## Replies to the comments of Prof. J. Vila

We are grateful to the referee for the constructive criticism, which helped to improve the clarity of the manuscript. Please find below the replies to the specific comments and an account of the modifications implemented.

1. *In the complete and very-well written introduction, they use a very general terms from clouds. I believe it will be interesting to mention to the reader than thin clouds (with cloud optical depths below 5) have a different impact on GPP than thick clouds (lines 1-5 in page 2) (see Pedruzo-Bagazgoitia et al, 2015).*

   We added the discussion on cloud thickness, but in our opinion, it belongs more to Discussion. It now reads (p.14, l. 15-18): "The maximum corresponds to the clouds with the diffuse fraction on the order of 0.4-0.5. According to Cheng et al. (2016) and Pedruzo-Bagazgoitia et al. (2017), this $R_d/R_g$ corresponds to optically thin clouds with cloud optical thickness less than 5. Conversely, GPP decreases for optically thick clouds, which has also been demonstrated by Cheng et al. (2016)."

2. *I understand that the authors opted for a simple radiative transfer model due to a more complex radiative transfer model will require more input information that maybe is not available. My question here is if they have a reference on a study on how this simplication of the transfer of radiation might inuence their ndings.*

   For the aims of this study, it is enough to have a reliable clear sky model, because the criterion of clear sky is based on the comparison between measured data and modelled clear sky radiation. The study showing how the simplified model performs in comparison with the full radiative transfer model is one by Ineichen (2008), and the comprehensive study demonstrating the validity of this clear sky model based on the measurements from several US sites was done by Sengupta and Gotseff (2013). More detailed discussion was given in the manuscript (p. 9, l. 12-13 and l. 25-31).

3. *I also understand that they employ irradiances in their analysis (Eqs. 2-4 at page 5). Here, I would like to hear the opinion (or a discussion point) of the authors if the actinic flux can be a better variable to determine the effect of aerosol on GPP.*

   Actinic flux represents spherically integrated energy on a volume of air, while irradiance represents the energy transported across a surface (Madronich, Photodissociation in the Atmosphere..., JGR, 1987). Therefore, irradiance is dependent on the incidence angle and decreases under glancing angles, while actinic flux remains constant. Thus, for a clear day, actinic flux would not change much in the range of moderate zenith angles. It would be changed presumably by clouds and aerosol presence, in contrast to irradiance, which additionally depends on the cosine of the solar zenith angle.

   The potential advantage of using actinic flux for daytime and maximum growing season GPP studies, similar to ours, is that GPP saturates after a certain radiation threshold (ca. 700-800 umol s-1 m-2) and stays relatively constant, similar to actinic flux (this does not account for wapor pressure deficit cycle, leading to higher GPP in the first half of the day). Thus, elimination of angle dependence could ideally keep both GPP and radiation parameter constant under clear sky conditions. One important reason to use irradiance is that this is the typically measured and reported parameter which allows for comparison with other studies without additional confusion. This is relevant accounting for the fact that actinic flux is typically associated with atmospheric chemistry and UV-range of wavelengths.

4. *Perhaps, and in order to make connections with other studies, it is worth to show every now and then an equivalence between the condensation sink and the aerosol optical depth. Closely related to this, how relevant is the scattering efficiency (line 15 page 8) as an independent variable from the condensation sink in their study?*

   We agree with the comment, and add a figure demonstrating the connection between AOD500 and CS in the manuscript (see Appendix A and Fig. A1). Scattering coefficient and AOD characterize in situ and column-integrated scattering properties of aerosol respectively, we added a short discussion on this in Appendix.

5. *A general comment that it might be relevant. I miss in all the Figures information on the canopy height. In my opinion, this information should be given due to the different transmissivities of direct and diffuse radiation in the canopy. For instance, in gures 6 and 7, they could give different colours at which heights the measurements were taken. To be more comparable, this could have been done normalized by the canopy height.*

   Transmissivities of radiation depend not only on canopy height but also on LAI and on the distribution of leaves in the canopy, there is also difference for opened and closed canopies (e.g., Ross, 1981, The radiation regime and architecture of plant stands). We gave the information about the canopy height at the sites in Section 2. The measurements of LAI are not available for all sites. Moreover, this parameter is sensitive to the method of measurements and varies greatly even for the same site, which makes its usage difficult. For example, all-sided LAI of all trees >1 cm diameter in 2014-2015 from allometric equations (regression foliage biomass on tree dimensions) and measured average foliage area to mass ratios: SMEAR II - 7.3 $m^2$ $m^{-2}$, SMEAR I - 3.2 $m^2$ $m^{-2}$. Optical methods (fisheye photos, below-canopy PAR) give projected LAI of about 3 for SMEAR II, data not available for SMEAR I). At Zotino measurements of LAI had been performed earlier than the data set was obtained and differ from 1.3 to 3.5 $m^2$ $m^{-2}$. Therefore, in this study we prefer to rely on PAR as a robust measured parameter, while the information about the fraction of absorbed radiation, aPAR, dependent on LAI and canopy parameters, is contained in LUE = GPP/PAR, defined similarly to Cheng et al. (Using satellite-derived optical thickness to assess the inuence of clouds on terrestrial carbon uptake, JGR: Biogeosciences, 121, 1747-1761, 2016). Based on the fact that PAR dependences are rather universal, this approach allowed us to draw some general conclusions regarding GPP maximum in Section 3.2.2.

6. *Could the authors explain better the overestimation of the cloud-biased data? (line 15 page 9)*

   It follows from Fig. 5, that for clean atmosphere (low CS) and under clear sky conditions, the diffuse fraction following from the results of the clear sky modelling is small, on the order of 10%. However, many measured points, selected using simplified criterion of clear sky which includes cloud-biased points, have higher diffuse fraction values. This means that on average they result in a higher diffuse fraction 12-17% (Table 3) as compared to ∼10% predicted for Rayleigh scattering conditions by the model.

7. *I believe their criteria is robust to distinguish between aerosol effects and thin clouds (line 35 and p[age 10). However, haze can be very difcult to distinguish. Could the authors comment on this point?*

If the reviewer means plumes from forest fires under haze, the clear sky model fails to give right predictions for these periods. Therefore they were excluded from consideration in the aerosol-radiation part of the study (data set from Fonovaya, 2016), which we mentioned in the manuscript.

8. *Figure 8 summarizes and it is in my opinion the highlight of the paper. However, all the data is gone and only the estimated dependences are given. Why? I understand that the data can be very scattered but I think it can be interesting for the reader to see by him/herself these maximum behaviour. The behaviour reminds me the one reported by Min and Wang (Geophysical Research Letters doi:10.1029/2007GL032398, 2008, see Figure 1). Since they dont have a discussion section, I think as a reader I will appreciate a more elaborate discussion.*

   We added the figure (Fig. 9) and discussion in the manuscript (p. 14, l. 26-30). Our data sets look similar to those reported by Alton et al. (A sensitivity analysis of the land-surface scheme JULES conducted for three for- 5 est biomes: Biophysical parameters, model processes, and meteorological driving data, Global Biogeochemical Cycles, 20, 2007) and Alton (Reduced carbon sequestration in terrestrial ecosystems under overcast skies compared to clear skies, Agricultural and Forest Meteorology, 148, 1641 - 1653, 2008). The increase in GPP reported for SMEAR II is also similar to Alton (2008), but for mixed forests we obtained increase up to 30% as compared to moderate 10% increase for broadleaf forests reported by Alton (2008). Note that he used parametrization for the diffuse fraction of global radiation while we had measurements of diffuse radiation at four sites out of five.

We thank again the referee for the useful suggestions. We hope that the manuscript is now suitable for publication in ACP.