

Interactive comment on “Screening of cloud microorganisms isolated at the puy de Dôme (France) station for the production of biosurfactants” by Pascal Renard et al.

Anonymous Referee #2

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This manuscript by Renard et al. isolated bacteria from cloud droplets and analyzed the bacteria's ability to produce biosurfactants. The authors identified surfactants by culturing the bacteria, centrifuging the samples, and using a pendant drop tensiometer on the supernatant to determine surface tension. The authors demonstrate that a number of biosurfactant-producing bacteria exist in their samples. In particular, the α - and β -Proteobacteria phyla are shown to be widespread and effective biosurfactant producers. This work is novel and well within both the scope of ACP and the interests of its readers. However, the manuscript as it stands has some major outstanding issues that must be addressed before publication can be recommended.

Major Comments:

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- English grammar: This paper must be completely proofread by a native English speaker before publication should be considered.

-Clouds: the largest hole in this manuscript is the lack of cloud-water analysis. The authors acceptably demonstrate that biosurfactant producing bacteria exist in their cloud water samples but fail to demonstrate if the bacteria actually have a measurable effect on their collected cloud droplets. Even if the authors are unable to measure surface tension depression in their cloud samples, this should still be noted and contextualized in the manuscript. The paragraph in the discussion section, starting P14-L33, would greatly benefit from this analysis.

-Arbitrary choices: While not debilitating to the paper itself, two arbitrary divisions are made in this manuscript: 1) the surface tension division of Fig.3 and 2) the source region division of Fig. 4

For the surface tension divisions, the authors cite Baudel et al. (2012) and Ekstrom et al. (2010) saying that their divisions are chosen in a similar way. However, neither Baudel nor Ekstrom divide their data in the same way that the authors here are trying to do. It would be more correct to say that the authors are choosing bins for their samples that match 1-2 bins of previously published works. The other bins are completely arbitrary and the authors do not provide any reasons for why they chose >55 , 45-55, 30-45, and <30 mN/m. Some reasoning behind these bins needs to be present in the manuscript.

For the source region divisions, the authors present no reasoning for dividing the air masses into marine/highly marine/etc. From Table S1, it is impossible to tell why marine and highly marine are split. Fig. S1 suggests the split is because of time over open ocean but the authors need to be explicit here. I would also argue that Fig. 4 adds nothing to the manuscript and should be replaced with the HYSPLIT trajectories and some additional meteorological statistics (e.g. wind direction histogram for the sampling site for both cloud-sampling days and non-sampling days). There is also no

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reason to not show the air mass height results from HYSPLIT.

Furthermore, the analysis starting on page 11, which attributes statistical difference between the air mass divisions (one of which has 3 samples!), seems weak. I would suggest that there are stronger ways to segregate the air masses given the Table S1. I believe the whole analysis should be rerun using the chemical speciation data.

Shorter Comments:

-Mann-Whitney and KW ANOVA – can the authors comment on how appropriate it is to assume statistical independence for air masses that, though they are measured at the sampling site as from 2 different directions, shared the same path as few as 5 days prior to sampling?

-Figure 5 needs to be replaced with a proper box and whiskers plot or some other way to judge what the real spread in the data looks like (possibly note the std. dev.?)

-Paragraph starting P15, L26 – I am generally a proponent of contextualizing your work but I am not convinced this paragraph adds anything to the manuscript. It should either be removed or condensed and moved to the introduction.

Minor comments:

- S2.2, P4, L23-25 : Given that most of the people reading ACP are not biologists, a one sentence explanation on why those three specific agars were chosen should be added to the text. Also, TSA should be defined.

-P5, L11: Should read “3.2” not “III.2”

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