

## **Review bg-2020-324 :**

### **General comment**

Rosselli et al. (2020) present a paper entitled «Determining the hierarchical order by which the variables of sampling season, dust 2 outbreaks occurrence, and sampling location, can shape the airborne bacterial communities in the Mediterranean basin».

The presented results are of great interest as there is an urgent need of data about bioaerosol's biodiversity and transport together with the understanding of the parameters shaping the air microbiome

The case studies presented in this paper are very well chosen as they consider different situations, all taken in Sardinia with sampling on two different sites: Sassari (North Sardinia) exposed to European influence, Calgari (South Sardinia) exposed to African influence. Also two time periods have been considered: May and September, as well as dust and clear sky events. NGS-based taxonomic analysis has been carried out in all these samples and compared using various pertinent statistical tools. The results allowed to propose a hierarchy of the variables determining the biodiversity of the collected bioaerosols: sampling season >> ongoing meteorological events > sampling site. The sampling period had clearly the major impact.

This paper is worth publishing in Biogeosciences when the authors answer some major remarks and possibly make new complementary analyses.

### **Major concerns**

My major concern lies on the fact that the authors did not compare in an extensive way the results obtained in this study with those previously published by Rosselli et al (Sci Rep. 2015). In my opinion they should have included these first results obtained at the same location sites (Calagari and Sassari), under dust and clear sky conditions but at a different season (February). The same data sets recorded by the same authors are available and could be compared directly with the present one using the same statistical tools.

This comparison would bring strong arguments to generalize the hierarchy of the variables influencing the structure of the microbial communities and make the conclusions clearer.

The discussion presented in the present paper has to be consistent with the one published earlier to be really acceptable and sound.

For instance, the present paper (Rosselli et al. 2020, Table 3, Page 17) clearly shows that dust events have little influence of the biodiversity indexes (Simpson 1-D, Shannon H), this is quite contradictory with what is claimed in Rosselli et al. 2015 (Table 2, Page 4). Also this dust influence is illustrated in PCA and Cluster dendrograms presented Figure 4 (Page 5) and 5 (Page 6) of the results published in 2015 (Rosselli et al.), again this is contradictory with figures 4, 5 and 6 (Pages 18, 19 and 21 respectively) of the present manuscript. Could the authors comment on these results and possibly merge the data of the two papers with new statistical analyses integrating all the data. It is quite important to clarify the influence of not of dust events on the microbiome compositions.

Another point concerns the evidence of a "major conserved core microbiome" that could be considered as a "global Sardinian air microbiome" ( Figure 3 Page 5, Figure 5 Page 6, discussion Page

6) published in Rosselli et al. (2015). Again it would be very interesting to merge the data obtained in 2020 and 2015 to confirm the presence of such a conserved core microbiome. Could the authors make this analysis with the integrated data.

Finally, in this paper (2020), Rosselli et al. efficiently exploit wind rose graphs integrating wind speed and direction, temperature and humidity (Figures SM9, 10, 11, 12, 13, 14) to explain some of their results. Actually these wind rose graphs are presented for March, April, May, June, July, August, September, October and November. Unfortunately, data are not presented for February, a time period of interest for the experiments reported in Rosselli et al. (2015). Could the authors add these data and comment about the results of 2015.

### **Other comments**

It would be interesting to give the total number of cells present in the various samples as it is also a very important indicator describing the air microbiome. Does this number differ depending on the situations (sampling site, season, wind..)?

Fig S1 and S4: the authors present data concerning the amount of PM10, do they have data on PM 2.5? It would be interesting to look at them and see if there is a variation of their concentrations with the seasons, locations, dust events ...etc.

The authors refer to the importance of the “daytime height of the planetary boundary layer over Europe “ (Page 26, line 543). This is indeed an important factor that can shape the air microbiome. Do you have data on the height of the boundary layer at the sampling sites and during the air mass trajectories? It would be of great interest to add it to this manuscript and take it into account.

Finally, I found some mistakes in the citation of the references:

\*Gleick et al (1993, Page 2 line 48) and Shine et al. (2000, Page 3 line 69) are not in the reference list.

\*Page 3 line 64 “Polymenakou “(and not “Polimenakou”).

\*Some references in the list are not cited in the text:

Harland et al, 2008 (page 34, line 748)

Koenig et al, 2010 (page33 line 706)

Kramer et al, 2006 (Page 35, line 768)

Latif et al, 2014 (Page 35, line771)

Poschl, 2006 (page 37, line 808)

Shao et al, 2011 (page 38, line 834)

Shiklomanov et al, 1993 (page 38, line 837)

Wainwright et al, 2003 (page 39, line 867)