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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloud-resolving chemistry simulation of a Hector thunderstorm" *by* K. A. Cummings et al.

Anonymous Referee #1

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This paper reports to my knowledge for the first time on a fine-scale simulation of a tropical thunderstorm that occurred over northern Australia on 16 November 2005. On that day, various instruments on board aircraft sampled physical and chemical properties of that thunderstorm and the authors compared these observations to the simulated properties of the storm. In general, the cloud-resolving model –with a simple chemistry scheme– resolves the characteristics of the storm quite well. The authors make the case that the Hector storm is probably as effective as mid-latitude storms in generating NOx from lightning. This contrasts with the view that mid-latitude storms would be more effective in producing NOx from lightning, because the wind-shear tends to be stronger for those storms.

I also think the paper really presents a nice testcase for assumptions often assumed in

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simulating lightning NOx productions. The temporal and spatial evolution of a thunderstorm, the effects of uplifted boundary layer air on anvil outflow composition, the choice for vertical (bulk) distribution of lightning NOx, etc. are all discussed in the perspective of how well they represent observed characteristics of the thunderstorm, and how well they relate to recent proposals on how to simulate LNOx production (Huntrieser and Ott-papers).

In spite of all this, I'm having difficulties in interpreting the results of this study. Hector is a rather well-documented, but to my knowledge also rather exceptional thunder storm. It does not become clear from the paper whether the results of this study now unsettle the hypotheses put forward by Huntrieser and Ott that tropical lightning NOx production is fundamentally different from mid-latitude LNOx production. The authors seem to suggest that this now seems the case for Hector-type storms, but to what extent are such storms representative for tropical thunderstorms? The authors indicate that their findings hold for a tropical island, and invoke the presence of different wind directions between the base and top of the cloud as a possible mechanism for longer channel lengths (that would explain stronger LNOx production). But the hypothesis that different wind directions may lead to longer lightning channels is not substantiated, and it is all but clear how important these islands are in driving the overall tropical LNOx production, or how general the mechanism of different wind directions might be. It might be possible that Hector is just a particular sort of thunderstorm, that is not at all representative for how the bulk of tropical lightning works. I think the authors should discuss these aspects in more detail and provide some guidance on how to interpret their results in the global (or at least tropical) LNOx production debate.

I find the paper at times very wordy. For instance the description on page 16720 of how model and aircraft CO were exactly compared, could also be moved to the caption of Fig. 11. Also the discussion of Fig. 11 and Table 5 itself is on the long side. The discussion on P16724, L5-20 is also long-winded; I think just mentioning the type of potential errors (errors in the spin-up state, simplified vertical motions in

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the model, representativity of the observations) would be sufficient here. On P16757-16758, Figures 16 and 17 could be merged into one figure with two panels.

Specific comments

P16715, L4-6: Please clarify whether the TUV code accounts for overhead cloudiness. This may be especially important for in cloud chemistry, and its dependency on photolysis rates.

P16716, L8: I don't think CAPE has been defined.

P16716, L28-29: I think the authors should provide some more insight into why convection comes to early in the model. Since the simulated cell is roughly as long-lived as the observed one, it seems that the effects of convection are captured well by the model, so what is it that goes wrong in the onset?

P16723, L15-16: are the directly observed and (photochemical model) inferred NOx concentrations consistent, given their different vertical intervals? Please clarify.

P16724, L22: typo Bierle should read Beirle.

P16725, L9-11: indeed OMI's overpass at ∼13:30 hrs local time may be too early to capture the freshly produced lightning NOx signal. But have the authors checked whether the subsequent orbit also passed over the region of interest? If that were to be the case, OMI would have observations at ∼15:00 hrs local time (at large off-nadir viewing angles but nevertheless useful).

Section 5.4: this section appears rather speculative, and I think the paper could do without it. Especially lines 9-11 on P16728 need more justification. It is straightforward to evaluate whether O3 loss in the cloud is caused by titration by NO with the model, and I recommend the authors do so. Also the comparison with the observations is thwarted because VOCs are missing from the chemistry scheme, and the representativity of the measurements is questionable. Of course the scientific question of how much O3 is produced from a thunderstorm is a very important one, but this section

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does provide the start of an answer.

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