The manuscript introduces an updated scheme for calculating the lightning NOx emissions by using the gridded hourly NLDN flash data. The updated scheme has improvements in simulating the NOx emissions compared with the previous scheme using the monthly NLDN flash data, which also requires two different scale factors in determining the lightning flash. The study also developed another scheme using linear and log-linear parameters, which is suitable to use when the hourly lightning flash data are not available, or the air quality simulations are set up to run real-time forecast, or future climate simulations. I personally appreciate the content and scope this study introduced. Natural source, such as lightning, will play an important role in determining the O_3 attainment, especially in the western U.S. I think the manuscript is acceptable to be published by the journal. I have some comments that need the authors to address.

We thank the reviewer for his/her positive comments and recommendation for publication.

Major comments:
Allen et al. (2010, 2012) developed the lightning scheme also using the flash rates from the NLDN, the same as the author proposed. I did not see what updates or advances the authors made considering that. Is that mainly because Allen et al. used monthly flash rates, while the authors used hours? Please elaborate.

The lightning scheme in CMAQ5.0 was developed by Allen et al. (2010, 2012) and was based on monthly NLDN data. In order to redistribute the monthly data into the modeling domain for hourly simulations, additional factors are applied based on the meteorological model predicted convective precipitation. Yes, the use of hourly NLDN data is one of the advances over the existing scheme in CMAQ5.0. When the hourly NLDN data are used, the lightning flashes are directly converted into lightning NO without dependence on the quality of the meteorological fields. In addition, the averaging over neighboring grid boxes and use of both linear and non-linear fits in the pNLDN scheme results in a better fit than in Allen et al.

The quality of the figures embedded in the manuscript are really low. I suggest the authors prepare clear plots when they submitted the manuscripts for review.

In the revised manuscript, the plots are separate from the main text and the resolution is improved.

Minor comments:
Line 28: suggest to remove “scheme and associated LNOx”

Following the reviewer’s suggestion, we have modified the sentence to read: “We first document the existing LNOX production and vertical distribution algorithm”.

Line 64-65: the authors should add some references listing how the previous studies about lightning NOx affect surface ozone, before the authors could make the conclusion of the importance of LNOx.

Many of the references already cited earlier in this paragraph (Murphy, 2016; Ott et al., 2010; and Schumann and Huntrieser, 2007) discuss and summarize estimated impacts of lightning NO_X on surface ozone. We believe these studies provide adequate justification for the importance of LNOx on atmospheric chemistry and resultant air quality.

Line 82: remove “For instance”

Thanks. It is revised as suggested.

Line 94: use abbreviations for “could-to-ground” since it was defined before

Thanks. Change has made.

Line 97-106: I suggest moving this parts into methodology.

Thanks for the suggestion, but we think that this description better fits in the introduction section.

Line 108: what is old and new scheme? It is confusing since the manuscript mentioned at least 4 schemes: previous parameterizations; Allen et al. 2010; hourly NLDN, and the newly developed parametrization scheme.

Following the reviewer’s suggestion, we have now modified the sentence as “a preliminary assessment of the spatial and temporal distribution of LNO columns in the existing (mNLDN), updated (hNLDN), and newly developed (pNLDN) schemes.”

Line 120: I suggest to remove this paragraph since this lightning NOx option was not discussed later in the manuscript any more.

There are two purposes for developing this manuscript: 1) to update the existing schemes using the most up-to-date information and develop a generic scheme for use without observed lightning data, 2) to document the schemes used in previous CMAQ releases. Even though the preliminary parameterization scheme wasn’t discussed later in the manuscript (due to production of unrealistic high LNO rates), we still want to keep it because it existed in earlier CMAQ versions. We feel that inclusion of this brief discussion would be useful to model users who have also used previous versions of the modeling system and these earlier LNOx parameterizations.

Line 131: to convert “what”?

To address the reviewer’s question, we have reworded this discussion to (line 164-166 of the revised manuscript as “First, a global factor (lightning yield) is applied at each grid cell to produce lightning flashes from model CP. Then, a local adjustment (LTratio) is applied at each grid cell to ensure that the local CP- and NLDN-based flash rates match.”

Line 167-168: remove “Wang et al (1998) ….”

Thank you for catching the typo. It is now removed.

Line 170: change to “and NO produced by CG flashes at a lower layer of the atmosphere (600hPa)”

Thanks. Change is made as suggested.

Line 211: CP was already defined in previous content

Thanks. It has been revised.

Line 211: In section 4, the authors used the different version and configurations of WRF to explain the performances of different years LNOx simulations. So here it would be helpful to show the verions/configuration of the WRF from 2002 to 2014.

To address the reviewer's suggestion, we have reworded the discussion on lines 251-253 of the revised manuscript as: "We analyzed meteorological fields generated by the WRF model simulations from 2002 to 2014 over the continental United States to examine the relationship between the observed lightning flashes and the predicted CP. Though the WRF model has evolved over a few versions (from version 3.4 to 3.8), the Kain-Fritsch (KF) convective scheme (Kain and Fritsch, 1990) was used consistently in simulations for all years." We have also added a data description and model configuration section after introduction to address the model configuration issue.

Line 245: convective precipitation was defined earlier

It is now revised.

Line 276-277: rewrite this sentence.

This sentence has been rewritten and now reads (lines 305-309)"As indicated in Figure 6, the spatial patterns of slopes generated using data from different time periods for both linear (upper panel) and log-linear regressions (lower panel) are similar except that larger values are created over the Great Plains east of the mountains when the most recent years' data (2009-2014) were used to perform the linear regression .

Line 280: see comments earlier. Please list the differences for the WRF versions.

Thanks, we have now added the information in the new data source and model configuration section.

Line 335-337: how the authors make the conclusions that the poor relations of NLDN flashes and model predicted CP was associated with the poor simulations of precipitation by WRF?

We agree with both reviewers that poor correlation between NLDN flashes and modeled convective precipitation may not completely be attributable to poor precipitation simulation by WRF. We have revised the discussion to now discuss other factors (in addition to WRF simulation errors) than can influence this poor correlation.