

Interactive comment on “The global impact of bacterial processes on carbon mass” by Barbara Ervens and Pierre Amato

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This is a very interesting paper that provides an innovative view – a new concept - of bacteria in the atmosphere. The authors make the point that a fraction of bacteria in the atmosphere is not inert but can multiply producing secondary bioaerosols and in parallel they can consume water soluble organic mass thus providing an alternative to the chemical degradation path for organic mass.

This is a nice and holistic view for the lifecycle of bacteria in the atmosphere. However, there are several gaps of knowledge in this cycle that the authors thoroughly discuss. Based on available literature and a number of assumptions that are clearly stated in the manuscript, the authors make rough calculations to evaluate the two terms involved

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in the bacteria budget, namely the secondary production of bacteria and the bacteria driven degradation of water soluble organic mass. This later, the biological degradation of organic mass, is compared to the chemical degradation pathway and found of potentially similar importance but with a large range of uncertainty.

Despite the significant gaps in knowledge that prohibit accurate estimates, I consider that, overall, the manuscript provides a new concept for the presence and functioning of bacteria in the atmosphere that deserves publication in ACP after some improvements.

Abstract: lines 21-22: ‘ the conditions under which microbial processes cannot be neglected as organic carbon sinks in clouds’ Please provide such information in the abstract.

Section 2.4.2: It is unclear which year of MODIS cloud data has been used. It would be nice to show the derived map of cloud volume and the ecosystem map that are later used to derive the numbers in Table 2 (in page 8 that is erroneously numbered as Table 1). What grid size is used for these calculations? How many grid points are used to derive the Fcloud over each ecosystem type? This is an important Table for the budget estimates that are further presented in the manuscript; therefore, it has to be well documented. Adding also a column with cloud fraction over each ecosystem as suggested by the other reviewer will be a significant improvement. It is also unclear 1) how the value of 0.15 is derived for FCloud, 2) whether the category ‘Seas’ in Table 2 contains also the sea-ice.

Line 215: explain to what you refer when writing ‘other formation rates’

Caption of figure 3. The reference to Burrows et al is incomplete.

Line 273: ‘sensitivity’ I think ‘uncertainty’ is more appropriate here.

Table 3: maximum range for Ccell (1E9) can you comment who and where has measured this ?

Table 3: comment for mcell first line: ‘assuming that they’ to avoid confusion replace

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'they' by 'cells'.

Lines 283-285: discussion about fungal spores: I do not see why this is discussed here. Please remove or rephrase sentence to better fit in the discussion.

Equation 6: F_c needs to be defined earlier, now it is defined in line 339. Also check units in this equation.

Line 340-342: provide an uncertainty range for F_c instead of one value. How this uncertainty is affecting the here presented estimates of WSOC loss by bacteria?

Line 364: 'slightly higher contribution of chemical reactions to WSOC loss'. Figure 4 shows that for Y_{voc} equal 0.5 the chemical loss can be double the bacteria loss. This is not 'slightly higher'. Please rephrase.

Line 372: Loss rate of 50 Tg/yr is stated here while in Figure 4, a maximum of about 30 Tg/yr is calculated. Make consistent.

Line 377: the authors claim that the WSOC losses are smaller than the predicted production rates of SOA_{aq}. However, when accounting for the range of these rates, there is no significant difference. Furthermore, this result will depend on the assumed F_c , so please rephrase.

Line 433: I think 0.7 Tg/yr should be 3.7 Tg/yr

Line 458: in our study provides.

In addition, please provide references for 1) Figure 1 in its caption 2) the value of 7 to 14% in Line 159

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