Earth Syst. Sci. Data Discuss., 5, C157–C160, 2012
www.earth-syst-sci-data-discuss.net/5/C157/2012/

Earth System Science Data Discussions

Interactive comment on "Bacterial biomass distribution in the global ocean" *by* E. T. Buitenhuis et al.

E. T. Buitenhuis et al.

e031@uea.ac.uk

Received and published: 9 August 2012

This short paper announces the availability of data on the abundance of bacteria (but see below) in the oceans. This is potentially a very valuable data set. Most of the data are from flow cytometric studies, with a few direct count estimates from the Sargasso Sea. The paper itself is eclectic, with several interesting asides (metabolic theory?) and lots (relative to the length of the paper) about attached bacteria even though all of the data are all about free-living cells. It's a bit surprising to learn that the grand average number of cells in the oceans is about 4 X 10ˆ8 cells per L, although that is perhaps only surprising if a reader is used to looking at coastal waters. The paper does not say why these data were chosen and other data sets were not used. It is essential that the "raw" counts of cells per L are presented at the web sites listed by the authors. I have

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

not tried to look at the actual data itself. Both the raw data and the gridded files are now publicly available at the web sites. The reader can convert those numbers into carbon or nitrogen using his or her favorite conversion factor. The authors have a small table (Table 2) summarizing a few of the estimates for factors used to convert cell abundance to biomass. I don't believe this table lists all studies on the topic(e.g. the Lee and Furhman number is not given, although the study is cited), but it is beyond the scope of paper to do so. Still the authors make a big deal of biomass when in fact the data they have are about abundance. They need to focus this short paper on abundance. We have added to the discussion: "Despite the uncertainties that we discuss above, we judge that the direct measurements of open ocean picoheterotroph cellular carbon that we have used here are the most precise conversion factors, and that in applications where the biomass of picoheterotrophs rather than their abundance is relevant (most notably in biogeochemical models), the database that is presented here has the largest coverage and the best estimates that are available at present." We state in 2.1: "we therefore excluded results from cultures or incubated in situ samples." This is why we did not include the Lee and Fuhrman conversion factors in Table 2, and indeed why the table contains few studies. We cite the Lee and Fuhrman study to explain why we excluded those results. We argue that the measurements on unincubated in situ samples in Table 2 should be more precise than using a wider selection of measurements with less stringent selection criteria, although we also point out that there is considerable variation in the conversion factors in Table 2. Thus, we have been at pains both to show the uncertainty in our database, and to argue that these are the most accurate biomass estimates that we can present at the present time. Specific comments Abstract: It seems very important to mention in the Abstract that the data set consists nearly entirely of estimates from flow cytometry and that the authors focused on the open oceans. The authors shouldn't end the abstract with a statement about the uncertainty of the conversion factor. This factor is not the topic or focus of the dataset being advertised here. We have added information about the coastal ocean throughout the manuscript: Abstract: "Surprisingly, the abundance in

ESSDD

5, C157–C160, 2012

the coastal regions is the same as at the same depths in the open ocean." Results, in paragraph 1: "Observations from the coast (bottom depth < 225 m) make up 12% of the data (4.9% of the ocean area, 0.12% of the ocean volume).", in paragraph 2: "There is no significant difference between abundance in coastal waters and in the upper 225 m of the open ocean (Fig. 3, one-way ANOVA, p=0.86).", in Fig. 3, and in paragraph 3: ", and only 0.0079 Pg C in the coastal ocean" About the conversion factor see the previous comment. Please also note that this was an aspect of our paper that was praised by Referee #1. P303, Line 18: It is essential that the authors add at least one more sentence about the relative abundance of bacteria versus archaea along a depth profile. See Karner et al. (2001) for starters. While using the term "bacteria" is acceptable for surface waters where microbes in the Bacteria dominate by far, it is not for deep waters where archaea can be as abundant as bacteria. We have added in section 2: "Archaea make up about 5% of picoheterotrophs in the surface, and typically about 50% of the population that can be distinguished by domain specific rRNA probes below 2000 m (Robinson et al. 2010 and references therein)." We have added in section 2.1: "The conservative conversion factor for incubated Archaea of 8.4 fg C cell-1 in Herndl et al. (2005) is similar to our conversion factor of 9.1 fg cell-1 for picoheterotrophs in the upper ocean, where the population is dominated by Bacteria." See also our reply to Referee #1. P304, line 12: The authors should cite an original research paper (or perhaps a chapter in a methods book or even a text book) for the Chauvenet criterion. It is not acceptable to cite a paper in preparation. We have added a reference to Glover et al., 2011. P304, line 15: The authors mention here a high value from the coast of Oman. But earlier in the paper, the authors say that coastal estimates were not considered. So were coastal values included in the data or not? We mention in 2.1 that we excluded conversion factors measured in coastal waters, we have added the words conversion factor to clarify this. No abundance/biomass data were excluded at all. Table 2: This could be more informative if the authors gave the oceanic region where the estimate was made. This information was added. The authors should explain how they came up with the global average conversion factor for

ESSDD

5, C157–C160, 2012

the open oceans. It's a straight mean of the three oceanic studies. It does not weigh the average reported by the three studies by the number of samples or replicates. It probably doesn't matter, and again, it's beyond the scope of this short note. But still, the authors should be clear about what they did. We added in 2.1: "We calculated the conversion factor at BATS from the geometric mean cell volume and the relationship between cell volume and carbon content (n=164) from Gundersen et al. (2002). We calculated the conversion factor as the average of the three studies in Table 2." Figure 3: This figure needs axes labels. Also, the figure could be more informative and present more data than a single depth profile. It could give the mean profile for the temperate zone, the tropics and the polar regions. Figure was changed as suggested. Figure 4: The vertical axis (depth) needs to be labeled. Axis was labeled

---

Interactive comment on Earth Syst. Sci. Data Discuss., 5, 301, 2012.