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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Assessing and improving cloud-height based parameterisations of global lightning flash rate, and their impact on lightning-produced NO_x and tropospheric composition" *by* Ashok K. Luhar et al.

Anonymous Referee #3

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General remarks:

The paper by Luhar et al proposes a new CTH-based lightning scheme that considerably improves the maritime behaviour of the original CTH parameterization proposed by Price and Rind in 1992 (PR 1992).

The paper begins with is a relatively clear introduction (section 1). Then in section 2 the authors first describe the chemistry-climate model used followed by a description on how the lightning scheme and the NO per flash were implemented in the model. In

subsections 3.1/3.2/3.3 they describe three previous CTH-based schemes (including the one proposed by Boccippio in 2002 that inspired the authors' new lightning scheme) and the new lightning parameterization proposed (subsection 3.4). In subsection 3.5 the different model runs to be compared in the paper are commented. Subsection 3.6 is devoted to compare the lightning flash rates derived from four model runs with those from satellite observations.

The modelled LNOx, its vertical distribution and verification are described in section 3.7.1 (global LNOx), 3.7.2 (adopted vertical distribution of LNOx) and 3.7.3 (tropospheric NO2 verification), respectively. Finally, section 4 of the article is devoted to comment the impact of the new lightning scheme on some key chemical components of the troposphere with specific subsections for NOx (subsection 4.1), O3 (subsection 4.2), OH (subsection 4.3) and CO (subsection 4.4).

The paper is overall well written but requites some important clarifications. The figures need some improvement. In particular, the numbers inside Figures 10, 13, 14 and 17 are not readable and should be larger. Also the numbers in the vertical and horizontal axes of Figures 5, 6, 10, 11, 13, 14, 15 and 16 are small and not very visible. The numbers in the color bars should also be bigger.

Some more detailed comments:

Section 1

What do the authors mean in line 5 of page 4 with "... The performace of the PR92 flash-rate parameterizations has not yet been tested properly for their land and ocean components separately" ?.

There are already previous works indicating that the PR92 scheme exhibit large land-ocean biases. This has been already been pointed out by Finney et al 2014, 2016 and others as the author themselves state in the lines 3-5 of page 4. Please rephrase this sentence or make it clearer.

Section 2

What is the time step of the ACCESS-UKCA model used?.

The authors state that their ACCESS-UKCA setup includes some additional modifications compared to the base UM-UKCA v8.4 model. These changes seem to produce an increase (see line 20 of page 5) in the tropospheric O3 burden of about 12 %. Have the authors compared this increased O3 burden with observations?.

Section 2.1

What is the convection scheme used in the ACCESS-UKCA model?. This is important and should be clearly stated since any lightning scheme will be sensitive to the chosen convection parameterization. Please write it in the manuscript for the sake of clarity.

Did you use / implement the spatial calibration factor (c) introduced in PR92 and shown in equation (3)?. This is not clearly stated.

The authors should advise readers that the use of the method suggested by Price and Rind GRL 1993 to distinguish between CGs and ICs was only derived considering a number of thunderstorms in the US. However here the authors assume worldwide applicability. The authors should mention the restrictions and assumptions underlying such method. Also, it would be good if authors could say something about how the assumptions of the PR93 method can affect the results of the paper.

Section 2.2

The authors seem to assume that the energy of CG and IC flashes is the same. Is it so?. If yes, please state it clearly and add appropiate citations supporting this assumption (for instance Ridley et al 2005, Ott et al 2010 and / or others). It is also assumed that 330 moles NO / flash is produced independently of whether the flash is CG or IC. Why 330 ?.

In this paper the amount of NO per flash is prescribed to 330 moles NO / flash. What

is the underlying reason for choosing 330 moles NO / flash instead of the 310 moles NO / flash concluded by Miyazaki et al 2014?.

By using equation (15) in Price et al JGR 1997 the authors could estimate (assuming the energy per flash) the number of NO molecules produced per joule. This an interesting magnitude to show and it is possible since they have computed the amount of global LNOx (Table 4, equation (21)) and are assuming that the energies of CG and IC flashes are the same, and that aproximately 75 % of predicted total flashes per second (Table 1) are CGs while 25 % are ICs.

In line 16 of page 7, the authors comment a little bit how the produced amount of LNOx is vertically distributed. It is mentioned that it is distributed evenly vertically from 500 hPa (aprox 6 km) to the cloud top for IC flashes, and from surface to 500 hPa for CG flashes. What is the rationale and / or the physical, chemical and transport reasons (and / or possible observations) for choosing / supporting such vertical distribution ?. This is a bit obscure to me.

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