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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Ecosystem–atmosphere exchange of microorganisms in a Mediterranean grassland: new insights into microbial flux through a combined experimental-modeling approach" *by* Federico Carotenuto et al.

Anonymous Referee #1

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1) General Comments The submitted manuscript presents both a micro meteorological measurement system as well as a modeling framework for time resolved quantification of biological aerosol and microorganism ecosystem-atmosphere exchange fluxes. Observational data was obtained over a Mediterranean grassland site in France over 3 measurement campaigns. A deterministic modeling framework was calibrated based on the first 2 measurement campaigns and model performance as well as sensitivities to input variables were evaluated on a more recent independent measurement campaign. In both domains the authors succeeded in presenting a conclusive and novel

combination of methods, producing a new (and to my knowledge the most directly measured) data set of high relevance. The proposed PLaNET model combines physically sound expressions of phyllosphere population dynamics and turbulent removal processes and was optimized through heuristic/trainable algorithms for solving non linear functions. Especially due the open source nature of their model and a detailed error and sensitivity analysis, both model and data set can be used and updated in the future. The manuscript accomplishes to reveal important insight (e.g. non linear responses) into interactions between bacteria/fungi population dynamics, grass land micro meteorology and the resulting microbial exchange fluxes. Therefore I recommend publishing this work in ACP following minor revisions.

2) Specific Comments

P2L22: "…some periods…some land uses." Rather vague description doesn't help the reader in getting an overview of previous work, rephrase or delete. 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. 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? 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? 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? P4L23: "similar herbaceous species, such as..." these species were not listed for study site 1. 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? 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. 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. 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? 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 H_2O 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. 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? 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.

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

3) 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

Foken, T. and B. Wichura. 1996. Tools for quality assessment of surface-based flux measurements. Agricultural and Forest Meteorology, 78: 83-105.

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