now describe the material the bottles were made of. The methods section now reads "*Water samples were carefully siphoned from the Niskin bottles, into 100 ml narrow mouth borosilicate Winkler bottles*".
And the abstract reads:"*We did so by comparing metabolic rates derived using quartz bottles, transparent to UVB, and borosilicate glass, which is opaque to UVB and part of UVA, incubated under the in situ solar radiation.*"

General and overall comment

Reviewer: I cannot judge the quality of the radiation measurements, but the oxygen measurements are of very high quality and as they observe on p.5835, line 7 the differences between the +UVB and -UVB rates are significant.

It seems to me that there are quite a number of inconsistencies between the text and the data in Table 1.

Authors: Indeed, the differences were statistically significant at $P < 0.0001$ for each of the five experiments (t-test). Thank-you for alerting us to these inconsistencies between the text and the date in Table 1, which indeed were due to an error in our side (some communities were deemed as heterotrophic whereas these were autotrophic).

Action: The text was corrected, since the data in the Table 1 was correct. We now report that all of the experiments conducted showed a significantly different NCP between glass and quartz incubations.
The abstract now reads: "*All five experiments showed significantly different NCP rates between samples incubated under the full in situ solar radiation and those incubated under reduced UVB. One of the experiments showed elevated NCP when the community was exposed to the full solar radiation, while four experiments showed a significant tendency for NCP o be reduced in the presence of UVB, showing an overall significant tendency for NCP to be reduced in the presence of UVB radiation.*"

The results now reads: "*All five experiments comparing NCP rates derived from incubations in glass and quartz bottles showed significantly different NCP rates between samples incubated under the full in situ solar radiation and those incubated*

under reduced UVB (t-test, P < 0.001, Table 1). The probability that the difference in NCP between quartz and glass bottles was obtained by change was, for each individual experiment, less than 0.0001 (t-test, Table 1), and the combined probability that the difference was obtained by chance was, when the experiments were taken in concert, less than 10-6 (Fisher's χ2 test, Fisher 1925). Four of the experiments conducted showed a significant decrease in NCP in the presence of UVB radiation (t-test, P < 0.001). However, one of five experiment, that conducted at the northernmost station, yielded an increase in NCP to 6.75 ± 0.03 mmol O2 m-3 d-1 in the presence of UVB radiation compared to 3.48 ± 0.02 mmol O2 m-3 d-1 when UVB was removed (Fig. 3, Table 1). Yet, there was a strong, statistically significant overall combined probability for NCP to decline significantly in the presence of UVB (Fisher's χ2 test, p < 10-6, Fisher 1925), involving a median reduction of NCP by 0.45 mmol O2 m-3 d-1."

The discussion now reads:"*However, plankton communities exposed to UVB showed a prevalence to display reduced NCP rates when compared to those where UVB was removed.*"

Reviewer: There is also a feature that the authors need to address. Their broad conclusion is that incubations in the presence of UVB give lower rates of NCP than in its absence – this they observe in 4 out of 5 cases. From simple probability theory the statistical case is weak – with the small data set and the distribution of the data non-parametric test is probably most appropriate – my calculation is that the odds of it occurring by chance are 25% or more.

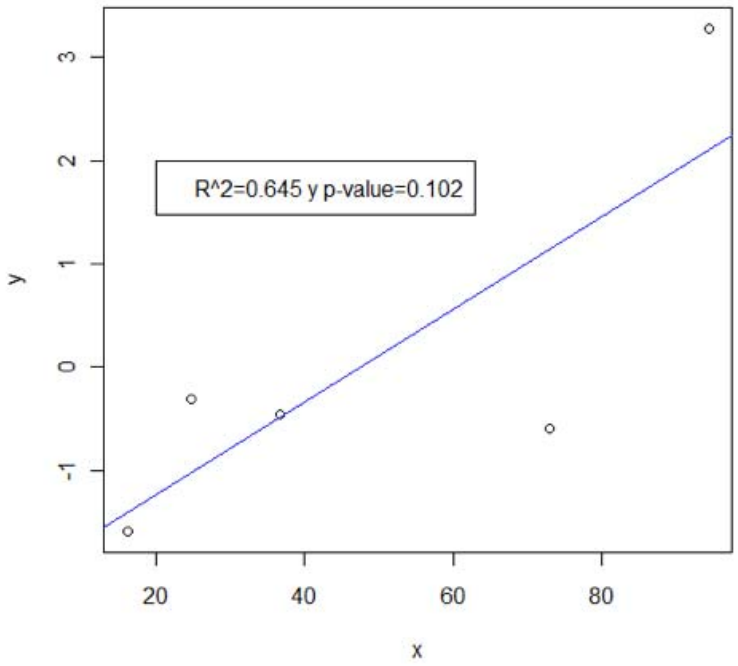
Authors: We agree that a reduction in NCP in the presence of UVB was observed in 4 out of 5 experiments. However, the inference that the odds of this result being obtained by change be 0.25 is incorrect, it would be if each individual experiment was not replicated, but they were based on replicated (7 bottles) incubations. The probability that the difference in NCP between quartz and glass bottles was obtained by change was, for each individual experiment, < 0.0001. Moreover, provided the experiments were independent of one another, it is possible to aggregate the P values for the individual t-test to derive a combined probability that the overall tendency for differences being observed was derived by change. The combined probability is 0 (i.e. < 10^-6; Fisher χ2 test, Fisher 1925). Using the same procedure the combined probability for the one sided test testing for NCP being lower in the presence of UVB is also highly significant (i.e. < 10^-6; Fisher χ^2 test, Fisher 1925), so the likelihood that the results obtained are derived by change is statistically nihil.

Action: The text now address these issues. The text now reads: "*All five experiments comparing NCP rates derived from incubations in glass and quartz bottles showed significantly different NCP rates between samples incubated under the full in situ solar radiation and those incubated under reduced UVB (t-test, P < 0.001, Table 1). The probability that the difference in NCP between quartz and glass bottles was obtained by change was, for each individual experiment, less than 0.0001 (t-test, Table 1), and the combined probability that the difference was obtained by chance was, when the experiments were taken in concert, less than 10-6 (Fisher's χ2 test, Fisher 1925). Four of the experiments conducted showed a significant decrease in NCP in the presence of UVB radiation (t-test, P < 0.001). However, one of five*

experiment, that conducted at the northernmost station, yielded an increase in NCP to 6.75 ± 0.03 mmol O2 m-3 d-1 in the presence of UVB radiation compared to 3.48 ± 0.02 mmol O2 m-3 d-1 when UVB was removed (Fig. 3, Table 1). Yet, there was a strong, statistically significant overall combined probability for NCP to decline significantly in the presence of UVB (Fisher's χ2 test, p < 10-6, Fisher 1925), involving a median reduction of NCP by 0.45 mmol O2 m-3 d-1."

Reviewer: If you do a regression analysis of the effect of UVB on NCP (quartz-glass rates) the R2 is quite high (0.8) and the relation shows a positive relationship between NCP and UVB, not an inhibition. The authors need to give thought to this.

Authors: Indeed, there is a relationship between the difference in NCP and the incident UVB, but this relationship is not statistically significant (p = 0.101) and driven by a single data point (see figure below), that corresponding to the community showing an increase in NCP in the presence of UVB, which also showed the highest overall NCP value.



Reviewer: "Indeed, NCP in the presence of UVB was negative in all but two experiments,. . ", this does not match the data in Table 1, where NCP is negative in only one instance, and it is negative both in the absence and presence of UVB.

Authors: The reviewer is correct and the statement in the text is in error. The text has been corrected as shown above.

Reviewer #1: p.5835, line 10 "with a median decrease in NCP by 2.17 mmol O2 m−3 d−1.", the median decrease as far as I can determine is -0.31, quite different from what they report.

Authors: The reviewer is correct and the statement in the text is in error, although the median reduction is 0.45 mmol O2 m−3 d−1. The text has been corrected accordingly (see above).

Reviewer #1: p.5835, line 11 "NCP increased when plankton communities were exposed to ambient UVB levels in only one station, that at the northernmost station sampled, where NCP increased to 6.75 ± 0.03 mmol O2 m−2d−1compared to 3.48±0.02 mmol O2 m−2d−1 when UVB was removed (Table 1).", this statement is correct, however the point that seems to be missed that this station had the highest UVB - 94 μwatts cm-2sec-1 at the surface and by my calculation, despite the higher Kd, still the highest at 5m. This implies an opposite effect on UVB on NCP to their general conclusion.

Authors: We agree that the last experimental station showed opposite results to the general pattern obtained on the remaining stations. Whereas this community also received the highest UVB, the community was also that growing in warmest waters and receiving the highest PAR irradiance as well. Indeed, this anomaly suggest that the response of NCP to UVB is not a simple function of the UVB level, and that communities receiving high UVB do not necessarily show reduced NCP in the presence of UVB relative to that when UVB was excluded. However, speculating on the causes of this anomaly would involve significant speculation and should be addressed in future research.

Authors: We have revised the text to acknowledge more explicitly this inconsistency and the caveat it casts upon the effect of UVB on NCP. The text now reads: "*The northernmost experimental station showed opposite results to the general pattern obtained on the remaining stations. Whereas this community also received the highest UVB, the community was also that growing in warmest waters and receiving the highest PAR irradiance as well. Indeed, this anomaly suggest that the response of NCP to UVB is not a simple function of the UVB level, and that communities receiving high UVB do not necessarily show reduced NCP in the presence of UVB relativeto that when UVB was excluded. Further research is required to identify the traits that can render some plankton communities to show enhanced NCP in response to UVB, which may involve changes in resource supply, community structure or effects of UVB on micrograzers, among other factors*".

Reviewer #1: p.5835, line 24 "Respiration rates at surface were generally lower and more uniform across stations, resulting in a prevalence of net autotrophic communities (GPP > R) throughout the entire section (Fig. 5). In contrast, the surface waters were supersaturated with CO2 at all open-ocean stations, supporting, therefore, a net efflux of CO2 into the atmosphere (Table 1)." Net heterotrophy is not the only explanation for CO2 supersaturation. The area is an upwelling area and the upwelling water will be supersaturated with CO2, further it will warm up – increasing the supersaturation. Exchange of CO2 across the air sea interface is a slow process

and there is a possibility that equilibrium may not have been reached. This needs exploring, if nothing else to eliminate it.

Authors: We agree that the net heterotrophy is not the only explanation for CO2 supersaturation.

Action: In the new version of the manuscript we now discuss better the possible role of upwelling in driving surface CO_2 supersaturation. The results now reads: "*Respiration rates at surface were generally lower and more uniform across stations, resulting in a prevalence of net autotrophic communities (GPP > R) throughout most of the section (Table 1, Fig. 5). In contrast, the surface waters were supersaturated with CO2 at all open-ocean stations, supporting, therefore, a net efflux of CO2 into the atmosphere (Table 1).*" and the discussion now reads: "*Plankton metabolic rates were generally positive (NCP > 0), indicative of autotrophic communities acting as a CO2 sink. In contrast, there was a prevalence of supersaturation of surface waters in CO2 (Table 1), suggesting that this was driven by physical and thermodynamic factors, rather than biological ones (cf. Torres et al. 2011).*"

Reviewer #1: p.5836, line 19 "All stations occupied had autotrophic plankton communities when incubated in glass, as has been done in the past." Not all, 4 out of 5 did, and with the same pattern in the case of the incubations in quartz.

Authors: We agree the statement was in error (see above).

Reviewer: p.5836, line 25 "Exposure of surface (5m) communities to UVB radiation, greatly reduced NCP in all but one community and rendered all, except one of the communities investigated heterotrophic."and p.5837, line 8 ", the communities in surface waters resulted to be, in general, strongly heterotrophic, thereby acting as a CO2 source, consistent with the supersaturation in pCO2 in surface waters driving a CO2 efflux to the atmosphere." And p.5838, line 28 "The use of quartz bottles to allow the UVB component of the irradiance field yields net heterotrophic communities in surface waters, consistent with the supersaturation in pCO2 in surface waters observed along the cruise." All three statements are, as far as I can see, incorrect. I think the authors mean is that most of them (4 out of 5) become more heterotrophic, although all remain in the same state of trophic balance – autotrophic communities remain autotrophic.

Authors: The reviewer is correct.

Action: The statement has been corrected in the revised version of the manuscript, (see above).

Reviewer: p.5837, line 22 "Our results show that, for the communities studied along the Humboldt Current System, removal of UVB increases net community production, by suppressing respiration and possibly increasing gross primary production." This is patently a wrong claim and inconsistent which their correct observation (p.5837, line 1) that "Our experiments did not allow evaluate (sic) the effect of UVB on respiration vs. that on GP, . . ".

Authors: We agree.

Action: We have corrected the statement. The text now reads: "*Our results show that, for the communities studied along the Humboldt Current System, removal of UVB affects net community production, with a prevalence of a tendency for net community metabolism to decline*".

Reviewer: p.5837, line 3 I don't think the Pringault et al reference is relevant to the present discussion, as Pringault et al specifically mention (p. 324, line 8) that they used UV-free light: "Visible light (UV and IR free) was provided by two fluorescent tubes (daylight spectrum, Sylvania Luxline plus F36W/860, Sylvania, Germany)."

Authors: The reviewer is correct.

Action: We have removed this reference.

Reviewer #1: p.5837, line 24 "Moreover, the exclusion of UVB from the solar radiation not only inflates NCP rates, but may even alter the NCP, in our case shifting the communities from net heterotrophic to autotrophic." There isn't a single instance of this in their data set as far as I can see.

Authors: We agree.

Action: The sentence, which was in error, has been removed.

Reviewer: p.5838, line 2 (also in the Abstract) "These results show that UVB radiation, ... , may have suppressed net community production of the plankton communities in the study area, possibly driving plankton communities in the Southwest Pacific toward CO2 sources." I think the authors need to qualify this statement, which actually is a speculation, especially as it is in the Abstract. Agreed, they properly use "may have" but, formally for what it's worth, the UVB dose response (i.e. the NCP downshift versus UVB irradiance level) is the inverse, so we need some caution in this respect. Secondly, and more importantly, I don't think we understand what controls NCP, so how can we project. We can expect feedbacks between P and R operating within the food web, if the time response of these feedbacks is longer than the incubation time, then short-term experiments cannot be used to make long term predictions. By all means think about what the implications of the study may be, but make the assumptions and limitations clear and also make it clear that you are speculating. Of greatest importance, if you want to put speculations in the Abstract (where there is no indication of the limits to the data) then make it clear both in the text and especially in the Abstract that it is no more than a speculation. Some people may only have ready access to the Abstract, further others may not have the background to judge the reliability statement. In both cases I would recommend "We speculate that . . . ",

Authors: The reviewer is correct, and we have revised the Abstract and Discussion accordingly, avoiding unsupported speculation and inconsistencies with the results presented.

References:

Fisher, R.A. (1925). Statistical Methods for Research Workers. Oliver and Boyd (Edinburgh).

Actions taken to accommodate the comments of Referee #2

Thank you for reporting the opinions and recommendations on our paper entitled "Impacts of UV radiation on plankton community metabolism along the Humboldt Current System". We trust these changes are acceptable, and look forward to hearing from you

General and overall comment

Reviewer # 2: I am in line with reviewer 1 that the text has to reflect what is presented in the table.

Authors: Thank-you for alerting us to these inconsistencies between the text and table 1. You are most certainly correct, and we will amend our manuscript.

Action: In the revised version of the manuscript we corrected the text inconsequence.

Reviewer # 2: Furthermore it is unfortunate that the amount of data is very low, which prevents a decent statistical evaluation of the UV effect.

Authors: The need of long incubation time, and the need of a large number of replicates have resulted in the general low number of metabolic measurements at the ocean (e.g. del Giorgio and Duarte 20020). Whereas the number of stations is limited, the experimental matrix consists of a control and treatment replicated 7 times each in the five stations, across the latitudinal gradient. Indeed, the differences in NCP between treatments including and excluding UVB were statistically significant at $P < 0.0001$ for each of the five experiments (t-test). A reduction in NCP in the presence of UVB was observed in 4 out of 5 experiments. Provided the experiments were independent of one another, it is possible to aggregate the P values for the individual t-test to derive a combined probability that the overall tendency for differences being observed was derived by change. The combined probability is 0 (i.e. < 10^-6; Fisher $\chi 2$ test, Fisher 1925). Using the same procedure the combined probability for the one sided test testing for NCP being lower in the presence of UVB is also highly significant (i.e. < 10^-6; Fisher χ^2 test, Fisher 1925), so the likelihood that the results obtained are derived by change is statistically negligible.

Action: The text now address these issues. We now report that all of the experiments conducted showed a significantly different NCP between glass and quartz incubations. The results now reads: "*All five experiments comparing NCP rates derived from incubations in glass and quartz bottles showed significantly different NCP rates between samples incubated under the full in situ solar radiation and those incubated under reduced UVB (t-test, P < 0.001, Table 1). The probability that the difference in NCP between quartz and glass bottles was obtained by change was, for each individual experiment, less than 0.0001 (t-test, Table 1), and the combined probability that the difference was obtained by chance was, when the experiments were taken in concert, less than 10-6 (Fisher's χ2 test, Fisher 1925). Four of the experiments conducted showed a significant decrease in NCP in the presence of UVB radiation (t-test, P < 0.001). However, one of five experiment, that conducted at the*

northernmost station, yielded an increase in NCP to 6.75 ± 0.03 mmol O2 m-3 d-1 in the presence of UVB radiation compared to 3.48 ± 0.02 mmol O2 m-3 d-1 when UVB was removed (Fig. 3, Table 1). Yet, there was a strong, statistically significant overall combined probability for NCP to decline significantly in the presence of UVB (Fisher's χ2 test, p < 10-6, Fisher 1925), involving a median reduction of NCP by 0.45 mmol O2 m-3 d-1."

Reviewer # 2: As reviewer 1 points out the inhibitory effect of UV could be either way, positive or negative. This is not a good basis for a study that could have the potential to initiate follow up investigations.

Authors: The experiments conducted demonstrated that UVR affects the net plankton metabolism, an such effect has not been tested, nor demonstrated previously. In addition, most measurements made previously on surface planktonic metabolism used glass bottles, which is known that removes UVB. Since UVB is part of the solar radiation the previous net community production data are not exact measurements. Primary production measurements have been evaluated this effect before, experiments revealed inhibition or not, depending on water masses or communities. So, testing the generality of our findings in the ocean is a need, even independently of our results.

Reviewer # 2: The PUV usually is reliable instrument, however, as with any other radiometer the measurements depend on its calibration. There is no mention of it in the text.

Authors: We agree. The PUV 2500 radiometer was calibrated by the Biospherical company in California before the cruise, and after that send to the research vessel when departing from Spain.
Action: We have included information on the calibration in the revised version of the manuscript.

Reviewer # 2: I also miss a discussion of the irradiance data comparing it to other environments.

Authors: We agree, although there are few measurements yet published on underwater UVR.

Action: The revised version of the discussion now compares the underwater UVR data presented here with that reported by Morel et al. (2007) and by Huovinen and Gomez (2011), both in the South Pacific. The text now reads: "*The results presented showed increased UVB penetration towards the Equator along the Humboldt Current System. The penetration of UVB in the waters sampled was intermediate between the extreme transparency to UVB of the ultraoligotrophic waters in the South Pacific Gyre, where UVB was reported to penetrate down to 150 m (Morel et al 2007), and the turbid waters of the Chilean littoral (Huovinen and Gomez 2011). The later display extinction coefficients in the UVB range between 1.71 and 1.31 m-1 (Huovinen and Gomez 2011), well above the values we observed (0.47 - 0.23 m-1)*".

Reviewer # 2: It would have added to the story to separate the effects of UVB and UVA irradiation, to see the impact of the different fractions.

Authors: We appreciate and understand this point. However, our paper does not address UVA, because, contrary to UVB, UVA is not filtered out by borosilicate. Hence, while understanding the effects of the different wavelengths of solar radiation is of fundamental importance, our contributions focusses on the effects of excluding UVB on the estimates of NCP.

Detailed comments

Method:

Reviewer # 2: Why were UV experiments not also conducted in the Patagonian channels?

Authors: The experiments were not conducted in the channels due to the logistics and political limitations of the research cruise. The research ship could not stop in the Patagonian channels for safe navigation. Therefore, it was possible to conduct experiments only when the ship was in Open Ocean.

Reviewer # 2: page 5831, line 18 onwards: I suggest to mention the calibration date and if possible give an indication on the quality of the measurements.

Authors: We have detail on the quality of the measurements. The instrument was calibrated a few months before the cruise and then loaded in the research vessel. We did not right down the calibration date of the instrument.

Action: The text now reads: "*Underwater UVR and PAR profiles were obtained at the 5 oceanic stations around noon using a calibrated PUV-2500 profiling radiometer (Biospherical Instruments) which measures UVR at 6 wavelengths: 305, 313, 320, 340, 380, 395 nm, with 10 nm Full-Width Half-Maximum (FWHM) standard, except 305 (controlled by atmospheric ozone cut off). The PUV-2500 detectors incorporate interference filters that, in combination with UV-passing/light-blocking filters and sensitive silicon photodiodes, minimize errors associated with spectral leakage, resulting in an error of the irradiance measurements of 10-5 µW cm-2 nm-1*.".

Reviewer # 2: page 5832, line 1 onwards: Maybe I missed it but what has been really done with the light absorption measurements and how do they contribute to the findings?

Authors: We have revised the methods section to add clarity in this aspect. The light absorbed by suspended particles, which is one of the major components of the light extinction coefficient underwater, was analyzed in the study, and the spectral properties of phytoplankton light absorption in the UVR bands was examined to check for the presence of UVR protective pigments. We found light absorption peaks in the UVR bands of the spectral absorption coefficients of phytoplankton, indicating the presence of UVB stress and the need of photoprotection.

Action: The manuscript now reads: "*The light absorption coefficients spectra of the phytoplanktonic component was examined in the UVR bands to search for peaks of photoprotective substances, indicative of UVB stress.*"

Results:

Reviewer # 2: I miss the presentation of the pCO2 data which is shown in the table but not in the text.

Authors: In the new manuscript the pCO2 data is presented and discussed the pCO2 data in detail. Further details on pCO2 along the cruise track are reported in Torres et al. (2011), now cited in the text.

Discussion:

Reviewer # 2: page 5837, line 8-9: I suggest to indicate the station numbers also in the table.

Authors: The number of stations will be included in Table 1, as suggested.

Reviewer # 2: page 5837, line 10-15: I am not sure whether this statement reflects the authors finding or the one of the citations at the end.

Authors: To avoid further misunderstandings we have re-written the sentence in the revised version of manuscript.

Reviewer # 2: page 5837, line 17-19: I would find it useful to present the data of other authors (in this case the average of Robinson and Williams measurements) if it is used to compare to the own data.

Authors: Unfortunately, Robison and Williams report a frequency distribution but not the average. We now cite also studies in the region.

Action: The text now reads: *he metabolic rates observed were within the range of values reported for the ocean (Robinson and Williams 2005) and with recently reported values for the Southeast Pacific off Chile (e.g. Daneri et al. 2011, Jacobs et al. 2011), and indicated that the area sampled was not exceptionally productive at the time of the study... Indeed, communities towards the norther range of the area covered have been reported to oscillate between autotrophic and heterotrophic phases (Jacobs et al. 2011).*".

Reviewer # 2: page 5839, line 2-6: Is there a citation that measured the troposheric ozone destruction in the southern hemisphere?

Authors: We agree that a reference is needed.

Action: We now refer to: Madronich 1993, Weatherhead, E. C. and Andersen, S. 2006, Atkinson RJ (1997), in the introduction.

Technical issues:

Reviewer # 2: page 5836, line 23-24: There are suddenly commas in the numbers instead of dots to indicate decimal points.

Authors: This inconsistency between commas and dots has been corrected as you suggest.

Actions taken to accommodate the comments of Referee #3

Thank you for the useful and constructive opinions and recommendations on our paper entitled "Impacts of UV radiation on plankton community metabolism along the Humboldt Current System". We have spared to effort to address the comments received and hope that the resulting manuscript is acceptable to you.

General and overall comment

We are very grateful for the thoughtful comments of all three referees and have sought to address them in the best way we can. We believe the paper has substantially improved thanks to key points raised in the recommendations. We acknowledge that the first draft of the paper lacked a clear and focused standpoint with respect to the broad theoretical discussion on effects of UV on planktonic communities. The new version includes more explicitly the recognition of other studies and approaches. We included new references, following the suggestion of the reviewer, and sharpened all sections of the manuscript. As a consequence of the significant changes in the previous sections outlined above, we have re-written the introduction and discussion to make sure that the conclusions are supported by data and not to overestimate its significance. We trust that the changes made help bringing more clarity to the reader

Specific comments.-

Reviewer # 3: System definition. The Humboldt Current System extends from ~42 S up to about the equator (e.g. Carr 2002, Montecino et al. 2005, Thiel 2007, Quiñones et al. 2010). The southern border of the Humboldt Current System is related to the WestWind Drift Current (WWDC), which corresponds to the meridional edge of the subtropical gyre. In fact the WWDC constitutes the origin of the northward Chile–Peru Current and the southward Cape Horn Current (Reid 1965, Wyrtki 1965, 1975, Silva and Neshyba 1977, 1979). In other words, in strict sense, this paper is not only about the Humboldt Current System but also the Cape Horn Current (Patagonia). Five of the eight sampling stations are located in the Humboldt Current System and three stations on the southern Patagonian fjords. In consequence the title of the paper should be modified, not referring to the Humboldt Current System but to the eastern South Pacific off Chile.

Authors: We appreciate this observation and agree with the reviewer.
Action: The Title and Introduction have been corrected accordingly. We now better describe the study region and incorporate more information on the measurements and methods performed at each station to avoid confusion of the reader. The title has been changed to "Experimental assessment of the effect of UVB radiation on plankton community metabolism along the Southeastern Pacific off Chile" ; the introduction section indicates "*The eastern South Pacific off Chile is an area of particular interest for evaluate the potential effects of solar ultraviolet radiation (UVR 280-400 nm)..... The high productivity of the marine ecosystem in the Southeastern Pacific off Chile has been related mostly to physical factors such as upwelling, river runoff, Kelvin wave associated mixing events, mesoscale structures, and topography (Alheit & Bernal 1993, Quiñones & Montes 2001, Hormazabal et al. 2004, Jacob et al. 2011)*.",

and the methods section reads "*The study was conducted on the Humbold-2009 cruise on board RV Hespérides from 5 to 15 March 2009. The cruise track followed the South eastern Pacific off Chile, starting in the Patagonia channels (54.80º S) proceeding North along the Humboldt Current until the proximity of Antofagasta (Chile, 23.85º S; Fig 1).*".

Reviewer # 3: Introduction. The authors focus a major part of the introduction in justifying the importance of conducting their research based on examples about the penetration of UV in the oligotrophic ocean: "For instance, UVB levels sufficient to cause mortality of photosynthetic plankton have been reported to penetrate as deep as 150m in the "clearest" natural waters on the south pacific gyre (Morel et al., 2007), 60m in the subtropical Atlantic (Llabres and Agusti, 2006) and to 26m in the Mediterranean Sea (Llabres et al., 2010)". The Humboldt Current System is the most productive ocean system of the world and their sampling stations are coastal, therefore it does not make any sense to provide examples from some of the most oligotrophic waters of the world. They should use examples from Chile and from other upwelling and fjord systems of the world. For instance, they should refer to: Cabrera et al 1995, Cabrera & Fuenzalida 1999, Lovengreen et al. 2000, Montecino et al. 2004, Lovengreen et al 2005, Huovinen et al. 2006, Hernandez et al. 2006, 2007, 2011).

Authors: We are grateful for this comment and the suggestions. Certainly our manuscript lacked specific references to the area sampled. These elements of the introduction elaborated on the state-of-the-art on UV penetration in the ocean, but nowhere was there an expectation that the same penetration depths would apply to our study. The reference to the oceans clearest waters, was included because those waters were only discovered very recently (2007) and the results are of great importance, demonstrating that UVR penetrates deeper in the oceans than indicated by laboratory measurements and theoretical models predicted. However, the references of Cabrera et al 1995, Cabrera & Fuenzalida 1999, Lovengreen et al. 2000, Lovengreen et al 2005, and Hernandez et al. 2011, while very interesting refer to studies measuring incident solar radiation in Chile, based on measurements on land. Hernandez et al. 2006, 2007, repored results on experimental tests of the effect of UVR on coastal bacterioplankton, but not UV penetration depths. Only the work of Huovinen and Gomez 2011 (that the reviewer cites below), reported measurements of underwater UVR, but these were in the littoral of Chile. Certainly, our manuscript is reporting novel underwater measurements of UVR in the area, demostrating greater transparency than the data reporter by Huovinen and Gomez 2011.
Action: In the revised manuscript, the Introduction was re-written to sharpen the arguments on our standpoint and include relevant references reporting underwater UV penetration in the studied area. The text now reads: "*The results presented showed increased UVB penetration towards the Equator along the Humboldt Current System. The penetration of UVB in the waters sampled was intermediate between the extreme transparency to UVB of the ultraoligotrophic waters in the South Pacific Gyre, where UVB was reported to penetrate down to 150 m (Morel et al 2007), and the turbid waters of the Chilean littoral (Huovinen and Gomez 2011). The later display extinction coefficients in the UVB range between 1.71 and 1.31 m^{-1} (Huovinen and Gomez 2011), well above the values we observed (0.47 - 0.23 m^{-1}).*".

Reviewer # 3: Introduction. Taking into account the subject and location of this paper, the previous work conducted by Hernandez et al on the effect of UV on prokaryotes in the Humboldt Current System must be cited (Hernandez et al. 2006, 2007, 2011). Science advances generating knowledge in a synergic manner based on the knowledge produced by other present and past scientists. Recognizing other scientists work is a need and ethical imperative.

Authors: We are grateful for this comment. We acknowledge that the original version of the manuscript missed relevant references, which of course was not intentional nor implied any ethical miss behaviour from our side. We agree that science advances generating knowledge in a synergic manner based on the knowledge produced by present and past scientists. We also agree that recognizing other scientists' work is necessary and an ethical imperative, as you suggest.

Action: We now refer to Hernandez et al. 2006, and 2007 work on heterotrophic bacteria in the region. The text now reads:"*Experiments conducted in productive waters in the upwelling zone (36 ºS) showed deleterious effects of ambient levels of UVB on production, size spectra and DNA structure of bacterioplankton communities with contrasting photobiological histories (Hernández et al. 2006, 2007*). "

Reviewer # 3: Introduction. The author stated ". . . where the presence of a shallow oxygen minimum zone limits the capacity of marine biota to find refuge from UV at depth. . .". Although this is true for many organisms there are many species in the Humboldt Current System very well adapted to low oxygen conditions (e.g. Judkins 1980, Ulloa et al. 2000, Gonzalez et al 2000, Gonzalez and Quiñones 2002, Sellanes et al 2007). The evolution of many species in the Humboldt Current System have been strongly influenced by the presence of the oxygen minimum zone.

Authors: We appreciate this comment and we agree on the general idea.
Action: We carefully incorporate this issue within the context of study, in the new version of the manuscript. The text now reads: "*The assessment of UV impacts of community metabolism may be particularly relevant for the Humboldt Current System, along the Chilean coast, one of the most productive regions in the world (Thiel et al. 2009), where the presence of a shallow oxygen minimum zone limits the capacity of marine biota to find refuge from UV at depth, which may be possible for specially-adapted organisms present in the region (e.g. González and Quiñones 2002)."*

Reviewer # 3: Methods. The definition of the study zone should be re-defined. Only a portion of the track (5 stations) is located in the Humboldt Current System (i.e northern than 42 S).

Authors: We appreciate this observation.
Action: The definition of the study zone has now been re-defined in the revised version of the manuscript as requested, including the title (see above).

Reviewer # 3: Methods. What is the number of replicates for the seston samples?.

Authors: There were not a specific sampling of seston during the study. We used a CTD-Rossette systems, with 24 Niskin Botles, and 30 L Niskin Botles for upper surface water.
Action: The text was now corrected to increase clarity: "*Light absorption properties of phytoplankton from the surface waters were measured in samples from the Niskin bottles in the oceanic stations. Seston was concentrated by filtering a variable volume of water (0.5 to 2.5 l, depending on particles concentration) through 2.5 cm Whatman GF/F filters. Light absorption by particles concentrated on the filters was measured immediately after collection. The optical density of the filters (ODf) was measured in a dual beam scanning spectrophotometer (Shimadzu UV-2100) using a clean, water saturated Whatman GF/F filter as a blank. The wet filters are placed in front of the photomultiplier and the clean, water-saturated filter used as a reference blank through baseline corrections. ODf was measured at 1 nm intervals between 280 and 750 nm, covering the UV-B, A and PAR spectra.* "

Reviewer # 3: Methods. It is a major weakness of the paper that no information is available regarding the oxygen distribution in the water column of the sampling stations. Oxygen content is a key variable in the Humboldt Current System and the oxycline could have affected the measurement especially those located in northern Chile. For instance Eissler and Quiñones (1999) found a positive significant correlation between respiration and dissolved oxygen concentration off northern Chile (Antofagasta, 23 S). Therefore caution should be exerted in interpreting and extrapolating the results because the authors do not have (or report) information on one of the most crucial variables that could have affected their results.

Authors: Our study was not performed in coastal waters. The depth of the stations occupied was > 3318 m (up to 5234 m). As a consequence the minimum oxygen was found between 300 to 500 m depth, well below the photic layer so not affecting our measurements. Since our measurements of NCP, R and GPP are based in changes in the oxygen consumption and production, we can indeed provide all the information suggested by the reviewer.

Action: We now report information on oxygen concentration and the depth and position of the stations of the manuscript (see Tables 1 and 2).

Reviewer # 3: Methods. The bottles for "light" were incubated on deck at in situ temperature. What is the range of variation in temperature during the incubation process? How were the dark bottles incubated? Was the temperature controlled? It is well know that temperature is a critical factor in determining metabolic rates.

Authors: Dark and light bottles were incubated together.

Action: In the new version of the manuscript we included more information on the temperature during the incubation process and also more detail on the experimental design. The temperature was controlled in the incubator on deck to $< \pm 1$ º C from the *in situ* value.

Reviewer # 3: Methods. The standard error of the in situ respiration and primary production rates should be reported.

Authors: The standard error of the in situ respiration and primary production rates was indeed reported in Table 1 and figure 4.

Reviewer # 3: Methods. The authors stated "In contrast, the quartz bottles allow the entire light spectrum to reach the sample. Hence, the difference in oxygen evolution between the quartz and borosilicate bottles represents the effect of UV radiation on NCP (i.e. the net result of impacts on both GPP and R)". This statement is not totally correct. It has been proven that some quartz bottles absorbed about 10% of UV-B and about 50% of radiation at 200 nm (e.g. Bühlmann et al. 1987, Köhler et al 2001). Did you measure the UV-B and UV-A absorption by your quartz bottles? How your measurements or estimate of UV absorption by the Quartz bottles affected your results? The authors should include a graph with the transmission spectra (in %) of the quartz and glass bottles used.

Authors: Yes, we did measure the % transmittance of the glass and quartz materials used in the incubations. Both quartz and glass removed a similar percentage (around 10%) at the PAR bands, but differences were larger at the UVA and UVB bands. In any case, our measurements of the spectral properties of the materials do not included UVC bands, since UVC do not penetrate underwater. Solar radiation at the bands indicated by the referee (200 nm) are completely absorved by the upper atmosphere, and by the water, and do penetrate underwater.

Action: We have included the information requested by the reviewer in the revised version of the mansucript, as a new Figure 2. The text now reads:"Evaluation in a dual beam spectrophotometer of the transmittance of the materials involved showed that borosilicate bottles removed 15 % of the solar radiation in the blue range, with the transmittance decreasing sharply with decreasing wavelength to remove all of the radiation at wavelengths shorter than 280 nm, while quartz bottles removed about 10 % of radiation in the blue spectra, with only a slight decrease in transmittance (< 3) with decreasing wavelength (Fig. 2)."

Reviewer # 3: Methods. Clarify if all your stations are on the continental shelf. Provide total depth of the water column.

Authors: Apart the samples from the Patagonian channels, none of our stations were on on the continental shelf, which was particularly narrow and close to the continent in the Pacific area sampled, with all stations located deeper than 3,000 m and one of our stations having more than 5000 m depth.
Action: Information on the position and depth of the stations has been now included in the revised manuscript (cf. Tables 1 and 2).

Reviewer # 3: Results. Salinity measurements should be provided especially in the case of the experiments conducted in the fjords. It is known that at 5 m depth the influence of freshwater and dissolved organic matter can be substantial and, obviously, influence the results and how representative of the whole fjord area they could be.

Authors: We agree.
Action: The salinity has been now included in the revised manuscript (cf. Tables 1 and 2).

Reviewer # 3: Results. It is necessary to include the quartz and borosilicate transparence spectrum of the bottles used.
Authors: We agree.
Action: We have included the information requested by the reviewer in the revised version of the mansucript, as a new Figure 2, see above.

Reviewer # 3: Results. Data on cloudiness, wind speed and ozone concentration for each experiment should be reported.

Authors: Unfortunately**,** data on cloudiness and ozone concentration are not available.
Action: Data on wind speed has been now included in the revised manuscript (cf. Tables 1 and 2).

Reviewer # 3: Results. The authors claim that exposure to UVB gives lower rates of NCP than in its absence based on their results of 4 out of 5 cases. What is the statistical significance of this result? Can it support one of the key "findings" of the manuscript?

Authors: Indeed, the differences in NCP between treatments including and excluding UVB were statistically significant at $P < 0.0001$ for each of the five experiments (t-test). A reduction in NCP in the presence of UVB was observed in 4 out of 5 experiments. Provided the experiments were independent of one another, it is possible to aggregate the P values for the individual t-test to derive a combined probability that the overall tendency for differences being observed was derived by change. The combined probability is 0 (i.e. < 10^-6; Fisher $\chi 2$ test, Fisher 1925). Using the same procedure the combined probability for the one sided test testing for NCP being lower in the presence of UVB is also highly significant (i.e. < 10^-6; Fisher χ^2 test, Fisher 1925), so the likelihood that the results obtained are derived by change is statistically negligible.

Action: The text now address these issues. We now report that all of the experiments conducted showed a significantly different NCP between glass and quartz incubations. The results now reads: "*All five experiments comparing NCP rates derived from incubations in glass and quartz bottles showed significantly different NCP rates between samples incubated under the full in situ solar radiation and those incubated under reduced UVB (t-test, P < 0.001, Table 1). The probability that the difference in NCP between quartz and glass bottles was obtained by change was, for each individual experiment, less than 0.0001 (t-test, Table 1), and the combined probability that the difference was obtained by chance was, when the experiments were taken in concert, less than 10-6 (Fisher's χ2 test, Fisher 1925). Four of the experiments conducted showed a significant decrease in NCP in the presence of UVB radiation (t-test, P < 0.001). However, one of five experiment, that conducted at the northernmost station, yielded an increase in NCP to 6.75 ± 0.03 mmol O2 m-3 d-1 in the presence of UVB radiation compared to 3.48 ± 0.02 mmol O2 m-3 d-1 when UVB was removed (Fig. 3, Table 1). Yet, there was a strong, statistically significant overall combined probability for NCP to decline significantly in the presence of UVB*

(Fisher's χ2 test, p < 10-6, Fisher 1925), involving a median reduction of NCP by 0.45 mmol O2 m-3 d-1."

Reviewer # 3: Discussion. The authors stated: "The waters sampled were not particularly transparent to UVB compared to the ultraoligotrophic waters in the South Pacific Gyre, where UVB was reported to penetrate down to 150m (Morel et al., 2007)." It does not make any sense to compare their results with oligotrophic waters. They should compare their results with previous estimates from the Patagonian fjords and from the Humboldt Current System (e.g. Hernandez et al. 2006, 1007, 2011, Montecino et al. 2004) and from other fjords and upwelling ecosystems worldwide.

Authors: We appreciate the important comment. Some of the references cited do not report underwater UVB levels, so comparisons were not possible. We have compared our results with those reported of Huovinen and Gomez (2011), which is an important reference. We agree that contains very valuable data, but the reviewer would understand that this publication (Huovinen and Gomez, 2011), is very recent, published after we submitted our manuscript for review, explaining way it was unnoticed in our discussion and reference list. In any case, our measurements indicated a much larger transparency to UVB in the waters we sampled, with Kd values between 0.47 - 0.23 m-1, than that in the area sampled by Huovinen and Gomez (2011), reporting much higher Kd values between 1.71 and 1.31 m-1, indicative of very turbid waters. We have examined the other references indicated by the reviewer, but none reported data on UVR penetration and water transparency.
Action: We now compare underwater UVB penetration with the values reported by Huovinen and Gomez (2011). The text now reads "*The results presented showed increased UVB penetration towards the Equator along the Humboldt Current System. The penetration of UVB in the waters sampled was intermediate between the extreme transparency to UVB of the ultraoligotrophic waters in the South Pacific Gyre, where UVB was reported to penetrate down to 150 m (Morel et al 2007), and the turbid waters of the Chilean littoral (Huovinen and Gomez 2011). The later display extinction coefficients in the UVB range between 1.71 and 1.31 m-1 (Huovinen and Gomez 2011), well above the values we observed (0.47 - 0.23 m-1).*".

Reviewer # 3: Discussion. "The metabolic rates observed were within average values reported for the ocean (Robinson and Williams, 2005), and indicated that the area sampled was not exceptionally productive at the time of the study". This is a careless use of the information available for the Humboldt Current System regarding net community metabolism with several papers published after Robinson and Williams (2005). The authors should compare, at least, their quite restrictive temporal experimental coverage with the results of the following papers: Gonzalez et al. (1998), Eissler and Quiñones (1999), Daneri et al. (2000), Montecino et al. (2004), Montero et al. (2007), Jacob et al. (2011), Daneri et al. (2011), Iriarte et al. (2011).

Authors: An experimental assessment there is always a trade-off between spatial and temporal scales. We are grateful for this comment. The paper of Robinson and Williams, 2005, however, is the largest meta-analysis on plankton metabolism yet available, so it is appropriate comparison. Some of the references cited by the reviewer do not allow comparison as primary production was measured using C14, which cannot be compared to O2-based GPP or NCP measurements.

Action: We have revised the manuscript to provide a broader framework for comparison, including some of the papers cited by the reviewer, where appropriate (i.e. when the rates reported could be compared with those we report). The text now reads "*The metabolic rates observed were within the range of values reported for the ocean (Robinson and Williams 2005) and with recently reported values for the Southeast Pacific off Chile (e.g. Daneri et al. 2011, Jacobs et al. 2011), and indicated that the area sampled was not exceptionally productive at the time of the study... Indeed, communities towards the norther range of the area covered have been reported to oscillate between autotrophic and heterotrophic phases (Jacobs et al. 2011).*".

Reviewer # 3: Discussion. There are several factors, in addition to UV radiation, that could affect the balance between primary production and respiration in the Humboldt Current System. The microbial community inhabiting the mixed layer of the coastal zone off northern Chile is subjected to marked trophic gradients triggered by synoptic-scale shifts in the upwelling driven by wind stress. Drastic changes in the wind-stress direction and hydrographical and chemical characteristics of the mixed layer influenced the phytoplankton size structure, primary production rates and net ecosystem metabolism. I strongly recommend the authors to read and incorporate in the discussion the findings of the paper by Jacob et al. (2011) entitled: "Community metabolism, phytoplankton size structure and heterotrophic prokaryote production in a highly productive upwelling zone off northern Chile" (Marine Ecology Progress Series 430: 23–34). They should also analyze deeply the paper by Daneri et al. (2011).

Authors: We appreciate and understand this recommended. The manuscripts cited are rather recent and were not available at the time of our submission. We note that the paper by Jacob et al. 2011, refers to an area north of our study area, but it is indeed highly relevant and we now refer to the findings reported therein.

Action: In the new version we incorporate the references cited in a broader discussion on controls of planktonic metabolism in the region. The text now reads "*The metabolic rates observed were within the range of values reported for the ocean (Robinson and Williams 2005) and with recently reported values for the Southeast Pacific off Chile (e.g. Daneri et al. 2011, Jacobs et al. 2011), and indicated that the area sampled was not exceptionally productive at the time of the study... Indeed, communities towards the norther range of the area covered have been reported to oscillate between autotrophic and heterotrophic phases (Jacobs et al. 2011).*".

Reviewer # 3: Discussion. The possible effect of different types of radiation on organic matter and/or other compounds should be incorporated in the analysis. For instance, there is a significant relationship between wavelength band and the production of H2O2, which is a potential toxicant and it can affect the distribution and redox-chemistry of biologically active metals such as iron, copper and manganese. The photochemical production of H2O2, has been demonstrated in other upwelling systems such as the Benguela (e.g. Gerringa et al. 2004). Moreover, it has been shown that phototransformation of DOM significantly alters both bacterial metabolism and community structure in surface water for a variety of coastal ecosystems (e.g. Abboudi et al. 2008).

Authors: We agree.
Action: We have revised the mansucript to include this aspect in the discussion of the revised version of the mansucript. The text now reads "*Our experiments did not allow evaluate the effect of UVB on respiration vs. that on GPP, as R was measured in the dark, although there is evidence that UV radiation enhances R (Ekelund 2000) and suppresses GPP (Holm-Hansen et al 1993). UVB radiation affects planktonic production by reducting planktonic photosynthetic rates by 15 % per unit biomass (Cullen and Neale, 1994) and by reducing the biomass of photosynthetic plankton due to increased cell mortality rates in the presence of UVB radiation (Llabrés and Agustí, 2006, 2010, Agustí and Llabrés, 2007). UVB radiation also affects planktonic respiration by impacting on microheterotrophs (Hernández et al. 2006, 2007) and photochemical reactions conducive to changes in the availability of dissolved organic matter (Obernosterer et al., 1999).*".

Reviewer # 3: Discussion. The authors stated: ". . .the communities in surface waters resulted to be, in general, strongly heterotrophic, thereby acting as a CO2 source, consistent with the supersaturation in pCO2 in surface waters driving a CO2 efflux to the atmosphere" and " The use of quartz bottles to allow the UVB component of the irradiance field yields net heterotrophic communities in surface waters, consistent with the supersaturation in pCO2 in surface waters observed along the cruise. Whereas UVB radiation is expected to impact only on the surface waters, it is the metabolism of the communities therein that most directly affects surface pCO2". Their interpretation is a clear overstatement (not only for their extremely low sample size and temporal coverage) and it does not take into account all possible variables affecting surface pCO2. It is known the Humboldt Current System tends to be a source of CO2 to the atmosphere due not only to the biology but also due to the effect of upwelling. I strongly recommend the authors to read the following papers on previous pCO2 measurements in the Humboldt Current System and use them to provide a proper and complete interpretation of their results regarding pCO2 and its possible relation to metabolism: Torres et al. (1999, 2003, 2009, in press), Lefevre et al. (2002), Placencia et al. (2010). Furthermore, upwelling itself is important in determining heterotrophic or autotrophic community metabolism in the Humboldt Current System (Jacob et al. 2011).

Authors: We agree with the reviewer, and have re-written discussion in 85% to make sure that the conclusions are supported by data and to avoid unsupported speculation. We incorporate new reference, as suggested. However, we should clarify that we are not saying that the UVR is the only drivers for planktonic metabolism, we argue that UVB affect the metabolism in a regional scale without taking into account some temporal variability like upwelling.

Action: We have revised the discussion to avoid speculation and add relevant references, following the reviewers advice. The text now reads "*Plankton metabolic rates were generally positive (NCP > 0), indicative of autotrophic communities acting as a CO2 sink. In contrast, there was a prevalence of supersaturation of surface waters in CO2 (Table 1), suggesting that this was driven by physical and thermodynamic factors, rather than biological ones (cf. Torres et al. 2011).*" and have revised the rest of the text accordingly.

Reviewer # 3: Discussion. There is a word missing in the following sentence: "Indeed incubation in glass material removes the incidence of in the water column..."

Authors: We agree..
Action: We replace the sentence with "*Indeed incubation in glass material removes the incidence of UVB radiation into the water column...*"

Reviewer # 3: Discussion. A key "conclusion" of this manuscript is: "Exposure of surface (5 m) communities to UVB radiation, greatly reduced NCP in all but one community and rendered all, except one of the communities investigated heterotrophic. . ." and " "Our results show, therefore, that the penetration of UVB radiation . . . affects the communities located in the upper layers of the water column. In experimental evaluation of the effect of UVB radiation in surface waters, those most strongly affected by UVB, showed that UVB radiation strongly suppressed net community production in most communities, resulting in a dominance of heterotrophic communities in surface waters, compared to the prevalence of autotrophic communities inferred when materials excluding UVB radiation are used for the incubation" It is well known that microbial communities can be negatively affected by regular solar radiation. In fact, Hernandez et al. (2006) have already demonstrated that in the Humboldt Current System there was significant inhibition of prokaryote secondary production by PAR, followed by UV-A and UV-B, especially for surface waters as compared with deeper waters from the Equatorial Subsurface Waters. In addition, Li et al. (2011) showed that from coastal to pelagic surface seawaters, UV-B (280–315 nm) caused similar inhibition, while UV-A (315–400 nm) induced photosynthetic inhibition and increased from coastal to offshore waters. In other words, the species assemblages as well as the DOM content of the specific sites could be very important. In the case of the manuscript of Godoy et al., this should be a clear warning to be extremely cautious in interpreting their results due to the very low sample size.

Authors: We agree that our first version provided a broad conclusion of effects UV on net community productions.
Action: We have revised the text to broaden the discussion and include a more explicitly the recognition of assumptions, limitations, conclusions and speculations.

Reviewer # 3: Discussion. It is necessary to incorporate in the Discussion section the fact that UV radiation could also have positive effects on primary productivity. UV-A enhances carbon ïnˇA¸xation by phytoplankton under reduced (Nilawati et al., 1997; Barbieri et al., 2002) or rapid fluctuation of solar irradiance (Helbling et al., 2003) and allows photo repair of UV-B-induced DNA damage (Buma et al., 2003). Furthermore, the presence of UV-A could result in higher biomass production (Wu et al., 2005).

Authors: We appreciate and understand this point. However, our paper does not address UVA, because, contrary to UVB, UVA is not filtered out by borosilicate. Hence, while understanding the effects of the different wavelengths of solar radiation is of fundamental importance, our contributions focusses on the effects of excluding UVB on the estimates of NCP.

Reviewer # 3: Discussion. It is stated: "Our results show, therefore, that the penetration of UVB radiation increases towards the Equator along the Humboldt Current System. . .". The number of stations (5 stations) and the temporal cover (less than 12 days) in the Humboldt Current System do not allow any generalization regarding trends in the penetration of the UVB in the water column.

Authors: We appreciate the important comment. In the new manuscript we avoid any inference at the level of the whole system. We clarified our approach and its limitations in the manuscript and re-wrote the Discussion accordingly.
Action: We now acknowledge openly the limitations of the study.

Reviewer # 3: Discussion. The authors state: "These results show that UVB radiation, which has been increased greatly in the study area due to trophosperic ozone destruction, may have suppressed net community production of the plankton communities in the study area, possibly driving plankton communities in the Southwest Pacific toward CO2 sources." This is pure speculation. The temporal and spatial coverage (8 stations, mostly 1 depth) is absolutely inadequate to support this generalization. This speculation gets even worse by the fact that the authors do not review any of the relevant available literature on the Humboldt Current System and the Patagonian Fjords.

Authors: We agree. In the new version we put a lot of effort in using more precise language, presenting examples and providing references to avoid unnecessary generalizations and speculations throughout the paper (also see early comments).
Action: We have re-written the discussion to avoid speculation.

Reviewer # 3: Discussion. Regarding the production and metabolism of the Patagonian fjord key information should be incorporated such as Gonzalez et al. (2010). Regarding UV in southern Chile, the paper by Huovinen and Gómez (2011) should be incorporated.

Authors: We are grateful for this comment which led to important changes in the discussion on revised manuscript. We strongly appreciated the reference of Huovinen and Gómez (2011), which contained relevant data to our study, and could not be included in the original submission as it was published after we submitted our paper.