

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

Anonymous Referee #2

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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 4×10^8 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 not tried to look at the actual data itself. The reader can convert

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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.

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.

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.

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.

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?

Table 2: This could be more informative if the authors gave the oceanic region where the estimate was made.

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The authors should explain how they came up with the global average conversion factor for 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.

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 4: The vertical axis (depth) needs to be labeled.

Karner, M. B., E. F. DeLong, and D. M. Karl. 2001. Archaeal dominance in the mesopelagic zone of the Pacific Ocean. *Nature* 409:507-510

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

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