Review of 'Lightning NOx, a key chemistry-climate interaction: impacts of future climate change and consequences for tropospheric oxidising capacity', by Banerjee et al.

General Comments

This paper presents results from a series of model experiments with the chemistry-climate model UKCA, investigating the influence of climate change on changes in lightning NOx emissions, and the consequences for future levels of tropospheric ozone and hydroxyl radical. The paper is interesting and well presented, but I do have some suggestions for adjustments.

My main concerns are about how methane is treated and the consequences for how the simulations are interpreted. Methane concentrations are held fixed (p8757 l16). This is done for reasons of computational efficiency, but this is an artificial model imposition, and it has important influences on model results. Clearly, in the real world, methane concentrations are not held fixed and would be free to respond to any changes in OH.

Fixing methane concentrations in the model means that when lightning NOx increases, and OH increases, and the methane lifetime reduces, methane concentrations cannot respond. Since methane is an ozone precursor, this mechanism of influencing ozone is not included in the simulations. If methane concentrations were not held fixed, methane would slowly adjust to a new (lower) equilibrium concentration, and the resultant change in ozone would be less than is reported. Similarly, the changes in OH would also differ from those reported. The differences in the changes in ozone and OH between a fixed methane run and a run where it were not held fixed are likely to be substantial, and this must be more clearly acknowledged than it currently is (e.g., this major caveat is not mentioned in the Abstract). It is relatively easy to calculate the approximate impact of holding methane fixed on the results, e.g., following the methods described in Stevenson et al. (2013).

If this issue and the minor comments below are addressed, then I recommend this paper is published in ACP.

Specific Comments

p8759 l8: Reference is made to the interactive calculation of LNOx, but this hasn't been described yet. I think it makes more sense for the last paragraph of Section 2.1 to probably move to the end of Section 2.2.

p8759 l10-11: '*The Base(fLNOx) and Base runs should be identical; indeed there are negligible differences…*' This sounds like they are definitely *not* identical. It is not quite clear how different these two base runs are. Are they slightly different because the LNOx emissions are specified differently? In the Base run, are LNOx emissions varying every timestep? (If so, what is the timestep?) Are these high time resolution variations in LNOx emissions also carried over to Base(fLNOx)? At this stage, you haven't described exactly how the LNOx emissions are specified – and even the upcoming Section 2.2 doesn't describe the temporal variability in the fully interactive run.

p8759 l27: *'…H is the cloud-top height (km)…'* Is H height above sea-level or above the surface topography? UKCA uses 'hybrid height'; is H really hybrid height?

p8760 l9: Why are the NOx emissions distributed evenly in log-P from 500 hPa to the cloud top? How sensitive are your results to this (relatively arbitrary?) choice?

Section 3.1/p8762 l8/throughout: In the Introduction, you define LNOx as '*NOx produced from lightning'*. So I think LNOx refers to the fraction of NOx derived from lightning, or 'lightning NOx' for short. In other places you refer to 'LNOx emissions'. But in other places, you just use LNOx with the implication that you mean LNOx emissions. This may sound pedantic, but I think the text should be tightened up to remove any ambiguity; this would certainly be the case for NOx vs. NOx emissions, so I think the same applies to LNOx vs. LNOx emissions. For example, the text (and figure caption) describes Figure 2a as showing the correlation of P(Ox) and LNOx, but the figure actually shows LNOx emissions. If every reference to LNOx really means the emissions, then I suggest change the opening definition.

p8763 l27-28 '*…decrease in ozone is seen throughout the troposphere…primarily due to increased humidity in a warmer climate.*' This is a relatively old and well known result, and probably deserves a reference (e.g., Thompson et al., 1989; Johnson et al., 1999; Doherty et al., 2013).

p8764 l4 (l10; also p8766 l14) Figure 4 does not show *zonal* changes.

p8765 l6-8 Better to use years (rather than months), the 'normal' unit used for methane lifetime.

p8765 l9 '*This might have implications…*' I think there is little doubt: it *will*.

p8766 l21 '*P(Ox) contributes to the tropospheric ozone burden…'* This is a slightly odd way of putting it: it is quite possible for P(Ox) to increase whilst the ozone burden decreases (e.g. if Ox destruction increases by more than P(Ox)).

p8767 l21-26 Indeed – the fact that your experiments used fixed methane concentrations have important caveats for both the reported changes in ozone and OH. These major caveats needs to be reflected in the Abstract: the quantitative results quoted there would change significantly if the experiments were repeated with methane concentrations free to adjust, and in the current Abstract, the reader does not know that methane concentrations are held fixed in the modelling.

p8777 Figure 3. Maybe it is also worth plotting the difference between Figure 3a and 3b?

References (additional to those already in the paper)

Thompson, AM, et al. (1989) Sensitivity of tropospheric oxidants to global chemical and climate change, Atmos. Environ. 23, 3, 519-532

Johnson, CE, et al. (1999) Relative roles of climate and emissions changes on future tropospheric oxidant concentrations, J. Geophys. Res., 104, D15, 18631-18645

Doherty, RM, et al. (2013) Impacts of climate change on surface ozone and intercontinental ozone pollution: A multi-model study, J. Geophys. Res. Atmos., 118, doi:10.1002/jgrd.50266