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Interactive comment on "Importance of aerosols for annual lightning production at global scale" *by* S. Venevsky

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First of all, thank you for interesting review.

<A critical flaw in the paper is that although aerosols have effects on cloud microphysics (and on radiative forcing) and therefore on electrification, aerosols by themselves do not produce deep convection.>

There is no intention (and there no any of such statements) in the study to assume that aerosols produce deep convection by themselves and that lightning is possible without convection. On the contrary, I wrote in the Conclusions "As for the finer than annual time steps, many studies showed that lightning production, described by related indices (Schummann resonances or other) is positively correlated to surface temper-

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ature at diurnal and daily time scale for tropical regions (Price, 2013). These findings demonstrated that description of atmospheric thermodynamic instability is most likely necessary for estimating of the flash rate at diurnal, monthly and possibly seasonal time steps." Simplified aerosol model at annual time step which does not include convective parameters was designed intentionally to demonstrate that aerosols in form of CCN play significant (most likely equal) role in lightning production as the thermodynamic instability, which is of course necessary condition for lightning. Implicitly, I assumed that during the year deep convection may happen everywhere, which I think is simplification, but not an over-simplification as total cloud amount at annual time step is greater than 30 % everywhere http://www.atmos.washington.edu/CloudMap/. I am far from the idea that such a simple purely aerosol based model can be applied at fine than annual time step.

<The correlation between aerosols and lightning comes through the correlation of aerosols to large land masses and thus to instability.>

I respectfully disagree with statement. Correlation between convective precipitation taken as instability descriptor and CCN, object of this study is rather low at global scale (r2 is around 0.3 for annual averages). Another instability descriptor CAPE is not able to distinguish maritime and continent conditions (Williams, Stanfill, 2003). High correlation between lightning and aerosols in this theoretical study was found for Pacific Ocean and as well in observation study of Yuan et al. 2011for the same area, i.e. not only for land. Besides I cannot see from physical sense why aerosols should obligatory highly correlate with instability.

<Aerosols only have a modulation effect, and if that could be captured in a simple model it would be much more interesting. As it is, the paper develops a correlation between aerosol optical depth and lightning, which happens to capture the thermodynamic contrast between continental and maritime conditions, but no new real physical insight is presented.>

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I also kindly disagree here. At first I do not develop correlation between AOD and lightning (this correlation - AOD/lightning rate – for the globe, by the way, is not so impressive near 0.4) I found high correlation between simulated (not observed) CCN and observed lightning production rate. Secondly, I attributed high correlation to physical mechanism of secondary ice multiplication regulated non-monotonically by CCN concentration (of course in conditions when convection is in place). I assume in aerosol model that CCN influence lightning by affecting secondary multiplication everywhere (when convection happens) and this is a new explanatory physical mechanism and new insight to the old discussion.

<I don't think there is much question now that aerosols can play a role in affecting thunderstorm electrification via the effects on the cloud microphysics. Those effects are likely to vary substantially from case to case, however, so the extrapolation of simulations using a single environment (Mansell and Ziegler, 2013) to the whole globe is difficult to justify.>

I do not make extrapolation of (Mansell and Ziegler, 2013), I make a generalization of this study, i.e. took their physical explanation and took only their non-monotonic functional form of dependence of lightning by CCN concentration. No any numeric parameters from (Mansell and Ziegler, 2013) were used in a design of simple aerosol model of this study.

<The effects are subject to factors such as wind shear, so invigoration may or may not happen. So aerosols can modulate convection, but the modulation is not necessarily monotonic. You can have whatever aerosols you want, but without the thermodynamic instability there will be no convection and no lightning.>

This is, of course, correct and I also write this in my Conclusions section (see second paragraph). I did not have intention to attribute lightning to aerosols only. I made a model intentionally ignoring convective parameters to underline importance of aerosols for lightning production and demonstrated that such a model has AT LEAST equal ex-

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planatory ability with only thermodynamic – no aerosols based model. I also think that only model combining both instability and aerosols can completely describe lightning.

<Figure 1 of Alaratz et al. (2010) shows a rather selective analysis area that excludes many higher flash densities at low AOD.>

Here I cannot comment as I do not know, which flash densities/AOD data we speak about if it is not published. Anyway, I do not use directly neither AOD, nor results of Altarz et al, 2010 in my study.

<One effect of aerosols at high concentration is ignored, and that is the cooling (and stabilization) of the boundary layer by the backscattering of radiation. This weakens convection by reducing instability, and would be difficult to separate from reduced precipitation from the microphysical effect of smaller droplets.>

Yes, The cooling stabilization effect of the boundary layer is ignored in this study. This as I mentioned in comment before was done due to a design of this study (i.e. intentional design of AH model without convection mechanisms). Combined model should include this effect and separation of cooling and microphysical effect of small droplets should be further studied. I will include this in discussion in reviewed version.

<Figure 4 suggests a substantial high bias in the AH model for low flash rates over oceans. Although the African continent looks better by eye than the thermodynamic model, it seems to do less well in South America (too low) and has a poor pattern in North America, basically missing the high flash rates in central and southeastern United States. Central Europe and Asia also show high bias. (The Thermo model has a high bias in Asia, as well.) Just looking at the figure, the Thermo model has a much better map of overal coverage of lightning.>

By eye is difficult to judge which of the models is better AH or TH, Somewhere AH model is better in representing lightning spatial pattern – somewhere TH model is better. I agree with this. That is why I used correlation analysis to compare models (see

Table 3). It should be noted, however, that AH and TH model are in different initial conditions for comparison. TH is a polynomial fit model with 10 coefficients and is applied separately for land and oceans, AH model is also a fit, but a fit to prescribed functional form with only 4 coefficients. AH model is applied simulteneously for land and for oceans. From pure mathematical point of view if one put a polynomial fit with 10 coefficients of lightning by CCN separately for land and ocean, results of AH model will be probably even much more impressive.

<Table 3 is not explained at all. The p values in the tables are mostly ridiculously small, so why bother?> I am going to put more accurate explanation for the Table 3 in revised version, thank you. RC in the Table is the regression coefficient observed/calculated flash rate. P-values are related to RC coefficient.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 4303, 2014.