Response to Referee#2

We thank the reviewer for the careful reading of the manuscript and helpful comments. We have revised the manuscript following the suggestions, as described below.

Summary
This manuscript has developed a global emissions and transport model for primary biological aerosol particles (PBAP). There have been several other prior attempts to do this for all types of PBAP (e.g., Jacobson and Street, 2009) as well as individual types (e.g., fungal spores Heald and Spracklen 2009; bacteria Burrows et al 2009a/b). In this regard, the work is necessary but the manuscript itself does not make new advances in our simulations or understanding of global PBAP. In general, the manuscript has several major omissions of the data and methods driving the model, and this makes it not possible to interpret the results in any meaningful way. This paper requires major revisions to be acceptable in ACP.

Major comments

1. The manuscript states to have simulated the three main types of PBAP, yet there is no detail in the manuscript about the pollen emissions. The authors need to decide if are going to retain the pollen section of the PBAP inventory. If the authors choose to continue to include pollen in their total PBAP assessment, there are several new sec- tions that are absolutely necessary to understand what how the authors are simulating emissions and when and where they might be important. These include the following:

Comment: a. Add a section 2.2.3 for pollen. Section 2.2 is titled "PBAP emissions," with one subsection devoted to bacteria (2.2.1) and the other to fungal spores (2.2.2), yet there is no corresponding section on the pollen emissions. A section explaining the pollen emissions parameterization is absolutely required. Similarly, this should include the size distribution implemented in the model (as is for fungal spores and bacteria).

Response: We apologize for this omission in the text. The section has been added to the revised manuscript.

Comment: b. Provide some useful discussion on the pollen atmospheric distributions and their re- alism. For example, there is no discussion about the pollen emissions distributions that they simulate despite including a figure of pollen (Section 3.4). Specifically, their model simulates the highest concentrations of pollen in the tropics, which is inconsistent with the plant distribution of wind-driven pollen. Most plants in the tropics use insects or birds for transmission, so it is not expected that there would be high emissions in these locations. It is impossible to determine why this is without the explanation of the pollen emissions model (see point above).

Response: The emissions depend on LAI similar to fungal spores, therefore, it should not be unexpected that concentrations are high in the tropics. It is difficult to discuss the realism of the global pollen distribution, as an important effort is needed to collect a global database for pollen. The Jacobson and Streets (2009) provided the first pollen parametrization to be used for a global model. This parameterization has not been evaluated against observations for any global model, and has been mostly used by the community to provide a global distribution for pollen, despite of its many deficiencies (e.g absence of the plant phenology and the land cover type). This was not the objective of the present study, as we focused on fungal spore distribution. In the discussion section, we recommend the use of a more recent parametrization developed and evaluated by Wozniak and Steiner (2017). For these reasons, we decided to remove Figures 5a and 5b from the main text and added them to the Supplementary Information, but we keep section 3.4, to be able to discuss the contribution of pollen to the total mass aerosol composition.

Comment: c. There are ground-based pollen count observations of similar spatial sparsity to fungal spores. Why is a comparison of measured versus modeled not included?

Response: Our purpose in this study is to focus on fungal spores parametrizations and to less extent bacteria. Pollen has been added to model as an additional PBAP only in order to estimate their contribution to the total aerosol mass (see section 3.6). A further modeling study using the new pollen emission parametrization proposed by Wozniak and Steiner (2017) will be the object of a following study including comparison with the available pollen count observations, especially in Europe, where a large pollen counts database is available.

Comment: d. In the final discussion, there is hardly any recommendations or future work regarding these emissions and improvements. Again, if the authors choose to keep pollen in there, a more rigorous discussion is required to explain the role of this specific type of biological particle.

Response: Recommendations have been added to the discussion section.

Comment: 2. There is no information about the LAI distribution used in the model. This is rather important for the fungal spore discussion, as much of the explanation for the three different fungal spores is often tied back to differences in leaf area.

Response: Both the HS and HU parametrization in our simulations use the same LAI distribution (as well as the pollen parametrization). Information about the LAI distribution used in our simulation has been added to the revised manuscript in section 2.1.

Comment: 3. Section 3.2: This type of correlation seems rather obvious: modeled LAI is of course going to be lower in urban areas, so then another factor would have to compensate (and it would be a meteorological parameter). I'm not

sure how this would not be taken into account already by the existing models. If the authors think that this is important, then they should explore this in greater detail.

Response: We found these results interesting because they show the importance of the difference between urban and non-urban observations and their relationship with these physical parameters (usually not measured). This information could be taken into account potentially in future observations. Unfortunately, we could not find any observation publication reporting about these differences.

Comment: 4. Section 3.5: For the FBAP comparisons, why not also include the pollen and see if that improves any observations? The caveat of why FBAP may not (page 6, lines 20-21) or may (page 10, lines 10-13) is rather confusing.

Response: As shown in figure 5a, the pollen number concentrations are much lower that bacteria and fungal spores concentrations, therefore their contribution to the total PBAP concentrations will be very low. Besides, as explained in Page 10, the upper size limit of the UV-APS and WIBS is nominally 20 um, though in practice the inlet design of an individual measurement site frequently lowers the upper size point somewhat, therefore pollen can not be included for comparison with FBAP observations.

Minor comments

Comment: 1. The title should reflect the full acronym of PBAP (e.g., add the term "aerosol")

Response: The word has been added to the title.

Comment: 2. Abstract Lines 26-27: Needed?

Response: We believe that these lines summarize one of the main findings of this paper, therefore they should be kept in the abstract.

Comment: 3. Page 2 Line 19: fungal spores reference about being the most abundant and genetically diverse – this is a rather old reference, is there any more modern support for this idea?

Response: The reference to the work of Fröhlich-Nowoisky et al. (2009) has been added

Comment: 4. Page 2 Line 35: Global and regional models are cited here, yet the papers primarily refer to global studies on fungal spores and bacteria. There is a wealth of literature out there on pollen, and this should be included if pollen is kept in the manuscript.

Response: Actually, Jacobson and Streets (2009), whose pollen parametrization has been used here and Hoose et al. (2010) include both pollen modeling.

Comment: 5. Page 5 line 31: Because the model was run without meteorological nudging, some brief reference to prior met evaluation of the model to indicate to readers that biases in the concentrations are not due to meteorological parameters such as temperature.

Response: References to the model evaluation has been added in section 2.1., as recommended by the referee.

Comment: 5. Page 4, section 2.2.1: More detail on the bacteria emissions, specifically the fact that they are constant and are not simulate with any meteorological dependency should be made very clear in this section. This is discussed later in the manuscript (e.g., Section 3.5) but it would be clearer to provide more detail here such that a reader is not looking up all the references.

Response: For the sake of clarity, we provide a table of the best-estimates fluxes in the Supplementary Material, and a reference to it in section 2.2.1.

Comment: 6. Page 4, line 29: what does "best estimate number fluxes" mean in this context?

Response: "best-estimate" is used considering the optimization method used by Burrows et al. (2009b) for the emission estimates to fit the observed number concentrations.

Comment: 7. Page 6, line 18: What are some examples of "highly fluorescent particles of non- biological origin," and would these be more likely to be observed in anthropogenically influenced areas?

Response: This sentence "such as certain kinds of aged brown SOA, diesel soot particles and some HULIS types" has been added to the text.

Comment: 8. Section 3.1: Note several references to Figure 1 that should be Figure 2, also, the fit metrics are not displayed as stated in the text (lines 12-13).

Response: This has been corrected in the revised manuscript.

Comment: 9. Section 3.1: Please clarify what model layers are used to compare to observed fungal spore counts, as this also may affect the model evaluation.

Response: The word "surface" has been added to the mean number concentrations.