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Biogeosciences Discussions

Interactive comment on "Rapid biological oxidation of methanol in the tropical Atlantic: significance as a microbial carbon source" *by* J. L. Dixon et al.

Anonymous Referee #2

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This study follows a previous work published in the ISME journal ("Microbial methanol uptake in northeast Atlantic waters"; Dixon et al., 2010), about microbial uptake of methanol into particulate biomass, oxidation rates to CO2 and biological turnover time of methanol in temperate North Atlantic waters. In both studies they apply the same methodology to compare uptake and oxidation rates and turnover times of methanol in tropical and temperate North Atlantic waters. In fact, they include the same 3 stations from the Dixon's et al. 2011 study to compare uptake rates into particles. Additionally, they measure bacterial leucine uptake rates and estimate the methanol contribution to bacterial carbon demand (BCD).

The main conclusions of the study are:

1. Measured concentrations of methanol in subtropical North Atlantic waters are up to 300nM (more than 3 times higher than in temperate NA waters), with uptake rates up to 146nM d-1 (about 5 times higher than in temperate NA waters) and turnover time as low as 1 day -extremely low turnover time compared with the lowest turnover time (11 days) estimated for shelf waters in the temperate NA.

2. Methanol contributes on average 13% to BCD in the Central NA Gyre (?!) – with a maximum of 54%

3. Based on air to sea gas exchanges estimations, they conclude that the atmosphere is not a major source for methanol and suggest an "in situ" (as yet unidentified) methanol source.

The paper is well written and concise, although sometimes is difficult (at least for me) to follow the origin of data (i.e. which data correspond to actual measurements and which have been obtained from the literature or averaged from other studies).

My main concern with this work is the great degree of assumptions used to derive their conclusions. To publish this paper, I think the authors should constrain better the uncertainty in their estimates (although then the conclusions might change).

Major comments:

1. Methanol concentrations, and estimates of methanol oxidation to CO2 (E) and uptake rates into particles (G).

Methanol concentrations in seawater were not measured at stations where 14C labelled methanol uptake/oxidation experiments were performed (stations 1, 2 and 3, close to eutrophic-mesotrophic NW Africa coastal transition zone). Since E and G are derived from the product of "k" (apparent rate constant) multiplied by the in situ concentration of methanol, it is necessary to know the latter to have a precise estimate of the rates. Moreover, calculations of turnover time, the ratio E:G and the %Carbon from

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methanol contributing to BCD depend also on the in situ methanol concentration (Table 2). The authors use (for their calculations) a range of values of methanol concentrations derived from in situ measurements in the North Atlantic during the AMT-19 cruise (across the centre of the oligotrophic NA subtropical Gyre). Given the large variability observed in methanol concentrations between regions (Dixon et al 2010, Williams et al., 2004), I understand that the authors cannot extrapolate the open ocean values of the AMT cruise to the near shelf stations of this study. At best they should include a wider range of concentrations, including eutrophic regions too, but in that case I am not sure how transcendent would be the conclusions.

2. Bacterial production (BP), bacterial respiration (BR), bacteria growth efficiency (BGE) and carbon demand (BCD)

BP was calculated using a carbon to leucine conversion factor (CF) of 0.73 kgC mol leu-1. The authors claim that this value represents an average value used in other studies close to their sampling locations. However, the fact is that "the sampling locations" in the tropical NA spans a transition zone from eutrophic to oligotrophic waters. Eutrophic-mesotrophic stations closer to the upwelling (i.e. 1, 2 and 3, used to calculate de E:G ratio) would presumably have a CF >1.5 (Alonso-Saez et al., 2007; see also discussion in del Giorgio et al. 2011; L&O 56, 1-16), whereas the most oligotrophic stations would presumably have a CF closer to 0.2 (Alonso-Saez et al., 2007). Moreover, empirical CFs in temperate waters may vary from <0.5 to >2. This variability –consistently found in coastal-open ocean gradients- should be considered, unless you estimate the CFs for your study taking into account leucine respiration during your experiments (Alonso-Saez et al 2007).

BR –that is used together with BP to calculate BCD- was derived from the general equation of Robinson (2008), relating BP and BR: BR=3.69 BP0.58. This equation explains only 52% of the variance of BR (at a global scale!); hence this must be considered in the final calculations of BCD. Perhaps would be better to derive an equation from published concomitant values of BR and BP from the regions of study, or use a

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range of BR values published from the same region.

BGE- The derived BGE , after several assumptions in BP and BR (see above), are very low (2-4%). I doubt they represent realistic BGE for the whole region of study. From the 3 papers cited to compare with the results of this study (Alonso-Saez et al, 2007, Moran et al 2007 and Robinson 2008), only in the first one BGE is estimated from direct measurements of BP and BR. In the Alonso –Saez et al study (spanning a zonal gradient of productivity across the same sampling region of this study) BGE ranges from 1% to 56% (average >10%), and correlates well with CF.

In summary, I feel the conclusions of this study would be very different, taking into account the uncertainty and variability in the estimates. I believe you cannot apply the same average values of methanol concentration, CF and BR (which were not measured during the cruise!) to all the stations, due to the trophic variability across your sampling regions.

Minor comments

1. P 3901, L 28: "...microbial methanol turnover times of 12-24 days..." Shouldn't it be 11-33 days instead 12-24 (Dixon et al 2011)?

2. Page 3902, Line 1: "...nutrient limited tropical waters..." Were the waters at stations 1,2 and 3 also nutrient limited?

3. P 3902, Section 2.1. Please, include dates for the cruise

4. P 3904, Section 2.4. How do C14 uptake experiments during 6 hours (stations 1-3) compare with experiments from dawn to dusk (stations 7-12)? Did you check for DOC14 excretion?

5. P 3906, 2nd parag. With a single day-night cycle it is difficult to see whether the pattern is reproducible at each station. For instance, it is not evident a rise before dawn at stations 1 and 2. (Notice that the label for station 3 is lacking in the legend)

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6. P 3907, section 3.1.2: “Iberian peninsula”?... It seems to me that the stations are far from the Iberia peninsula.

7. P 3907, L 17-18: “. . .away from the influence of upwelling or continental inputs e.g. dust”. Dust storms cross the Atlantic Ocean and reach the Florida (US) coast. Stations 7-12 are thus potentially under the influence of dust deposition.

8. Table 2. Why station 4 has a range in longitude (16-18W)?

END OF REVIEW

Interactive comment on Biogeosciences Discuss., 8, 3899, 2011.