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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The effect of lightning NO_x production on surface ozone in the continental United States" *by* B. Kaynak et al.

Anonymous Referee #1

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Summary of the Paper: This paper addresses an important science question that what is the effect of lightning NOx emissions on the surface ozone, in particular if they can affect the distribution of regional high ozone events. The paper is well organized, easy to read, and the team's knowledge in this area is well reflected. The main focus of the analysis presented in the paper is if the addition of lightning NOx production in air quality modeling would affect background ozone leading to a change in the air pollution control strategy. Because of its policy implication, one should consider robustness of the presented results, in particular how sensitive is the results to the model configuration used. It is well known that modeled ozone production by NOx emissions is very sensitive to the resolution of the model. An excessive ozone production is estimated if the NOx emissions are dispersed in a large grid box initially because biogenic VOCs

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are already well dispersed. Because physical dimension of lightning is extremely narrow and intermittent, I suspect if the present grid models are capable of quantifying such effects properly.

The reviewer would like to ask the following questions:

(1) Wouldn't the impact of lightning production of NOx behave more like an addition of NOy species in the atmosphere? That means that direct addition of NO and NO2 into the photochemical system with 36-km grid resolution may interfere with the radical balance expected in the grid model, thus affecting reactivity of the system. NOy can be transported longer distance and also subject to direct deposition (and washout) as well as involving in the heterogeneous reactions.

(2) Spatial and temporal allocation of cloud and precipitation related process is more an art form than an exact modeling. More often than not, we are forced to implement such process as a phenomenological parameterization than an actual physical processor. Although the authors explained clearly what they have done to distribute the NOx emissions from the satellite-estimated IC/CG lightning information, I see a few issues with the current approach. First, have you checked how well the MM5 simulated cloud, precipitation, and moisture fields correspond to the satellite observed cloud and precipitation? Second, the paper reports that it is rare that the satellite observed IC/CC lightning NOx could not be allocated to the target or adjacent cells. Wouldn't it because a very low threshold value (not described in the paper, but I assume "0" cloud fraction) is used to declare there is no "modeled" cloud to allocate lightning NOx emissions? Because the NOy species will go through heterogeneous processes in air quality models, distribution bias of cloud/precipitation must be accounted for.

(3) Again, is the vertical resolution – 13 layers – enough to represent the effect of lightning NOx emissions on ozone assessment? Can we resolve the different effects of IC/CC discharge (although the same emission rates are used in this paper) vertically with this model resolution?

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(4) The paper uses O3-NOz plot to estimate the effects of lightning NOx on the background ozone (Figure 11). The ozone-NOz plot is supposed to represent the number of moles of ozone produced for each mole of NOx reacted. Usually, the background ozone (which varies significantly from place to place) concentrations are initially subtracted from the maximum values in the scatter diagram to represent the ozone production rate. The scatter in Figure 11 is too large to justify estimation of both the offset as the background values and slope to represent the ozone production rates.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 5061, 2008.