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Interactive comment on "Direct effect of aerosols on solar radiation and gross primary production in boreal and hemiboreal forests" *by* Ekaterina Ezhova et al.

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The authors present and discuss a very interesting study on how the diffuse radiation driven by the presence of aerosol enhances gross primary production (GPP). Based on observations taken at five sites, they reported an increase that varies between 6-14% depending on the aerosol loading. In the methods, special attention is paid to separate the effect of clouds from aerosol, and to determine the differences between homogeneous coniferous forest at high latitudes and mixed forest at lower latitudes. This is done by applying a criteria in the observations to determine the effect of diffuse radiation on the light use efficiency and the photosyntetically active radiation. I find

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Discussion paper

the results very interesting and worth to be published in Atmospheric Chemistry and Physics. I include my comments on potential improvements of the paper.

Comments:

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).

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 simplification of the transfer of radiation might influence their findings.

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.

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?

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 figures 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.

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

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 difficult to distinguish. Could the authors comment on this point?

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 oi:10.1029/2007GL032398, 2008, see Figure 1). Since they don't have a discussion section, I think as a reader I will appreciate a more elaborate discussion.