

Interactive comment on “Chemolithoautotrophic production mediating the cycling of the greenhouses gases N₂O and CH₄ in an upwelling ecosystem” by L. Farías et al.

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Review of bg-2009-112

This manuscript titled “Chemolithoautotrophic production mediating the cycling of the greenhouse gases N₂O and CH₄” by Laura Farias and co-authors contains a huge dataset of generally very high quality. The data is a mixture of monitoring data (time series of nutrients, oxygen and nitrous oxide for the last 7 years and methane for the last 2 years), as well as experimental data based on ¹³DIC assimilation under various conditions (dark/light, N₂O, CH₄) with ATU and GC7 as inhibitors for ammonium/methane oxidation and archaea, respectively. Based on the experimental results they report very

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high carbon assimilation rates under dark conditions, among the highest ever reported, and the dark fixation (chemolithotrophic carbon assimilation) is making up a considerable part of the total primary production. They mainly attribute this dark fixation to the chemoautotrophic activity of aerobic ammonium oxidizing (AAO),- and methane oxidizing bacteria (AMO).

1. Does the paper address relevant scientific questions within the scope of BG? Yes
2. Does the paper present novel concepts, ideas, tools, or data? Yes
3. Are substantial conclusions reached? Yes
4. Are the scientific methods and assumptions valid and clearly outlined? No
5. Are the results sufficient to support the interpretations and conclusions? Partly
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes
8. Does the title clearly reflect the contents of the paper? Yes
9. Does the abstract provide a concise and complete summary? Yes
10. Is the overall presentation well structured and clear? No
11. Is the language fluent and precise? No
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes
14. Are the number and quality of references appropriate? No

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15. Is the amount and quality of supplementary material appropriate? A considerable part of the data could be moved to the supplementary material.

General comment:

As mentioned above the dataset is impressive and only the monitoring data alone with the flux calculations would be enough to make a very interesting publication. However the authors have not quite succeeded in combining this with the considerable amount of experimental data as well as depth profiles into a clear story with well founded conclusions. It is not at all easy to treat such a huge complex dataset which is probably the reason why scientists so seldom try to use oceanographic monitoring data to upscale the results from their experiments. In total there are 7 main figures with a total of 21 panels and 5 Tables with upto 325 data cells each, dealing with physical and chemical oceanographic parameters, inventory calculation for the fixed nitrogen species, CH₄ and N₂O gas analysis on both experiments as well as monitoring, air-sea flux calculations of CH₄ and N₂O, ¹³DIC assimilation experiments and natural abundance ¹³C isotopes on bulk POM. Although the graphic presentation is good and most numbers presented in the tables seems okay, it took me a lot of effort time to overview the dataset and partly because of the size of the tables it takes a lot of time to read and follow the data references in the result and discussion part. Still after reading it partly 3 times I am not sure if I see the same in the tables as what they describe and discuss. The rather large amount of abbreviations (O-L, B-L, S-L, B-Layer, S-Layer, additional to the more common ones like AAO, AMO, CA, ATU, OM, POC, GC7,) does not ease the understanding, i.e. is there a difference between B-L and B Layer and does n.d. means no data or not detected. In the end most of the information in all of these data get lost and in the summary there are rather vague statements without any specific numbers or estimates (more like the abstract), whereby the most specific conclusion I cannot really see in the data (see below). I think this is a very important study based on an impressive dataset, but needs to be restructured where the most important data gets more visible and the statements in the conclusions/summary more defined.

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The statements in the abstract - The conclusion about the importance of dark assimilation of inorganic Carbon, chemolithoautotrophic assimilation, in an upwelling area is well founded. However, is the mentioned 27 % the average of the percentage dark fixation or the percentage of the integrated total assimilation (i.e. if it makes up 88-98% when total production is 0.5 mg/d/m² and 2-4% when production is 50mg/d/m², the average % can be about 27% but the total contribution of dark fixation might be 5%). This is a very important finding and needs to be clarified and probably both numbers should be in.

-the argument about the chemoautotrophic assimilation causing the lighter d¹³C values at the oxycline (O-Layer or O-L) is interesting, but should not then this be the zone of the highest dark fixation? According to the dark assimilation rates in Table 4 then the higher rates are found in the bottom waters (B-L, B-Layer).

-In short the authors state that: Dark CA was reduced by 27-48% by ATU (AAO inhibitor) and 24-76% by GC7 (archaea inhibitor)! By looking at the results presented in Table 4 I do not see this, i.e. 3.53 ± 1.4 is not significantly different from 2.54 (no error range) and even using the average values I would get lower % values. There is a significant difference by the addition of GC7 which is interesting but looking further down the table the addition of GC7 increases the CH₄ consumption considerably, which is not contradicting the statement that the inhibited Archaea are gaining their energy from CH₄ oxidation (AMO).

-A similar problem occurs with the statement that AAO produced N₂O at a rate of 8.88-43.85 nMd⁻¹. The lower estimate (8.88) I can find as the net N₂O production without any inhibitor and the max (43.85) is not appearing in the table. I do not see why the net rate is taken as AAO activity, because in most of the experiments the N₂O production do not significantly change by the addition of ATU. Why is all other sources of N₂O excluded? In the detailed discussion about this (p29 line 1-4) aerobic denitrification (Kuenen et al) as well as potential N₂O production by the anammox reaction (Kartal et al) is not included. Moreover, using the authors CA attributed to AAO

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(27-48% of dark CA) to calculate the Ammonium consumption would give ammonium oxidation rates in the range of 0.2-3 μ M NH₄ d⁻¹ using a conservative conversion factor of 1mgC /mol NH₄ (Feliatra and Bianchini 1993, Buttinini et al 2000), with an average \sim 1 μ M/day which is well above the values from Lipschulz et al (1990), Ward and Lipschulz (1989) and Lam et al (2008) from Peru.

The above mentioned concerns need to be addressed properly before the statement that NH₄ and CH₄ oxidation is the main driver of the Dark CA or not. At the moment this is not convincing and the dark CA in it self as well as the flux of the climate gases N₂O and CH₄ might be two more interesting results to focus on

Suggestions:

-the English is generally well understandable, but the many sentences would be easier to understand if they were shortened. This could reduce the length of the manuscript considerably and make it easier to read.

-please split the discussions/descriptions of CH₄ and NH₄ oxidation into separate sections (i.e. page 5, 13, 26, 28), making clear statements before comparing the two processes. - too many digits after the comma. Nobody can measure 22.567 delta 13C, use 22.6, nor is it interesting to know that the measured rate was 13.27 \pm 1.56 (use 13 \pm 1.6). This makes tables and data much easier to read and remember.

-N₂O and CH₄ fluxes vs. time would i.e together with upwelling intensity would be a very informative plot. Maybe integrated (m²) dark and light CA along with this would be a very informative way to plot the data for the discussion.

Some minor comments

-P4 L18-20. please define redoxcline for your study

-P5 L1-5. Could you use available AAO rates from literature (see above) to calculate expected Dark CA from AAO

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-p5 L10.16. Please rephrase and be more specific about the role of the processes.

-p5 L20 delete "performed"

-p8 L11 GFF stands for Glass Fiber Filter

-p8 L14 is 10% accuracy for Nitrate correct?

-p10L2 POCr recovered (spacing)

-p10L10 From which literature? What does this mean for your data presented in tables?

-p11 L5 How were the negative controls tested with HgCl₂. Please rephrase.

-p11 L16-20 Why did you test the bags for leakage looking for trace gases like N₂O and CH₄ when the gas you are actually looking for, Oxygen, is $>$ 1000 more abundant in the atmosphere?

-p12 L2 what volume was the GC bottles (water and headspace)? Do you not lose gas when you "poured" it into the sample bottles?

-p12-14 please skip the abbreviations PLD and MLD (there is enough). How was the PLD determined?

-p14 L8. "In general, it did not match the MLD in Table 1" This sentence does not make any sense to me. Please rephrase.

-p15 L8 The statement "Most of the time, the 22 μ M isobath lies at 60-70 m depth..." This is not at all what I see! Most of the time it is well above or below this zone.

-p15 L9-10. Why is this important to notify?

-p15 L15-16 Not so obvious! Can this be shown statistically (r^2)?

-p16 L1 This is speculation and do not belong in the result chapter.

-p19 L5 "... was around 5.5-8" should be: from 5.5 to 8 (if this is correct) this is anyhow quite a big range and I do not see any arguments why re-suspended matter could play

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a role. I would just leave out the whole C:N discussion

-p16 SD is not a common abbreviation for standard deviation (if this is what is meant)

-p16 L16-18. "The mean. . ." this sentence/statement is very unclear. Please rephrase.

-p16 The section starting at L19 and continues on p17 is very confusing, and it is unclear to me if this is important at all or could be left out.

-p20 L15 What is meant by: "The accumulation of POC in dark. . .experiments" Is it meant that the ^{13}C assimilation to the POC was linear then please rephrase accordingly.

-p20 L17-20 change to ".in monthly in situ. . ." If B-L and B-layer is the same then please be consequent throughout the manuscript(also for S-L etc).

-P21 L2 Please change is with was (there are more of these)

-P21 L4-5 This statement does not make any sense to me at the moment.

L10 this statement needs a reference and some modification i.e the primary production per m^2 is to my knowledge higher in Namibia than Chile and Peru is a more important fishery region than Chile.

L13 What is mean with "matter"?

L14-17 Why can this not be explained by the physical factor controlling the upwelling? This point is very important for the manuscript and needs to be explained very clearly to the reader.

-p22 L1 This needs a reference L5-6 needs a reference

L14 – a total reaction sum of 295.7 KJ m^{-2} do not seem much for the whole 7 year study period

-p24 L1-11 This mixed discussion of findings in the watercolumn and sediments is quite confusing and to my opinion not needed.

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L18-p25L4 Following your arguments should not the lightest ^{13}C come with the highest Dark CA?

-p25 L21 (the reference is not complete)

L2 change closeness with proximity

L18 and L24 Is this these per com. data the ones shown in Table 5? In this case I would suggest to use the numbers in the paper and include the owner of the data in the authorlist or in the acknowledgement.

-P26 L12 are these based on the data mentioned above then they need to be part of this study and not as Pers Comm.

L16 what is the notation 0.005 h^{-1} ?? something missing.

L22 Again, I do not see any dramatic change in table 3?

-p27 L1-3. I do not see any data on the community structure and archaeal C fixers nor any evidence for the system is sensitive to this. Based on the "Pers.Comm" references, there seems to be some information about the community structure available. Could these maybe be included in the manuscript? Any positive evidence in this direction would greatly strengthen this study.

-P29-L8-10 There is a long way from this vague statement here that some of the N_2O might be produced by AAO and the attribution of all N_2O production in the Abstract.

L13-15 This is very speculative.

L15 As far as I can see there are no ammonium oxidation rates in this manuscript! The conversion CA rates attributed to AAO (as suggested above) could be included though.

-P29-30. The summary is quite vague and speculative compared to what is stated in the results and discussion part.