Atmos. Chem. Phys. Discuss., 14, C4382–C4386, 2014
www.atmos-chem-phys-discuss.net/14/C4382/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Using cloud ice flux to parametrise large-scale lightning" *by* D. L. Finney et al.

Anonymous Referee #2

Received and published: 2 July 2014

This paper deals with a new lightning parameterization to be used in global models. The authors use the ERA reanalysis model that supplies the best estimate of global large scale files every 6 hours. The authors compare their new ice flux parameterization to other commonly used parameterizations in the literature.

I have a number of major comments, and a few minor comments:

1) All lightning parameterizations depend on two factors. First, the reliability of the link between some cloud parameter (height, precip, ice flux, etc.) and lightning in reality; and second, the ability of the model to reproduce correctly these cloud parameters. We may have a perfect relationship between CTH and lightning in the field, but if the model or reanalysis does not produce the correct CTH, the lightning will be incorrectly pre-

dicted. On the other hand, if the cloud parameters are correctly predicted, it is possible the parameterization is not exact, leading to erroneous results. The authors do not address this point at all. How well does the reanalysis predict CTH, ice flux, convective precip, etc.?? Before knowing this it is impossible to say which parameterization is better or worse. Is one better than the other because the parameterization is better, or because the reanalysis does a better job predicting the cloud parameter? How do we know?

In addition, every model and reanalysis will have its own meteorology, own cloud parameterization, own physics. So how can we say that if a parameterization is better in the ERA reanalysis, it will also be better in any other GCM? And if we cannot reach such a conclusion from this study, how could other researchers benefit from this study? Can these results be transferred to another climate model? If anything it would be interesting to compare the ICEFLUX parameterization in many different models to see how variable or stable to lightning distributions will be. If the results vary enormously from model to model (due to the different cloud parameterizations) then what is your conclusion?

2) The parameterization itself is not clear. How does the ERA reanalysis determine the ice mass at -25C? As you know water can exist down to -40C as supercooled water. This is also important for the electrification of clouds. When does water become ice in the model? What is the threshold? What fraction of the water at -25C is liquid? How sensitive are the results to this threshold? This needs to be addressed if you are proposing for others to use this parameterization. Furthermore, the parameterization should be developed using instantaneous observations of ice flux (model) and lightning (LIS). The orbit data from LIS is available and the observations of lightning (within 90sec) are available for individual clouds. You should take the ice flux for the same pixels in the reanalysis for building your parameterization. On the other hand, if you want to use only the monthly mean lightning and ice flux, then you should later only calculate monthly mean lightning using the reanalysis. You cannot calculate the lightning

every 6 hours when the parameterization was build using monthly mean values. Your parameterization is also sensitive to c the cloud cover in the model. How well does the reanalysis simulate cloud cover? Have you compared the convective cloud cover with satellite data? If the cloud cover is wrong, the whole relationship is wrong. And cloud cover is one of those sensitive parameters that are highly variable from GCM to GCM. Do you want your parameterization to be so sensitive to the model used?

You have also used the Price and Rind (1993) relationship to estimate total lightning for the parameterizations compared to the ICEFLUX method. This includes information about the CTH which may be in error in the ERA reanalysis. Hence, you may be adding problematic parameters to the other parameterizations by introducing the p factor in their calculation. So it is very difficult to know what is causing the differences between the methods.

3) You need to know that while the LIS data is the best global lightning data set to date, there are still many problems with these data. First, the satellite only samples a fraction of the true lightning, and only for 90sec per storm. Second, we know of problems over South America due to the South Atlantic Anomaly (SAA). Third, the diurnal cycle is problematic when looking at individual months of data. Hence, some of the differences in the comparisons with LIS may be also due to sampling problems of LIS (especially in the SAA region).

I would recommend looking at other regional lightning networks that give large scale lightning data (eg. UK ATD, WWLLN, LINET, STARNET) continuously in space and time, even if mainly from CG lightning. This would also allow you to develop a better parameterization using ERA reanalysis ice flux vs, regional lightning over 6 hours.

Minor comments:

Line 50: I should point out that the CTH parameterization did not use any model, but rather satellite cloud data from ISCCP which is the best representation of the real world. It therefore appears that any problems with this parameterization in models

or reanalysis is due to the model CTH simulation that is problematic (whether due to model horizontal of vertical resolution, intensity of convection, or other reasons).

Line 56: "more-or-less". How else would you get charge separation? I think this is accepted, not more-or-less.

Line 59: heavier

Line 78, 92: the resolution of $\sim$75km is still quite coarse to get any microphysical information out of these clouds. How do you determine the cloud cover in the box? How do determine the fraction of supercooled drops vs. ice?

Line 124: estimates

Line 127: that a 5-year

Line 154, 207: One the one had you claim the CTH is problematic in the reanalysis, while here you use CTH to correct for total lightning. Seems a contradiction.

Line 166, 170: Is not 1.09 a 109% increase?

Line 262: How sensitive is the parameterization to the cloud cover determination?

Line 301: Until you can simulate the cloud microphysics, GCMs will not be able to differentiate between graupel and ice.

Line 316, Fig. 4: I see more that 3 cells that are not stippled. Please explain.

Line 336: I suggest using monthly data, if this is how the parameterization was developed.

Line 363: Yes, but again how can you say this is a problem of the parameterizations, and not a problem of the reanalysis fields?

Line 376: don't forget LIS has problems over SAA

In conclusion, it is not clear to me from this paper that the ICEFLUX parameterization

is better that any of the other previous parameterizations. The authors have not convinced this reviewer that their methodology is better. And it is not clear whether the mismatch with the LIS data is due to the parameterizations or the reanalysis products used. Hence, it is difficult to know the usefulness of this study for others in the field. I think this paper needs major revisions and additions before it can be considered for publication.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 17817, 2014.