Atmos. Chem. Phys. Discuss., 14, C1384–C1386, 2014
www.atmos-chem-phys-discuss.net/14/C1384/2014/

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

Anonymous Referee #2

Received and published: 14 April 2014

1. Major Comments

I recommend the paper to be published after major revisions to it are made, such as by relating it better to the wider literature.

The paper is quite well written. Although I am not totally convinced by the veracity of the claim that annually averaged lightning is more related to CCN aerosols generally than to thermodynamic factors, I think the paper is valuable in moving the debate forward. In particular it is a novel and original idea to create a parametrisation of lightning in terms of the aerosol-sensitive microphysics of ice multiplication. It is important for the lightning community to accept that cloud-modellers can now predict lightning in terms of microphysical events and that their predictions can inform the analysis of ob-

servations of flash rates. Cloud particles are simply aerosol particles made bigger by activation and cloud-particles' mean size determine collision efficiencies and precipitation production, hence influencing cloud glaciation, latent heating and cloud dynamics. Thus, aerosol particles are known to control convective cloud properties by detailed modelling studies, and must have some influence on lightning.

The authors should address one by one the hypotheses of Sherwood et al. (2006), in a discussion section. Sherwood et al. also analysed global satellite data of lightning and other proxies for convective clouds, reporting correlation coefficients. Does the present study verify some of their hypotheses and disprove others ? Is there any conflict with their observations for those of the present paper ?

2. Detailed Comments

There are some issues of style to address. The authors need to split the introduction into multiple paragraphs. In the tables, what do RC and multiple R stand for ? Authors need to explain all this in the caption to the figure.

The authors should cite the paper from a few years ago by Alexander Khain where a landfalling hurricane was simulated and lightning probability predicted to be favoured where continental CCN were entering the system – on the periphery of the hurricane. They should cite the study by Sherwood et al. (2006) and comment on which of their hypotheses is supported by your results. Sherwood et all may have thought that AOD is largely related to dust (the largest aerosols) rather than to CCN concentration (the smallest aerosols). Could not the presence of large AOD simply reflect the presence of dry hot desert air that is more or less prone to vigorous convection due to thermodynamic reasons ? Have the authors accounted for how AOD varies greatly with humidity, which causes swelling of the CCN in subsaturated conditions ?

Equally, Phillips et al. (2001, 2002, 2005, 2007), Pinsky and Khain (2002) and Khain et al. (2012) all showed that in-cloud droplet activation predominates in the droplet concentrations in deep convective clouds. These papers need to be cited. This, in turn implies a dominant role for the smallest soluble aerosols. How can AOD measure reliably the smallest soluble aerosols, since they do not control the cross-sectional area exposed to radiation ?

In the text of the conclusions and Sec. 5, the authors need to summarise the correlation coefficients both of the aerosol and thermodynamic models succinctly. In Sec. 5, this is only done for the aerosol model. What is the correlation coefficient for the TH model ? One should not have to read through an entire table to find the right number.

I agree it is plausible for ice concentrations to both increase and decrease with increasing CCN concentration, and the modelling paper by Phillips et al. (2001, 2002) that showed this should be cited.

Finally, the authors need to explain why the dominance of CCN concentrations influencing the observed lightning arises from the choice of time-scale of the averaging. Over one year, the thermal equator has huge variability and the variations of lightning due to changes in temperature from month to month at a given location will be much less than the contrast between different geographic locations in the annual average of lightning. Essentially they large cancel out in the annual average. Is this correct ?

If the authors were to repeat their analysis of observations using instead monthly mean flash rates, would the thermodynamic model prove more important in explaining the observed lightning ?

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