

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

J. L. Dixon et al.

jod@pml.ac.uk

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Dixon et al. present measurements of methanol concentration and turnover time in a range of oceanic settings. Their findings inform present understanding of the biogeochemical cycling of methanol in terms of: 1) the importance of methanol for meeting bacterial carbon demand in the ocean; 2) the sources of methanol in the surface ocean; 3) the range of methanol concentrations and turnover times in the surface ocean; and 4) the extent to which methanol consumption by microorganisms is used to fuel energy production versus cellular growth.

The paper is well-written, the analysis is sound, and the data is a valuable addition

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The paper is well-written, the analysis is sound, and the data is a valuable addition to an important and under-constrained problem. I have a few suggestions that in my opinion would improve the paper, but they are minor, and I recommend publication essentially as-is.

We thank this reviewer for their positive and helpful review.

(1) My only significant complaint is that the abstract is missing a lot of the key points from the paper. This is an issue because most people will only read the abstract! E.g., you mention the turnover time is “as low as 1 day”, but it’s equally important to know how widely the values ranged, and whether they varied in any kind of coherent way. The fraction of methanol used for energy versus growth is also a valuable and unique finding, but it doesn’t even make it into the abstract? Also, there have been very few measurements of methanol concentration in the ocean mixed layer, and so a summary of your measured concentrations should be in the abstract. Lastly, your finding that methanol’s contribution to bacterial carbon demand varies in a predictable way with chlorophyll content is great and should be stated up front!

Agreed. Abstract amended as suggested.

(2) Figure 3 and the associated discussion is a very nice synthesis of the data in terms of how the methanol contribution to bacterial carbon demand varies with chl-a. Is there a similar synthesis you can provide describing how the concentrations and turnover times vary?

We have analysed our data but there are no other obvious robust relationships. However, as our database increases in the future this will re-evaluated for future publications.

(3) Ideally there would be some coherent relationship like in Fig 3 that could be used in a model, but at the least some space in the discussion section should be devoted

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to explaining and summarizing the observed variability in methanol concentration and turnover time.

Agreed. The observed variability in methanol concentrations and turnover times for stations 7-12a have been discussed in the Discussion section 4.1. As indicated in Table 2, we do not have concurrent methanol concentrations for the other stations, so we have used concentrations made in similar locations or times. However a comparison of methanol concentrations and turnover times between oligotrophic and more shelf/coastal areas has been added.

(4) The ethanol / formic acid / pH connection is not totally clear-cut. E.g. Jacob (1986) argues that formic acid affects cloud pH but mainly via scavenging from the gas-phase; with in-cloud formic acid production too slow to affect pH much. So an additional citation or two here on this issue would be helpful to the reader.

This has been added into the introduction with the Jacob 1986 reference.

(5) "volatile organic carbon compounds" : : : odd phrasing, is there another kind of organic compound besides the carbon variety?

Word 'carbon' has been deleted.

(6) Section 3.1.1. These E:G ratios are interesting; can you put them into context for the non-expert reader? Your ratios range from 360:1 to 12:1, i.e. 92 to >99% of the methanol is being used for energy rather than cellular growth. Are those values anomalous, or similar to any previous measurements for other carbon compounds?

Previously molecular scientists working on methyotrophy have traditionally assumed a more 50:50 split between energy and cell biomass (Murrell, pers comm.) which is more comparable to our shelf and coastal stations, such as L4 in Table 2. This difference between microbial use of methanol between oligotrophic and more coastal locations has now been made clear in the abstract. In agreement, Kiene and Hoffmann Williams (1998) found that Glycine betaine (GBT), used by a wide variety of marine organisms

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as an intracellular osmolyte, but is also conceivably a small microbial source of carbon (and nitrogen), found on average 47% of the GBT was respired as CO₂. This reference and information has been discussed and added to section 3.1.2.

(7) Fig 1: A citation or URL should be provided for the MODIS data. In the figure caption, "modis" should be "MODIS".

Changed and citation added to Figure 1 caption.

(8) 3909/17-22: run-on sentence; the reader glazes over.

Agreed. Sentence edited and significantly shortened and simplified.

(9) 3911/24: "often elicited necessary" ?

Changed to '... of DOC often required to sustain bacterial production during oligotrophic regimes.'

(10) Fig. 2. Missing symbol designator for station 3 in the caption. Also, specify the time zone for the x-axis (local versus UTC?). And what are the error bars (range, SD, : : :)?

All added to Figure caption, time was GMT.

(11) Fig. 3: End of y-axis title is cut off.

Amended.

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