1. Page 2, line 54: The authors use Bowers (2011) to support a statement about bacteria playing a significant role in the composition and dyamics of bioaerosols amongst all the different airborne microorganisms. The cited paper, though, focuses on the effect of weather and land-use type over diversity and abundance of airborne bacteria in the Colorado Front Range, it doesn't make any statements about other airborne bioaerosols. The reviewer suggests to revise this reference.

2. Page 2, line 57: The authors use Bauer et al. (2002) as a citation to substantiate the atmospheric concentration over land of bacteria, but the cited paper is about the carbon content of fungal spores. The reviewer suggests to use a more fitting reference or explicitate the connection between the number of bacteria in the atmosphere and the cited paper.

3. Page 3, line 65: rather than Lighthart (2006), the reviewer suggests using "Lighthart B. Shaffer B.T. Marthi B. Ganio L.M. (1993) Artificial wind gust liberation of microbial bioaerosols previously deposited on plants. Aerobiology9, 189–196." The latter is better suited to reference a statement about airborne bacterial agglomerates, as also cited by Lighthart (2006) itself.

4. Page 3, line 67: Tong and Lighthart (1999) does not contain detailed information about particle size. The reviewer suggests to remove or change this reference.

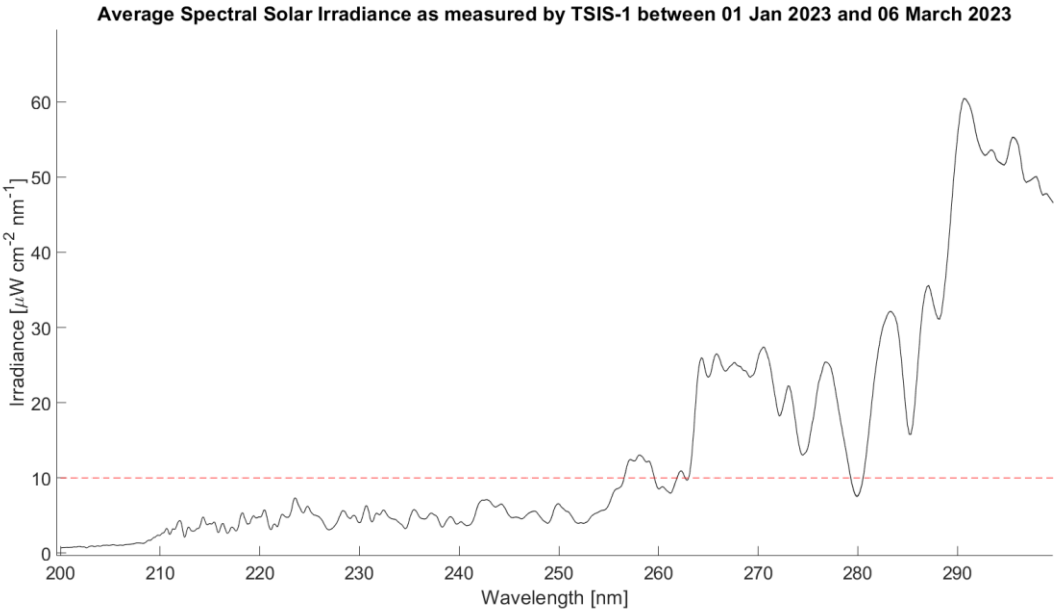
5. Page 3, line 77: Bauer et al. (2003) it's not present in the reference section. The only Bauer paper in the reference section is from 2002 and is about carbon content of fungal spores. Said paper does not detail any interaction between bacteria and the atmosphere. The reviewer suggests adjustment to this reference.

6. Page 4, line 96-97: "where trandisciplinary studies gathering […] issues are possible". "Gathering issues" is an odd phrasing, the reviewer suggests substituting "gathering" with "addressing" or to better explicitate the meaning of the sentence.

7. Page 6, line 167-168: what sensor does the PID use to measure concentration?

8. Page 8, Figure 2: The reviewer has some issues with the presented irradiance spectra. First of all, given that the data are presented over multiple wavelengths, shouldn't the correct unit for the y-axis be µW cm-2 nm-1 as it actually represents a spectral, rather than an integrated, irradiance? The irradiance in the UV-C range seems unusually high also for the Sun measured on the terrace of the Genoa Physics Department. Even if it's not clear due to the coarse resolution of the y-axis scale, it seems that UV-C at 200 nm is well above 10 µW cm-2 nm-1. That value seems unusually high. As a reference, the reviewer reports here a plot of average spectral solar irradiance measured by the TSIS-1 instrument on the International Space Station between the 1st of January and 06 March 2023. The instrument measures the solar irradiance at the top of atmosphere and normalizes it to 1 AU (i.e.: Sun-Earth distance). This measurement happens before any interaction between solar radiation and ozone happens in the Earth's atmosphere and, therefore, before reduction of any UV radiation. Data were originally in W m-2 nm-1 but were converted to µW cm-2 nm-1 by multiplying the original data by 100 in order to have units consistent with those presented in the paper. The plot shows the spectrum between 200 and 300 nanometers for better visualization of the range of interest and it shows that even above the Earth's atmosphere the irradiance up to (roughly) 250 nm

is well below 10 µW cm-2 nm-1. These values are almost constant with time: the plot contains also a shaded area which details the temporal standard deviation (SD), but the shading is not visible as the SD values are close to zero (maximum SD = 0.1263 uW cm-2 nm-1).



Data used in this plot are accessible through the following API link: https://lasp.colorado.edu/lisird/latis/dap/tsis_ssi_24hr.csv?time,wavelength,irradiance&time>=2023-01-01T00:00:00Z&time<=2023-03-07T23:59:59Z&formatTime(yyyy-MM-dd'T'HH:mm:ss) and their description can be found at: https://lasp.colorado.edu/lisird/data/tsis_ssi_24hr

Again the presented data are top of atmosphere, so at the surface of the Earth these UV values should be even lower.

Given that UV-C are the most harmful for biological particles, any excess in this spectral range in the Solar Simulator could severely impact bioaerosols' viability. Due to this reasons, the reviewer kindly asks the author to better explain the Solar Simulator spectra and the measured sun irradiance.

9. Page 12, figure 5: What does the error bars represent? Standard deviation? Standard error? Where does this uncertainty comes from? Repeated WIBS measurements at the same time?

10. Page 13, line 319: the acronym MISG is explained here, but was already used at page 7, line 173. Please move the acronym explanation at the first occurrance of it in the text.

11. Page 16, line 420: the acronym OD (which I assume stands for "Optical Depth") is used but never explained. Please explain it.

12. Pages 17, lines 437-438: it is stated "Data, obtained by CFU counting on agar plates, are averaged and used to figure out the uncertainty". What's the uncertainty metric? Standard deviation?

13. Page 18, figure 6: What are the error bars? Standard deviation? Standard Error? Also, while for CFU ml-1 the uncertainty should come from the fact that bacteria are spread in duplicates (page 17, line 434), where does the uncertainty of the QTx TOT come from? Was it also measured in duplicates?

14. Page 18, line 463: "OD600nm and QTx TOT have the same value of the b parameter". Looking at Table 1 that's not the case: for OD600nm the value is (3.3+-0.1)x10^-2, while for QTx TOT the value is (3.4+-0.5)x10^-2. If the meaning of the sentence was that the two values are not significantly different, please add a statistical test confirming it.

15. Page 19, lines 476-477: "the number of the counted colonies are averaged, to retrieve the bacterial concentration in the solution and its statistical uncertainty". Again, what's the metric for this uncertainty?

16. Page 20, line 499-501: The coefficient of variation of the experiments should be moved to the results section and then possibly discussed in terms of reproducibility of ChAMBRe experiments (see also comment #24).

17. Page 20: "Dark" conditions are never explained. Intutively, it just means with the Solar Simulator turned off, but please explicitate that.

18. Page 22, Figure 7: in the caption the colours are swapped and wrong: red is indicated for total bacteria and green for viable, while it should be red for viable and blue for total.

19. Page 22, lines 563-564: "particles, in the size range of 1-2 μm (τ=2-3 hours), the same of *E.Coli*". Commas make the sentence awkward to read. The reviewer suggests rephrasing to "particles in the same size range of E.Coli (1-2 μm) and τ of 2-3 hours".

20. Page 23, line 573: The usage of "Then" at the beginning of the sentence is confusing, it seems to imply that dead bacteria after ozone treatment were exposed to NOx. The reviewer suggests to rephrase it starting with "In another experiment …".

21. Page 24, Paragraph "Experiments with E.Coli and the Solar Simulator": all the results reported here must be discussed in view of the UV-C intensity in the Solar Simulator (see comment #8).

22. Page 24, lines 593-594: This sentence ending with "and data are here gathered" is not very readable. Please clarify its meaning.

23. Page 24, lines 598-599: "V:T (…) reaches zero after an hour". In figure 9, though, V/T appears to be 0 already at the 30 minute mark. What's the correct result? Also, please keep consistent the format of V:T in both text and figures (either V:T or V/T everywhere).

24. Page 25, lines 615-617: The reviewer thinks that reproducibility would need a more in depth discussion. First of all, in the text it is never explicitated the amount of performed replicates, with the exception of the caption of figure 7 where it is stated that eight repetitions were done for the baseline experiments. This point is, instead, extremely important as the reproducibility of the baseline experiments is crucial for the application of ChAMBRe itself. The reviewer encourages the authors to clearly state how the baseline experiments are performed in terms of replicates and how uncertainty and error propagation (if any) is estimated. Furthermore, the sentence here is not very clear, it suggests that the reproducibility (20% in terms of what? How was this reproducibility calculated?) is in line with the experimental sensitivity. What does this mean? That the experimental uncertainty is as big as the reproducibility? If so that would imply that any variation of viability could be inputtable to an error in reproducibility. The reviewer strongly suggest to clarify and clearly quantify all the parameters tied to the reproducibility and experimental uncertainty so that the reader can have a clear idea of the uncertainties tied to the ChAMBRe experimental protocol presented in this paper.