General comments:
This paper uses the CMAQ air quality model, driven by WRF meteorology to present various approaches to simulating lightning NOx over the USA. These approaches utilise the NLDN lightning data to: 1) directly determine Lightning NOx (LNOx) in the model, 2) to combine with simulated convective precipitation to determine LNOx, or 3) develop relationships between CP and lightning, that could then be applied to CP simulation when NLDN observations are not available. These 3 approaches are all valid approaches to consider and provide a useful comparison in this work. The authors describe the approaches and show how the models perform in different years in order to determine their robustness. The authors state that applying the NLDN observations directly will provide the highest fidelity LNOx simulation. However, when observations are not available they conclude that the third option of parametrisation based on CP is appropriate, despite some issues which have been well-described.

In general, I am pleased to see such a paper. Very often lightning parametrisations are not well documented so this paper is welcomed. Furthermore, the thorough description of the 3 approaches will serve other modelling groups well should they be trying to decide how best to develop a lightning scheme for their own models. However, the paper is not ready for publication in its current form.

We thank the reviewer for the overall positive assessment of the manuscript and for the constructive comments. Incorporation of the reviewer's suggestions and revisions in response to the comments has greatly improved the revised manuscript.

Firstly, data description sections for the NLDN data, and the WRF driving model, and how it couples to CMAQ, need to be added.

In this application, WRF and CMAQ were run separately (offline). CMAQ used the meteorological fields output by WRF and prepared using MCIP as input files. We have now added a data description and model configuration section after the introduction section to address the NLDN data source and model configuration issues.

Secondly, there is no discussion of appropriateness for chemical transport and reactions of applying a LNOx scheme that does not depend on the underlying simulated convection (through use of CP), as in the case with one scheme presented (hNLDN). This point is not a hindrance for the results of this paper which only looks at the LNOx itself, but it may be an issue should one look at ozone or OH for example.

In all the schemes, the lightning produced NO is treated as an emitted species and its atmospheric flux due to lightning is added to the existing NO from other emissions. This NO then undergoes the same chemical and physical processes as any other emission species does. In terms of the potential mismatch between LNO production and convective transport of other ozone precursors that could occur with the hNLDN scheme, we would recommend running WRF with lightning assimilation (Heath et al., 2016), then convection will be forced to occur at the correct times and locations, and will consequently eliminate any such possible mismatch.

We have provided additional information and recommended the use of lightning assimilation in WRF simulations in Section 3.3: Updates to the lightning module and the LNO production scheme.

Thirdly, the parametrisation developed has relationships that are highly spatially dependent, and therefore the caveats to applying it to different climates must be discussed. In summary, I can see that all the approaches presented here can have their uses, but that better data description and discussion of caveats is needed.

Yes, we agree. We have now revised our manuscript to provide the caveat and removed the obscure description regarding the application in areas outside the study domain.

Specific comments:
L33. “future climate studies”: I think it is debatable whether the model can do this because it does not seem necessary to me that the spatial dependency of relationships developed for fig5 will hold in different climates. The authors must at least include discussion of this in the main text and justify their opinion.

LNOx emission estimates for future climate scenarios are needed to adequately assess both air quality and atmospheric deposition amounts under these scenarios. We include the description here to help serve this need though the development, application and evaluation of the pNLDN approach that attempts to parameterize LNOx emissions as a function of convective rain using historical data. We nevertheless acknowledge the relationship may not hold if the climate changes dramatically in a such that the range of change is outside that of the magnitude of the historical data. We have now added the caveat in the summary and conclusions.

L33. “simulations focused outside the NLDN region”: Given the spatially dependent relationships of the model produced, and that there have only been produced over the NLDN region, I don’t see how a model has been developed that simulate anywhere else. Please can authors clarify how their model can be applied elsewhere in the main text. Otherwise, I can see that a method has been developed that could be applied elsewhere where lightning observations exist, so a statement to this effect could still be included.

We agree with the reviewer that the current discussion is confusing. We have removed this description in the revised manuscript.

L65. I think the Murray (2016) paper on lightning and air quality would be nice reference to include here https://link.springer.com/article/10.1007%2Fs40726-016-0031-7

We already reference this publication at multiple locations in the manuscript text.

L84. This is just one paper that has provided extra evidence on LNOx per flash, and it looks only at the gulf of mexico. For this statement you need to reference at least a selection of the raft of studies that have added to this estimate since the 2007 schumann and huntrieser review. Here’s some clues, they are not all required (it’s not a review) but hopefully you can find some of them (Huntrieser et al., 2008; Cooray et al., 2009; Huntrieser et al., 2009; Bucsela et al., 2010; Ott et al., 2010; Huntrieser et al., 2011; Miyazaki et al., 2014; Pollack et al., 2016)

We have now added Bucsela et al., 2010; Huntrieser et al., 2009 and 2011; Ott et al., 2010, in addition to Pickering et al. 2016.

Before section 2. Need a description of the NLDN since it is integral to this paper. Notably, is it cloud-to-ground or total lightning? You also need some basic description of the WRF model and its version used here, since that is driving the convective precipitation variable that is integral to the paper.

Thank you for the suggestion. We have now added the necessary information in the data source and model configuration section.

L132. "A local adjustment is applied…": Is this done at the end of each month simulation. Or maybe CMAQ is not run at the same time WRF? You need to clarify these details of the model setup, for it to make sense how the LNOx scheme is being applied.

CMAQ is run offline, i.e., WRF fields were processed using MCIP to provide hourly input for CMAQ simulations. The local adjustment factor varies monthly and is applied at each hour to the input LNOx emissions. We have now provided the clarification in the "data source and model configuration" section.

Fig1. Ideally all the starting points (inputs) would be on the left, leading then to the outputs on the right. I believe these starting points are "NLDN raw data", "ICCG climatology", and "Gridded met data". This would make the flow clearer.

Thanks for the suggestion. In the revised manuscript, we have revised the figure as suggested.

L141. "yield": I find this term confusing. DO you mean NOx per flash? Flashes per CP? Or something else? Please clarify.

Yes, it means the lightning flash yield per unit of CP. We have now provided this clarification.

Eq3. I'm not entirely clear why Ratio_NLDN2CP and LTratio are both needed. Can the same thing not be achieved with LTratio=sum(NLDN)/sum(CP)? i.e. apart from the cap of 50, you are fitting a gradient with zero-intercept to each grid cell? Anyway, this is an existing scheme that's already been used so I guess it is what is. There's certainly no problem, I just think it would be clearer not to have two parameters where one would do. If there is a good reason, then it would be worth adding it to the text.
Eq 3. I think it's kind of being implied throughout that NLDN is just CG flashes. THIs is the only way this equation would make sense to me. See earlier comment, please add a description of NLDN early on where you make this explicit.

Thanks for the comments. It is true that this is an existing scheme that is in the process of being replaced with the updated scheme.
Yes, it would be possible to combine Ratio_NLDN2CP and LTratio into one term that varies with location and month. However, we chose to break it into a term that varies just with month (Ratio_NLDN2CP that converts from mm of convective precipitation per hour to flashes per hour) and a term that varies with location and month (LTratio, unitless). The rationale was that LTratio could be set to 1 for some applications and allowed to vary for others.

L164 onwards: this could be a separate subsection in order to break up the various components a bit more clearly.

Thanks. We have now separated this part as Section 3.2.

L164-173. Would be helpful to show a figure of an example distribution, or better an example IC distribution and CG distribution, and the combined distribution.

An example vertical lightning NO distribution is available in Allen et al. (2012), Fig. 1. In all the schemes as shown by the equations in the manuscript, the total column of lightning NO emissions is generated based on lightning flashes, then the column NO is distributed vertically. We don't vertically distribute either IC or CG flashes. Additional clarification is provided in the new data source and model configuration section.

Eq7. Doesn't the ICCG_ratio need to come into the vertical profile equations somewhere? Possibly this equation. Otherwise the final vertical weightings of the LNOx column will assume equal numbers of each flash type?

The total number of CG and IC flashes are determined first, the vertical profile equations are applied separately, and then the total emissions from each source are summed.

Eq7. Why multiply by 0.2? Isn't that something to do with the ocean grid cells, but isn't the lower distribution for CG flashes?

We agree that equation 7 in its current could be confusing. To address the reviewer's question and avoid confusion for other readers, in the revised manuscript we have recast equation 7 as follows:

$$W = (Bottom_{Frac} - Top_{Frac}) \times F1 + (Bottom2_{Frac} - Top2_{Frac}) \times F2$$

In Equation 7, the weight (W) at each layer is the combination of two distribution density. The sum of each density through all the model column (all vertical layers) should be 1. However, the wider distribution (WMU=350 hPa, and WSIGMA = 200 hPa) extends beyond the top of the model domain, as such the sum of the first distribution is less than 1 (~0.93), while the second distribution (WMU=600 hPa and WSIGMA = 50 hPa) does add up to 1. Thus, in order to ensure that the sum of W through all the layers is equal to 1 and while also resembling the vertical distribution shown in Allen et al. (2012), each distribution needs to be scaled. The scaling factors F1 and F2 in the revised equation thus control the relative contributions of the two distributions to the vertical allocation of lightning emission. In the current CMAQ configuration, F1 = 1, and F2 = 0.2 (default). Also note that the vertical distribution obtained by this methodology is insensitive to the particular IC/CG ratio present in a given grid cell. When information regarding the vertical distributions for IC/CG flashes becomes available (for example, Lightning Mapping Array data could be used to obtain nominal distributions for IC and CG flashes), the factors F1 and F2 could be derived based on the IC/CG ratio at a particular grid cell to possibly represent variability in LNOx vertical distribution more accurately in time and space.

Section 2.2. This updated lightning scheme no longer depends on where convection occurs in WRF. This is often thought to be problematic because in some cases LNOx will not be transported and react as though in a convective environment. Please acknowledge this aspect of the update and give arguments for why it is appropriate.

As we mentioned earlier, in all the schemes, the lightning produced NO is treated as an emission species and it is added to the existing NO from other emissions, then it undergoes the same chemical/photochemical and physical processes as any other emission species do. In terms of the potential mismatch between LNO production and convective transport of other ozone precursors that

could occur with the hNLDN scheme, we would recommend running WRF with lightning assimilation (Heath et al., 2016), so that convection will be forced to occur at the correct times and locations, which will eliminate any such mismatch. We have provided additional information and recommended the use of lightning assimilation in WRF simulations in Section 3.3: Updates to the lightning module and the LNO production scheme.

Fig2. Could add an extra bar for all month correlation.

It has been added in the revised Figure 2.

Fig2. Are all the bars significant? There's only 12 points for each, so worth checking. Could just add a horizontal line at the correlation needed for significance at 5% level.

Yes, this Figure has modified as suggested.

Fig3. Please use grey for where there is no data, and a different colour for where values are close to zero. Also, rainbow colour bars are unappealing for several reasons https://www.climate-lab-book.ac.uk/2014/end-of-the-rainbow/ Please consider changing it.

We have revised the figure as suggested.

Fig3 scatter. You could add lines of best fit for each colour, and for all, to make things a little clearer.

It has been revised as suggested.

L236. "East and west". R1 and R5? Or all regions and R5? Please clarify what east and west refers to.

We have modified both the text and the figure caption to convey that R1 constitutes the West region while R2-R5 constitute the East region.

L241. "…and the log-linear is stronger in the upper value ranage…..". There doesn't look to be much in it to me, the spread around the lon-linear best fit line is similar all the way along. I would just remove this end part of the sentence.

The sentence is revised as suggested.

Fig5. Could add a panel for the NLDN lightning climatology. This will help interpret the relevance of each location.

The NLDN Lightning flashes over the same modeling domain for July 2012 and January and July 2013 were presented in our earlier publication (Heath et al., 2016).

Fig5. These fits by location make it questionable to apply in a different climate. It is quite possible the response of lightning to CP for a location could change in a different climate, e.g. updraught strength could feasibly reduce but CP increase. This could affect the lightning production. You need to discuss this point if you want to include any claim that the model can be applied to different climates.

Thanks, we have revised the manuscript as suggested.

Fig6. Are the log-linear slopes and the intercepts also stable over time? Either add the plots to the figure5 or describe in the text

Additional panel for the log-linear parameters has been added in the revised Figure 6.

L280. Is the same version of WRF not used for the whole time period? This must be explained in a WRF data description section that needs to be added before section 2.

The clarification has been added as suggested in the data source and model configuration section.

L314. "…dynamic cutoff values are used…": please show the resultant column LNOx annual cycle with this approach on fig8

In Figure 9a, the red line is the resultant column LNOx using the dynamic cutoff values. This same line is now also added to the revised Figure 8 as suggested.

Fig9. Why only these 2 years. Would be fine to have many panels of all available years. Or if these years demonstrate a particular point then fine, but it would be good to add an extra panel with a climatology of each model, with stardard deviation bars of each month to show interannual variability

We are able to add more panels for additional years. Since we have been using 2011 and 2013 as the representative years through the manuscript, we would like to stick to these two years to make it consistent. We are working to make additional analysis on all the available years to also assess trends and spatial variability – these will be reported in a future contribution.

L336. "…poor simulation of 2011 precip…": I think this is too strong. You have not shown the precip is poorly simulated, you have shown that the model based on CP doesn't work as well 2011. Lightning depends on many factors that may not be captured by CP variability. One of these factors may have varied in 2011 leading to poor model performance. I think you can say that one possible explanation for poor model performance is if CP was poorly simulated. If you want to say any more then you need to get precip observations and compare to the simulation.

Thanks for the suggestion, we have revised the description to remove the obscureness.

Fig10. Variance of pNLDN looks too low compared to hNLDN. I think it's worth mentioning this in the text.

We have added the description.

Fig11. Colour scale on this figure is not helpful. There needs to be a much larger upper value in order to see the detail. Or a logscale is often useful for such plots.

This Figure has been revised as suggested. Thanks.

L347. "..agree with each other for both years…": I can't tell if this is true because the colour scale lose so much of the detail through saturating.

The revised figure should make it clearer and we have revised the description accordingly.

L352. "..without including observations": although reanalysis is driving the WRF model? This is something that needs to be clarified in WRF data description section.

By the observations here we mean that the lightning flash observations. We have added the clarification in the data source and model configuration section.

L363. Worth commenting here what other schemes are also available in WRF to do the same thing. E.g. I presume a cloud-top height scheme exists?

The WRF model itself doesn't simulate lightning and thus it doesn't provide lightning NO_X production schemes. The WRF-Chem model does contain the cloud-top-height lightning prediction scheme, as well as LNOx schemes.

L386. Perhaps worth adding a bit more positivity regarding your paper along the lines of: "In this paper we have developed and demonstrated a method that can now be applied to new observations as they become available."

Thanks, this point is well taken.

Technical comments:
L120. "inline": Do you mean "online"? or maybe "interactive"?

All these words are interchangeably used in the modeling community. To add a little more clarification, we described it as "based on simulated parameters at run time" versus using static emission inputs.

L139. "…in that…": "in which"?

Thanks, it has been revised as suggested.

Fig1. Can the quality of the image be increased? The text isn't as clear as it could be. Increase the font size too?

Yes, in the revised manuscript, we have improved the quality of all the figures.

L167 "as in Wang…": "drawing from Wang who…"?

Thanks, this part of the sentence has been removed as suggested by Reviewer 2.

Fig3 caption. ".. for other months". "…for other months (not shown)"

Thanks, it has been revised as suggested.

Fig5. Can the image quality be increased.
Figures in generally are of poor image quality. Please can the dpi be increased.

Y**es, all the figure quality has been improved by separating them from the main text (the resolution was degraded when the figures were inserted into the main text).**

L372. “2018”: we are now in 2019. Rephrase the sentence

Thanks, we have revised the description with updated reference.