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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Direct radiative effect of carbonaceous aerosols from crop residue burning during the summer harvest season in East China" *by* Huan Yao et al.

Anonymous Referee #1

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General Comments This paper uses the WRF-Chem model to derive estimates of direct radiative effects (DRE) over a region of Eastern China heavily affected by agricultural burning emissions, for the period June 2013. Model output is evaluated against some in-situ surface measurements over the period and MODIS satellite AOD products. The parameterisation of Saleh et al., 2014 is used to estimate the impact of including an absorbing portion of organic aerosol from biomass burning sources (brown carbon, BrC) on the DRE. Sensitivity simulations are carried out without biomass burning emissions, without the absorbing BrC component of OA, To my knowledge, this is the first study that includes an estimate of the radiative impact of BrC in WRF-Chem, an important first step in understanding the impacts of this currently highly uncertain

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but potentially important aspect of OA. For that reason I think it would be suitable for publication with appropriate changes. However, a key issue I have with this paper is how the direct radiative effects are calculated. The authors do not have aerosol-cloud interactions, so there should be no indirect effects. However, the absorbing aerosol will still absorb radiation and affect cloud formation and dynamics (the semi-direct effect). What the authors currently describe as the DRE is really the sum of the DRE and SDRE. This issue needs to be appropriately tackled before the paper is suitable for publication. The paper is mostly well written, although the authors need to cite some more work from the field, and some improvements to English and structure of results section are needed to improve clarity (see details below). In addition, while uncertainties are discussed, there is no attempt to quantify them and there is no discussion of the statistical significance of their results (although I acknowledge it can be challenging to reach statistical significance over time short periods).

Specific Comments 1. Ln. 26: Here and elsewhere, the authors say BrC and BC introduced 'significant positive DREs', but do not calculate the statistical significance of these results. Unless the authors can prove this effect is statistically significant, I suggest avoiding use of the word "significant", as it commonly implies statistical significance in scientific writing.

2. Introduction: There are no references to any studies that have used WRF-Chem papers in the introduction. This is a severe oversight. The authors should discuss some other papers which use WRF-chem to estimate the DRE and other radiative effects of aerosol from biomass burning or other sources.

3. Ln. 66-67: The meaning of the sentence beginning "Off-line models with discrepancies. . ." is unclear in the context of the paragraph. Are the authors saying the previously cited articles use offline models? If so, please make this explicit. They should then describe how this can be improved with online models (e.g. WRF-chem), with appropriate references backing up this statement.

4. Ln 109-112. The Authors are using an incorrect definition of DRE here. The authors correctly state that only having direct radiative feedbacks and not aerosol-cloud interactions, there is no indirect effect in their simulations. However, by using an online model, the radiative impacts of absorbing aerosol will impact cloud formation, circulation and distribution in the model (i.e. there are semi-direct effects going on, indeed this is the advantage of using an online model over an offline one). Therefore, by evaluating all-sky TOA radiative fluxes, the authors are really presenting the combination of direct and semi-direct effects. A rough estimate of just the DRE can be calculated comparing the TOA clear-sky radiative fluxes between scenarios. A more rigorous calculation of direct (and semi-direct) would need double-radiation calls, with additional fluxes calculated without aerosol radiative interactions. Please see for example Ghan et al., 2012 and Archer-Nicholls et al., 2016 for further discussion on this.

5. Ln 123-128. This is phrased confusingly by first saying the refractive index of OA is 0, then immediately saying how the refractive index of OA is parameterized. I would suggest rephrasing as: "For each bin, the complex refractive index of the shell was derived by volume averaging that of every shell species (Barnard et al., 2010). By default, the imaginary refractive index of OA is zero. In this study, we adopted the Saleh et al. (2014) parameterization. . ."

6. Can the authors comment on how the Saleh parameterisation was developed, what its appropriate uses are, and how it has been used previously and tested? E.g. what data sources where used to derive it, from what emissions sources. This would help understanding of uncertainties associated with the Salah et al., 2014 parameterisation for those not familiar with it.

7. The authors switch between OC and OA a lot. Do they multiply OC from emissions/model output by a factor to give total OA? If so, please give the factor.

8. Section 2.2: Can the authors comment on how their emission inventory compares to other more commonly used biomass burning inventories over the region, in total mass

emissions for OC and BC.

9. Ln 210-14. It is not clear whether the fluctuations described are about the measurements or model. Please explicitly describe the observations first, then how the model behave in comparison. Please give some statistical measure of the model skill against these observations, as was done for the meteorological data.

10. Ln 234-239. What diurnal profile do the fire emissions have in your model? Are you saying the emissions from the fires are greater at night than during the day? I would expect the high surface concentrations at night are almost entirely due to the collapse of the nighttime boundary layer.

11. Ln. 267-8. Much higher that what previous DRE estimate? Please give hard numbers and references of previous estimates for this.

12. Section 3.2: are the radiative effects calculated over the whole month of June? Over the whole of the inner domain? Please be specific., these results really are just for a specific time and place and should not be interpreted as typical effects for the region. When comparing with results from other studies, are the comparisons over the same region over similar timeframes?

13. Ln 408-411. The authors discuss here that the aerosol will be bringing further effects on PBL, TKE, clouds and precipitation, but no attempt to present these changes is made. These changes will have an effect on the radiative balance (semi-direct effect) and should be documented.

Technical corrections: 1. Ln 9. Remove opening “The”.

2. 76-77. No reference given for WRF-Chem (usually Grell et al., 2005).

3. Ln 70. Insert space in “Zhang et al., 2008).The previous. . .”

4. Ln 142. I think the emissions factors for BC and OC are the wrong way round (unless BC emissions are four times that of OC!)

5. Ln 168. The authors say seven parallel simulations were conducted, but only list 5 in Table 2.

6. Ln 208. Remove the word 'tightly', it is redundant in this sentence.

Figures and tables: Table 2. and 4. These are missing the scenario with volume mixing instead of core-shell. Please include as well. Figure 4. Is 6:00 local time or UTC? Why is this time typical? Please mark on the maps the location of the Suixi site for reference, and make the wind arrows larger and less dense so they are easier to see. Please confirm whether the arrows are surface wind fields. Figure 6. These are just BASE-NOBCCB and BASE-NOBRC runs? Please also show panels for the other scenarios. Over what timeframe are these calculated? I assume the blue areas in panel a. are due to changes in cloud fields. I would assume if plotting the actual direct effect from BC, or just the clear-sky fluxes, that figure would only have red shading.

References: Ghan, S. J., Liu, X., Easter, R. C., Zaveri, R., Rasch, P. J., Yoon, J.-H. and Eaton, B.: Toward a Minimal Representation of Aerosols in Climate Models: Comparative Decomposition of Aerosol Direct, Semidirect, and Indirect Radiative Forcing, J. Clim., 25, 6461–6476, doi:10.1175/JCLI-D-11-00650.1, 2012. Archer, S., Lowe, D., Schultz, D. M. and Mcfiggans, G.: Aerosol-radiation–cloud interactions in a regional coupled model : the effects of convective parameterisation and resolution, Atmos. Chem. Phys., 16, 5573–5594, doi:10.5194/acp-16-5573-2016, 2016.

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