Authors' Response to Reviewer #1

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) P2L22: ": : :some periods: : :some land uses." Rather vague description doesn't help the reader in getting an overview of previous work, rephrase or delete

Following the reviewer's advice, the sentence at (P2L22) will be removed in the revised version of the paper.

2) P2L32ff: "But while Burrows *: : :*" The whole sentence should be rephrased to improve readability. The word "species" is used but it's not entirely clear if the authors mean microbial species.
Also it is unclear why "in reality" emissions are different from the results from Burrows et al.. If this is a conclusion based on the submitted work, it should rather be made in the conclusions chapter.

The sentence (P2 L32 – P3L1-2). will be removed in revised version to avoid unclear statements

3) P3L13ff: The site description is too general (e.g. no species resolved vegetation or bare soil coverage). This is surprising given that the authors describe the existence of a large variability in emission fluxes "especially because of variation in vegetation across space (P3L3)". Was the grassland actively managed, e.g. grazed or mown during or in between sampling? Was the management comparable between different campaigns and study sites?

The experimental field was not intensively grazed and was not actively managed during the measurement campaigns with no mowing or irrigation. This information will be added to the text (around P3 L16-17 of the old version of the manuscript).

4) P3L24: How does the flow rate translate to Reynolds Numbers. What are the resulting losses in the sampler's intake? Are losses biased towards larger or smaller aerosols?

The virtual impactor was designed following good design practices from (Marple and Olson, 2011). Literature data report a sampling efficiency ranging from 80 to 100 % for mildew spores (Schwarzbach, 1979) and calibration tests performed during the 2008-2010 campaign showed it to be capable of sampling aerosolized *P.syringae*. Considering its high flowrate it is operating at super-iso-mean-velocity and therefore sampling efficiency is expected to decrease for larger particles proportionally with the ratio between external wind speed and the Burkard's sampling speed (Brockmann, 2011). This information will be added to the revised version of the paper following the description of the MRG (at P4 L3 of the old uploaded version of the manuscript).

5a-d) P3-4 Chapter 2.1: General questions regarding the employed micro-meteorological measurement technique:

a) Besides precise instrumentation for the concentration gradient, steady state conditions are the key restriction for applying k-theory/gradient measurements. Was the sonic data used to investigate steady state conditions, e.g. employ standard quality checks:stationarity etc. (Foken and Wichura, 1996)?

b) Results from detectors precision study (MRG) are visible in figure 3 only. It would be helpful to also present actual values and compare them to measured fluxes.

c) was scaling of measurement height by the zero-mean displacement height discarded in estimation of K due to the comparably small canopy?

d) Since the authors employed a fast 3d sonic anemometer, I would welcome the addition of a more data driven flux footprint and possible flow distortion evaluation besides the cited literature relationships the authors followed. Did you discard measurements from specific wind directions, e.g. situations where sensor inlets are located downwind from the tower structure?

A more detailed description on quality checks, footprint analysis and flow distortion will be added in a new paragraph to the Supplementary Material S1. One figure will be added to the main text (Fig. 4). To summarize here:

a) the quality check (QC) flagging system from (Mauder and Foken, 2006) was employed with flags ranging from 0 (best quality) to 2 (fluxes to discard). Only 4.54% of half-hourly measurements had a flag 2 in the September 2015 campaign where EC and flux-gradient (FG) method were compared. Keeping or discarding those data didn't significantly change the convergence of the two methods, nevertheless we chose to exclude flag 2 data in the new version of the manuscript where all the analysis and figures will reflect such exclusion.

b) a new figure will be added (Figure 4) reporting the relationship between counted CFUs, fluxes and MRG. The other figures will be re-numbered accordingly. The new figure is uploaded alongside the authors' response.

c) the zero plane displacement was computed as two thirds of the average canopy height at 0.13 m, and then z was computed above such zero level. However, as the reviewer points out, the small grass height makes the computation quite insensitive to zero plane placement, but however it was considered.

d) In the EC/FG comparison experiment, grassland footprint resulted the main contributor to the measured fluxes (about 50-60% contribution). The reviewer is correct about possible flow distortion: we decided to use the Integral Turbulence Characteristics (ITC) test to assess the goodness of turbulence data, that is embedded in the QC flagging system described above. Since no flag 2 data were detected when wind was blowing through the scaffolding, we can assume the absence of significant flow distortion

6) P4L23: "similar herbaceous species, such as*: : :*" these species were not listed for study site 1.

The authors would kindly point out that the two major species were already indicated at P3 L16

7) P5L7: You assume that deposition is purely driven by gravitational settling? Are other means (e.g. negative gradient between vegetation canopy and atmosphere, interception, impaction) insignificant?

We thank Reviewer #1 and Reviewer #3 for this suggestion that allowed us to improve the modelling approach. A new version of the PLAnET model has been developed including interception and impaction effects together with gravitational settling, following a widely adopted model(Slinn, 1982). The whole model has been re-optimized and the Gompertz parameter (Eq. (2)) re-computed. The model results remain consistent with the previous version, with only small differences in the linear regressions between measured and modelled daily averages. For the 2008-2010 campaign the slope changed from 0.71 to 0.70, offset from 0.88 to 0.28 and r^2 from 0.59 to 0.54. For the 2015 campaign the slope changed from 1.05 to 1.31, the offset from 0.11 to -3.75 and the r^2 from 0.57 to 0.68, yielding an even better correlation between measured and simulated net fluxes. The relationship between the newly computed deposition velocity and friction velocity for the campaign data remains under the Gillette et al. critical threshold (Gillette et al., 1974;Gillette et al., 1997) and therefore no bias should exist in the measured fluxes. In fact, no depositional considerations are made for example by Park et al. (2011) when applying gradient method to PM_{10} fluxes. The new version of the model will be uploaded to MathWorks FileExchange and all the data and figures in the revised version of the paper will take in account deposition, impaction and interception.

As for the negative gradients, these were never observed in the current dataset. However we agree that this should require additional investigation and this *caveat* will be included in the revised version of the paper (around P10-L 21ff of the current uploaded version of the paper)

8) P5L18: Why did you use the Lighthart and Shaffer data to express observed fluxes as a logistic function of ustar? What is the goodness of fit for m1, m2, m3 and eq.2 in general? How does the fitting errors propagate in overall model uncertainty.

The Lighthart and Shaffer data were used to parameterize Eq. (2) independently from the validation data. Using our own measurements to obtain a fit between u_* and fluxes would have probably resulted in better overall results when comparing the model to the data, but reduced the applicability of the model outside the presented situation. m1, m2, m3 are the result of an iterative numerical optimization, therefore their confidence interval could not directly be computed; we performed a sensitivity analysis to assess the impact of those coefficients on fluxes (Table 2), revealing that changing the coefficients by 10% has a small overall impact on the predicted fluxes (the delta in the error function ε is < 0.5). The final adjusted r^2 was 0.44.

9) P6L5: The settling velocity is highly dependent on particle diameter and shape. Obviously for the applicability of a model, simplifications have to be made here, since time resolved aerosol size and density spectra are hard to measure or predict. However it would be worth exploring if deposition (i.e. Vg) has a larger impact on overall predicted net fluxes by varying aerosol size modes and densities (in reality these will have temporal patterns for instance due to phenology of different sources). It should at least be specified if the used literature values for particle density and particle diameter are representative only for grasslands or a specific season.

We refer to Raisi et al. (2013) that , even if related to a suburban area, is one of the most recent works were a long term monitoring of bacterial and fungal species was made and coupled with aerodynamic considerations. The seasonal and spatial variation that the reviewer's points out is absolutely true and is acknowledged as well by Raisi et al. (2013). As the reviewer correctly states, a single diameter choice was made as a simplification. In the cited work the highest fraction of cultivable fungi and bacteria was found in the range between 2.1 and 3.3 microns and we have therefore chose 3.3 μm as a cautionary representative diameter for bioaerosols in the model. The text will be modified to acknowledge the non-universality of this aerodynamic choice (around P10, L11ff of the present uploaded version of the paper).

10) P6L10: eq. 6: please report goodness of fit for linear regression between avg C and LAI. How does the uncertainty in Ca propagate into prediction of Fd?

Goodness of fit between the variables is rather low ($r^2 < 0.2$), however its impact on the overall model and the predicted net fluxes is limited: a 50% increase in simulated concentrations (Ca in the model) resulted in an average percentage change of net fluxes of roughly 2% on the calibration dataset.

11) P8L5: The comparison between flux gradient and eddy covariance flux measurements provide confidence in the observations. However, eddy fluxes are not necessarily the truth, flux errors in LE are often in the magnitude of tens of percent. EC measurements were made at different height than the gradient measurements, meaning that the EC instrumentation sees different parts of the grassland. Along these lines, the open path EC sensor was not cross-calibrated with the closed path gradient sensors? In other words, It will be hard to conclude if the gradient or the EC results are off. A more detailed error/uncertainty discussion of the net fluxes obtained from the gradient method would be appreciated here. It should at least be acknowledged that the conclusions made from the H2O flux comparisons do not necessarily

apply to aerosol flux measurements. What are the expected uncertainties introduced through assuming scalar similarity? The MRG or precision of the detectors employed at same height could be used to propagate a flux uncertainty.
Since the model is calibrated on the measured fluxes (plus minus uncertainty) also the model will have this uncertainty.

We fully agree with the reviewer in that the good correspondence between EC and FG water vapour fluxes does not necessarily apply to bioaerosol flux measurements and we will add a corresponding note of caution to the revised manuscript.

As far as the concentration profile follows M-O theory, the difference in sampling height should not impact retrieved gradient fluxes. Sampling height is an input to the method and is therefore accounted for. Our field measurement conditions overall meet M-O requirements (e.g flat terrain, homogeneous).

The reviewer is correct that flux footprints from the two methods are likely slightly different, being larger for the EC that has a higher sampling height. However, the footprint analysis we performed revealed that majority of the EC footprint is contained within the experimental field, that is very homogeneous, making the two flux measurements safely comparable. Such discussion will be added to the revised version of the supplementary material.

The comparison between EC and FG was performed in order to assess the presence of a significant bias between the two methods (i.e.: significant under or over estimation of the FG method at low/high fluxes). The latter intent will be clarified in the revised version of the paper by modifying P8 L5-6 and, in the conclusions, P12 L15-18.

12) P8L15ff: the 2008/2009 measurements have a wider spread, partly due to the fact that no detection limit was applied (e.g. in 2015 all negative fluxes were removed due to the detection limit). What is the reason for that?

In the 2008-2010 campaign, the samplers were calibrated with multiple replicates of aerosolized *P. syringae* at different dilutions (three dilutions at 10^2, 10^3 and 10^4 bacteria ml^{-1}, three replicates per dilution). Given that no statistically significant differences were detected between the two samplers (except in one replicate at 10^3 bacteria ml^{-1}), no MRG was computed.

This explanation will be added to the revised version of the paper alongside aerodynamic considerations about the sampler (see comment 4 above).

13) P11L33ff: besides rainfall other events could have an effect on PBA production. You mentioned the heat wave in 2003. What about water stress or cutting/mowing/ grazing. Some of these stress factors might have lagged interactions with LAI and microorganism population growth. It would be great to introduce 1 or 2 sentences about these effects and how they would change the annual emission from a grassland, if feasible

Following the reviewer comment, some further effects on PBAs production will be discussed in the new version of the paper (around P12 L11ff of the present uploaded version of the paper).

14) Fig(3): Why is the detection limit (MRG) half in Sept-Oct as compared to July?

We apologize for any lack of clarity. Figure 3 does not report MRG values, only fluxes. MRG values are calculated on the CFUs measured by the samplers and when the two samplers fall below such detection limit, the flux derived by FG method at that time is flagged (yellow star) as unreliable. The new figure 4 (see also comment 5b) should clarify the latter point

15) Technical Corrections: P1L19: "*: : :*than that*: : :*". Numerus P3L12: "similar terrain". Phrasing: Do you mean flat? P8L7: delete ",instead," P8L7: rephrase, e.g.: spanned over multiple seasons P9L32: "*: : :* all the emission-deposition processes", please rephrase P12L14: "*: : :*has been". Numerus P12L21: "*: : :*outward fluxes". Rephrase, e.g. emission fluxes

All the technical corrections will be addressed in the revised version of the paper where indicated

References cited by the authors in the response:

Brockmann, J. E.: Aerosol Transport in Sampling Lines and Inlets, in: Aerosol Measurement, John Wiley & Sons, Inc., 68-105, 2011.

Gillette, D. A., Blifford, I. H., and Fryrear, D. W.: The influence of wind velocity on the size distributions of aerosols generated by the wind erosion of soils, Journal of Geophysical Research, 79, 4068-4075, 10.1029/JC079i027p04068, 1974.

Gillette, D. A., Fryrear, D. W., Gill, T. E., Ley, T., Cahill, T. A., and Gearhart, E. A.: Relation of vertical flux of particles smaller than 10 μm to total aeolian horizontal mass flux at Owens Lake, Journal of Geophysical Research: Atmospheres, 102, 26009-26015, 10.1029/97JD02252, 1997.

Marple, V. A., and Olson, B. A.: Sampling and Measurement Using Inertial, Gravitational, Centrifugal, and Thermal Techniques, in: Aerosol Measurement, John Wiley & Sons, Inc., 129-151, 2011.

Mauder, M., and Foken, T.: Impact of post-field data processing on eddy covariance flux estimates and energy balance closure, Meteorologische Zeitschrift, 15, 597-609, 10.1127/0941-2948/2006/0167, 2006.

Park, M.-S., Park, S.-U., and Chun, Y.: Improved parameterization of dust emission (PM10) fluxes by the gradient method using the Naiman tower data at the Horqin desert in China, Science of The Total Environment, 412–413, 265-277, 10.1016/j.scitotenv.2011.09.068, 2011.

Raisi, L., Aleksandropoulou, V., Lazaridis, M., and Katsivela, E.: Size distribution of viable, cultivable, airborne microbes and their relationship to particulate matter concentrations and meteorological conditions in a Mediterranean site, Aerobiologia, 29, 233-248, 10.1007/s10453-012-9276-9, 2013.

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.

Slinn, W. G. N.: Predictions for particle deposition to vegetative canopies, Atmospheric Environment (1967), 16, 1785-1794, 10.1016/0004-6981(82)90271-2, 1982.