Authors' Response to Reviewer #3

All the reviewer's comments (in boldfaced red) have been numbered sequentially. After each comment the authors report their answer indicating eventual modifications that will be made to the revised version of the manuscript.

1) Some equations contain errors and units are missing or incorrect for a number of parameters, see the specific comments below. There are also a number of inconsistencies between the equations in the MS and in the code. I am puzzled most by the formulation for microbial population growth: is it assumed to respond instantaneously to changes in driving variables (temperature)? If so, is that a valid assumption on the 30 min. time step that you applied here, and at which time step would this assumption brake down? Or are the dynamics of the microbial population calculated transiently?
Moreover, the formulation and units of eq. 8 are inconsistent, which is where most of my confusion comes from.

In the model it is assumed that the microorganisms grow by a temperature-dependent factor (r) each 30 minutes (1800 s). Relatively fast growth are not unheard in field samplings with doubling times reported as low as 3.5 to 3.8 hours (see Hirano and Upper (1986) and references therein). However, fast growth in the model single time step is unlikely. Since a short time step is necessary to represent correct variation of friction velocity and other environmental parameters, the authors have introduced a calibration constant fine-tuned by the optimization procedure to adjust growth rate on such a short time-step. As for unit inconsistency please see comment 7 below.

2) Why is only gravitational settling considered as removal mechanism? Other dry deposition mechanisms can be relevant for particles of the assumed size (3.3 um). How sensitive are the calculated dry deposition fluxes to assumptions on the particle diameter?
The title promises new insights into microbial fluxes, but I do not see them in the abstract or conclusions. What are for instance the 'underlying driving forces (P12,L25)'of microbial emissions? What new insights has the combination of the flux measurements and the emission model yielded into these driving forces? Could you highlight these findings in the abstract and conclusion?

Please see reply to reviewer #1 comment 7 regarding the implementation of a deposition model considering impaction and interception.

About the 3.3 microns diameter, we chose it following Raisi et al. (2013) since, even if related to a suburban area, it was one of the most recent works where a long term monitoring of bacterial and fungal species was carried out along with aerodynamic considerations. For sizes close to the chosen diameter the average deposition fluxes are quite similar to what is presented in our paper (for a doubling of the diameter, the average difference in net fluxes is < 10 % on the calibration dataset).

About the "new insights", we aimed to highlight that microbial emissions are not driven only by turbulence, but are a more complex interaction of population dynamics, surface dynamics and atmospheric conditions, as shown with the PLAnET model. We can understand, though, that these are not really "new" insights but rather a confirmation of what was already hypothesized in past works. Following the reviewer's suggestion, we will modify the title in "Measurements and modelling of surface-atmosphere exchange of microorganisms in Mediterranean grassland".

3) I would like to see some more discussion on which types of microorganisms are sampled. The MS mentions viable microorganisms. Does that include both bacteria and fungal spores? Besides, can you say something about the size range of the observed particles? This will be important to eventually evaluate the role of the emitted bioaerosols on climate.

The chosen sampling medium was non-selective, thus allowing the growth of both bacteria and fungal spores and this will be clarified in the new text. Burkard samplers used in this experiment have proven to be able to sample both aerosolized *P. syringae* (see reviewer #1 comment 12) and mildew spores (Schwarzbach, 1979). Reasonably, particle size ranged from few (≈ 3) up to tens of microns (≈ 40).

4) P2,L21: in addition to these papers, (Crawford et al., 2014) measured PBA fluxes using the flux-gradient method, and (Ahlm et al., 2010; Whitehead et al., 2010) measured fluxes of coarse aerosol in tropical forests (presumably PBAs) using eddy-covariance

We thank the reviewer for these further references that will be added to the revised version of the text (around P2 L22-24 of the old version of the paper).

5) P5,L2: competition is mentioned here as a driver of the microbial dynamics, but I don't think it is actually included in the model. Please limit this description to processes that are included in the model.

The revised version of the text will clarify that the PLAnET model represents the source term only as a temperature dependent growth function

6) P5,L30: why is only gravitational settling included? For supermicron particles, also inertial impaction is important.

Please see reply to comment 2 and reply to reviewer #1 comment 7 regarding the implementation of a deposition model considering impaction and interception.

7) P6, eq8: I have some serious concerns regarding this equation, both as presented in the MS as in the code; this equation has microbial population size in the same units as the microbial growth and emission flux, which cannot be true. Should it read dN/dt=rNFn? In that case, it would represent exponential growth of the microbial population and loss due to emission. In the code, it is implemented as N(t)=N(t-1) + N(t-1)*r + Fn, in which units are also inconsistent. It could be solved by multiplication of the 2nd and 3rd term on the RHS by the timestep, which would yield a discretization of the equation for exponential growth.

We thank the reviewer for highlighting some inconsistency that were indeed present in the manuscript, and were corrected as detailed below. As for the model code and its numerical outputs, we understand the reviewer's concern that is due to a lack of clarity and lack of comments in the code, that however is correct and produced correct results.

Both emission and deposition fluxes are multiplied by the time step constant (ξ, in s, see Eq. (2) and Eq. (4)) therefore the correct dimensionality of F_e and F_d is CFU m^{-2}. New population at the time-step t is computed as the product of a growth rate (r, which is a ratio of temperatures and, therefore, non-dimensional) times the population at the time step t-1 (in CFU m^{-2}) times the net flux (F_e-F_d) which, thanks to the multiplication by the time step in s of F_e and F_d, is also in CFU m^{-2}. Fluxes are converted back into CFU m^{-2} s^{-1} by dividing F_e, F_d and F_n by ξ when outputting the variables, thus all the plots are consistent with the displayed units as well. We apologize for the lack of clarity in the explanation. All the explanations made here will be added to the revised version the paper immediately following Eq.(2) and (4). More comments will also be added to the revised version of the model that will be uploaded to MathWorks FileExchange.

8) P6,L24: it is unclear what is meant here: 'kmin, which is the point at which all process find an equilibrium'

Sentence will be rephrased in the revised version of the paper to better understand the link with the reference of Waggoner (1973) (i.e.: the presence of a fraction of microorganisms sheltered by wind action).

9) P7, L14: can you discuss how this choice has affected your results? This number seems to be important in determining the upper and lower bounds of the modeled microbial population.

The choice was made to avoid the unrealistic scenario of having always all the plants' leaves exposed to the atmosphere. However, it is a simple scaling of parameters ranging 1-2 orders of magnitude and, as it is possible to see from the model sensitivity analysis (Table 2), 10 % variations in k_{min} and k_{max} have minimal impact on model performance (values of $\varepsilon < 0.24$).

10) P9,L28-31: strictly spoken, the Burrows et al 2009a study does not discuss the effect of PBAP on precipitation, which is what this sentence seems to imply

Following the reviewer's advice, we changed the sentence from "Previous attempts to understand the impact of PBAs on precipitation" to "Previous attempts to understand the distribution of PBAs in the atmosphere".

11) P9,L32: I would add transport to 'emission-deposition process' (e.g. Wilkinson et al., 2012)

Following the reviewer's advice, we will add such process and reference at the indicated point in the text

12) P10,L28: what does it mean if the 95% confidence intervals include 0 and 1 or not?

The authors referred to the confidence interval for the slope of the regression. The fact that such intervals do not contain 0 implies that, even taking in account the uncertainties, the slope remains significantly different from zero (i.e. >0). Since the confidence intervals cross 1 we cannot exclude that by increasing the data point a better linear regression (with a slope close to the 1:1 line) could be achieved.

13) P11,L2-12: I miss a discussion here on the use of online detection of PBAs using fluorescence measurements (e.g. Gabey et al., 2010; Huffman et al., 2010) or single particle mass-spectrometry (Zawadowicz et al., 2017). These techniques measure concentrations, but could in principle be used in combination with micrometeorological techniques to measure fluxes (e.g. Crawford et al., 2014).

A brief discussion will be added in the revised version of the paper addressing the possibility of applying UV-LIF and SPMS to measuring microbial emissions.

14) P11,L18: it is unclear what is meant here: 'it is not to underestimate the long-term importance of evaluating the viable fraction of said fluxes'. Please rephrase

The sentence implied that, from the evolutionary point of view as well as for any interest in pathogen transport and colonization of distance places it is important to understand how many viable microorganisms are leaving the surface, since that would be the fraction of the total microorganisms potentially able to reproduce, colonize and, eventually, attack new areas and hosts. Nevertheless we agree with the reviewer that the significance of the sentence was not really explicit. In the new revised version of the paper the sentence will be rephrased.

15) P12,L9-10: is rain rate given in mm/hour here?

That is correct. The unit will be added to the revised version of the paper.

16) Fig. 6: with half-hourly observations and model data available, why are only daily average fluxes given? In addition, it would be interesting to see time series of observations and model data.

The model needed to run at half-hourly time steps for resolving meteorological dynamics, but then daily averages were calculated in order to reduce the significant random uncertainty inherent to the 30 minutes' data.

Technical issues

17) P4,L15: unit is missing for z0

Unit (m) will be added to the revised version of the paper.

18) P5, eq 2: in the code, Nk_max is given as N/k_max, which seems correct to me, as it would express the population scaled by the carrying capacity, and judging by the units. Besides, the values of m1-m3 differ slightly from those in L19. What are the units of m1-m3? They cannot all be unitless (as mentioned in Table 1 and 2) when Fe is in CFU m-2 s-1.

Thank you for pointing this out. Units for m1 and m2 are in CFU m^{-2} s^{-1}, while m3 is adimensional. Table 1 will be updated in the new version of the paper.

19) P6, eq 9: this equation seems to be missing an exponent ((Topt-Tmin)/(Tmax-Topt)), which is included in the code. What is the unit of r? Based on eq. 8 it should be s-1. Then also c should have this unit, and not none, as mentioned in Table 1 and 2. Please check these and other units throughout the MS.

We thank the reviewer for spotting the error, the exponent for Eq. (9) will be added to the new version of the text. As for the unit of r it is the result of a ratio of temperatures and an adimensional calibration constant (c, see Eq.(9)) and is therefore adimensional. See also the answer to comment 7.

20) Miscellaneous typos and language corrections:
P9,L9: won't -> will not
P10,L23: remove 'it'
P10,L24: the Planet -> Planet
P11,L1: a scaling -> scaling
P11,L14: transmit -> transmitting
P11,L15: represents -> represent
P11,L16: the atmospheric -> atmospheric
P12,L6: acting -> act

P12, L32: which is nested -> which it is nested
P12,L14: has-> have
P12,L24: suggest adding a comma between 'precipitation and'
P12,L32: which is -> which it is

Technical adjustments were made following the reviewer's suggestions.

21) Fig. 3 and 5: Data within years are plotted as if they represent time series (with continuous lines), but this is not always the case. This makes the plots hard to interpret.
Besides, time labels are placed at irregular intervals. Pls update these figures to make them easier to understand.

In the new version of the paper the figures will be reworked in order to remove lines, have clearer time labels and overall give a better presentation of the flux measurements.

22) In the code at L305: in the Cc calculation, a factor of 2 is missing in the exponent

We thank the reviewer for pointing out the mistake. The correction was applied, but the presented results are not heavily influenced by said mistake. With the introduction of the new impaction/interception model the settling velocity alone has become almost negligible. This correction will nevertheless be applied in the new version of the PLAnET model that will be uploaded to the MathWorks File Exchange.

References cited by the authors in the answers

Hirano, S., and Upper, C.: Temporal, spatial, and genetic variability of leaf-associated bacterial populations, Microbiology of the phyllosphere/edited by NJ Fokkema and J. van den Heuvel, 1986.

Schwarzbach, E.: A High Throughput Jet Trap for Collecting Mildew Spores on Living Leaves, Journal of Phytopathology, 94, 165-171, 10.1111/j.1439-0434.1979.tb01546.x, 1979.

Waggoner, P.: removal of Helminthosporium maydis spores by wind, Phytopathology, 1973.