

## ***Interactive comment on “Aerosol pollution radiative effects on land carbon uptake in China” by Xu Yue and Nadine Unger***

**Anonymous Referee #1**

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Yue and Unger studied the effect of aerosol pollution on land carbon uptake. Although this is a timely topic, the manuscript has no clear objectives. It is not clear whether the study aimed at developing better methods and demonstrates the achievement with an application over China or whether the study aimed at enhancing our knowledge about the interplay between aerosol pollution and carbon uptake. Although the title suggests the latter, parts of the result and discussion suggest a methodological study. The lack of explicit objectives makes it difficult to assess the value of the study. Depending on its objectives some of its shortcomings might be acceptable whereas for other objectives it is not.

Some definitions of the carbon fluxes at the ecosystem level (both in the introduction and the discussion) are not correct. In the manuscript, NPP, for example, is called the net carbon uptake. NPP is the net primary production. Rh needs to be subtracted to

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derive the net carbon uptake. It is not clear whether the reference for “uptake” is the land-atmosphere interface (then the common term is NEP or NEE) or the ecosystem (then the correct term should be NECB or NBP). Have a look at Chapin et al 2005 to get the terminology straight.

The use of a big leaf model assumes that the leaf mass is homogeneously distributed in space (reflected in the equations). This is not the case and may become important when one of the key processes is that diffuse light can penetrate deeper into the canopy than direct light. Whether this is true or not will depend as much on the canopy structure as on the LAI itself (noted in the discussion). Along the same lines: different PFTs may have a very different canopy structure. When differential effects between PFTs are targeted, this should be accounted for in the parametrization of the big leaf model. The authors address several of these issues in the discussion but the study makes no effort towards solving these issues. Therefore, the modelling work does not represent an advancement. Existing approaches have been implemented in YIB. I'm not saying these issues necessarily invalidate the results of the study but they should be clearly addressed both in the text and these considerations should be reflected in a sensitivity study. Again, whether these assumptions are acceptable depends on the objectives of the study.

MODIS NPP is classified as an observation (L224-226). This is overly optimistic: MODIS NPP is a model. Where NPP is calculated from a light-use based GPP and a modelled  $R_a$ . At present there is no means to detect  $R_a$  from space. So MODIS should not be considered an observation. Until present validating NPP, therefore, has to rely on scattered site observations.

On several occasions Table 1 is used to demonstrate that the sensitivity of the simulated GPP and NPP to diffuse light is acceptable. However, the majority of observational evidence in Table 1 is for NEP. If the observations for NEP are not used in the study they should be removed from table 1. Alternatively the simulated NEP response to diffuse light should be validated.

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