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

**Anonymous Referee #2**

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This study presents cloud-resolving 3D chemistry and transport simulations of the tropical thunderstorm Hector over the Tiwi Islands north of Darwin and compares simulated distributions of NO<sub>x</sub> and other trace gases with aircraft observations taken during the joint SCOUT-O3/ACTIVE campaign in 2005. The main objective is to better quantify NO production per lightning flash by matching model simulated and observed concentrations and making use of lightning observations performed by the lightning detection network LINET operated during the campaign. The study builds on a tradition of observational and modeling studies of Hector thunderstorms but has a number of distinct and new features as well as a clear focus on lightning NO<sub>x</sub> that make the paper worth publishing. These novel features include: 3D simulations using a model with a wet phase chemistry scheme, integration of lightning flash observations using a lightning placement scheme in the model, and comparison of simulated trace gas statistics in

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the thunderstorm anvil with rather extensive aircraft observations.

## MAIN POINTS

The paper is generally well written and structured, the modeling tools are appropriate and the results are relevant. Nevertheless, I found that some of the results and conclusions are not well substantiated. A main conclusion of the study, for example, is that a NO production of 500 moles per flash matches the aircraft observations best, a value which is consistent with previous studies for midlatitude and subtropical storms but higher than previous reports for tropical storms. I personally find the separation into midlatitude, subtropical and tropical thunderstorms introduced in previous lightning NO publications and adopted here problematic. Rather than associating lightning NO production with fundamental dynamical and microphysical (and electrical) characteristics of the storm, this approach makes a classification merely by region or latitude neglecting the fact that in a given region thunderstorms may have largely different characteristics (isolated storms, organized systems like MCS and squall lines, frontal convection, etc.). Hector is a good example for this problem. The study of May and Ballinger (Mon. Weath. Rev. 2007) nicely demonstrates how the properties of thunderstorms over the Tiwi Islands change between the pre-monsoon (buildup) season (the typical Hector season) and the monsoon season. They classified Hector as a continental storm whereas the monsoon convection over the same location is classified “oceanic” (see also Keenan and Carbone for classification of continental and maritime storms, QJRMS 1992). Continental tropical thunderstorms such as Hector follow a pronounced diurnal cycle, are more vigorous and electrically active, and extend to higher altitudes than oceanic/maritime systems. May and Ballinger (2007) showed that the background meteorology is largely different in the two regimes explaining the different storm characteristics. I don’t agree with the other referee that Hector is an exceptional thunderstorm (what is exceptional is that it has been investigated in much more detail than other storms due to the excellent local infrastructure) but it is only representative for one class of thunderstorms which is certainly not the most common class in the trop-

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ics. It would be much more suitable to discuss tropical thunderstorms and their LNO<sub>x</sub> production characteristics in such a framework and I am sure it would help explain some of the seeming discrepancies between different studies of tropical convection. Note also that storms over the Tiwi Islands develop in a wide range of shear (Keenan and Carbone, 1992) and therefore also a simple classification of tropical systems as low-shear storms doesn't apply. For this paper I strongly recommend to discuss the results with respect to the storm classification of May and Ballinger (2007).

Another somewhat weak point is that although the results based on a source of 500 moles NO per flash matches the observations rather well, the uncertainty in this estimate is not well explored. A sensitivity simulation is performed with a 10% smaller source strength but whether the resulting NO concentration is a simple linear function of the source is not addressed. Simulated and observed NO<sub>x</sub> in the anvil are compared in Section 5.3.1 by selecting the model layer that best matches the observations. It is unclear at this point whether evaluating the model at the true altitude of the measurements would lead to an overestimation or underestimation of NO<sub>x</sub>, and how this affects the conclusion that 500 moles/flash matches better than 450 moles/flash. If the simulation had been done with 600 moles/flash, would you have chosen an even lower level for the comparison and claimed that 600 moles/flash are matching best?

The low sensitivity of the CO results presented in Section 5.2 to the different LNO<sub>x</sub> scenarios is not at all surprising. The only immediate effect I can see is the impact of LNO<sub>x</sub> on OH levels but given the long lifetime of CO against oxidation by OH it is obvious that the CO results are little sensitive to the LNO<sub>x</sub> scenario on the time-scales considered here. It is therefore sufficient to discuss the CO-results only for one of the two scenarios and to mention that the results are essentially the same for the other scenario. Contrasting the results for the high and low LNO<sub>x</sub> scenarios is misleading as it implies an important influence of the scenario.

Section 5.3.2 on NO<sub>2</sub> column is not very convincing and little relevant in the context of this study since no direct observations are available to compare with. Furthermore, the

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quality of the simulated NO<sub>2</sub> values strongly depends on the quality of the simulated radiative transfer, but this seems to be done in a very simplistic way accounting only for clear-sky conditions which is by no means sufficient for representing NO<sub>2</sub> in an anvil cloud. The discussion on lines 12-29 on page 16725 could be shortened substantially. It would be sufficient to mention that the range of simulated values is similar to that reported by Ott et al. (2010) for model simulations but higher than direct observations from OMI obtained during the TC4 campaign (how large were the OMI columns reported by Bucsela et al. 2010? I couldn't find a number in the manuscript). It is also not very interesting to learn that the background NO<sub>2</sub> column outside of the cloud were similar to those other studies. What would you conclude from this?

## SPECIFIC POINTS

P16704, line 3: typo Vaughn -> Vaughan

P16710, line 18: This would probably be a good place for referring to the May and Ballinger (2007) study.

P16710, line 19: In my view there is no need to investigate storms for a greater variety of regions but there is a need to characterize LNO<sub>x</sub> production for a greater variety of storm types and environmental conditions.

Section 2.3: Another important numerical modeling study which is missing in this review is that of Chemel et al. (Mon. Wea. Rev. 2009), which simulated the Hector storm of 30 November 2005 observed during the same campaign.

Section 3: LINET also provides information on the vertical placement of (IC) flashes. This information appears not to be used. Why? Is the placement of the upper mode isotherm at -60°C consistent with the LINET observations?

Page 16718, line 7: Could the time shift of 2 hours between simulation and true storm development be due to the simple treatment of the surface as being a sensible heat source of 40% of solar flux? What about latent heat fluxes? Wouldn't that be relevant

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particularly in the morning, e.g. because the soil is still moist from precipitation on the previous day?

Section 5.1, discussion of Figure 8: There are not only differences in the storm core as represented by the 50 dBZ contour but also notable differences in the anvil structure which seem to be relevant for later discussions. The simulated anvil is sloping downwards with increasing distance from the core more strongly than the observed anvil. This could point towards too strong sedimentation in the anvil which would eventually lead to a vertical separation between non-sedimenting trace gases and sedimenting hydrometeors.

Page 16720, line 13-14: It should be “Tables 3 and 4” instead of “Tables 2 and 3”

Page 16722, line 23: As far as I understand, the TUV  $j(\text{NO}_2)$  values were computed only for clear sky conditions. In an optically thick anvil cloud one expects elevated  $j(\text{NO}_2)$  values near the top but lower values as compare to clear-sky conditions further below. How does this uncertainty affect the results? The instrument was alternating between an NO mode and a NO<sub>x</sub> mode. How well do simulated NO<sub>2</sub> values agree with NO<sub>2</sub> values that can be deduced by subtracting average measured NO from average measured NO<sub>x</sub> values in the anvil? Is the computation of clear-sky  $j(\text{NO}_2)$  values a major uncertainty or rather irrelevant?

Figure 6: It would be nice to see the continental outlines and the Egrett flight track during the anvil crossings in this figure.

Figure 14: The continental outlines and wind arrows are almost invisible.

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