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Geoscientific Model Development Discussions

Interactive comment on "Spatial and Temporal Evolution of a Lightning Diagnostic in HWRF (V3.7a)" *by* Keren Rosado et al.

Anonymous Referee #1

Received and published: 16 August 2019

Review of gmd-2019-139: Title: "Spatial and Temporal Evolution of a Lightning Diagnostic in HWRF (V3.7a)." by Rosado et al.

Summary: The authors utilize a diagnostic lightning forecast module to diagnose/estimate a measure of lightning activity within selected real tropical cyclone cases and an idealized case scenario.

Recommendation: Reject and, eventually, re-submit.

Main/General Comments: I. While the topic at hand is of interest to the community, I found the analysis generally very rudimentary with the authors going at length in describing figures limited to simple time series and horizontal/vertical cross sections. Because several tropical cyclone (TC) cases were simulated, the same basic anal-

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Discussion paper

ysis is repeated in a redundant manner, which “adds to the injury”. After reading a few pages, I honestly got bored. Given its repetitive nature, the entire results section could easily be condensed into 2-3 pages with the content distilled into concise arguments/hypotheses. With this subsequent gain in text length, the analysis could then be expanded by including more elaborated means to analyze in more depth the lightning and microphysics in the present hurricane simulations. Examples of such analyses are actually provided in the (very few) existing explicit modeling studies of (2D or three dimensional branched) lightning within TCs such as Fierro and Mansell (2017, 2018) none of which are actually either referenced nor discussed to put this study into a more appropriate context. In addition to the usage of a very basic, 100% diagnostic lightning scheme (i.e., no explicit storm electrification physics), the idealized TC (or “donut storm”) in Figs. 2 and 3 at 110h is completely unrealistic and, as such, cannot be used for verification. There are no rainbands and the eyewall width and eye diameters are unrealistically large. In the light of these unrealistic results alone, I am unfortunately inclined to recommend an editorial decision of rejection, for the time being. Additional major issues are listed below:

II. One salient concern of this study is the lack of rigor in analyzing TC lightning with respect to the inner core ($\sim$ r= 0-100 km) versus the outer rainbands (r= 100-500 km) in a methodical manner throughout the manuscript. This should be done systematically for the entire analysis to better compare the simulation results with the observations and, in turn, establish more meaningful relationships between intensity fluctuations and lightning activity produced by the current model in the context of TCs. When the authors state “there was more lightning at X hour”, I kept on wondering where and what was the inner core to outer band lightning ratio?

III. Emphasis should also be placed on defining proper lightning metrics (i.e., flash rates, flash density rates etc . . .) to establish more accurate comparisons with those from the WWLLN (pulse rates) (i.e., apples to apples comparison). Mentioning that WWLLN detects xx “lightning” is meaningless; same for LPI (in J/kg). I’d strongly advocate converting all to flash rate or a metric that is more palpable and, thus, comparable to obs [e.g., pulse rate, flash origin density rates . . . etc].

IV. Many observational and modeling works on storm scale electrification have shown that graupel and updraft volume were the bulk quantities exhibiting the best correlations with total lightning and, as such, should be shown in this analysis for both the inner core and outer region/rainband to provide a more adequate diagnosis of the relationship(s) between lightning activity and intensity fluctuations (see e.g., Fierro an Mansell 2018). The authors have the model output data to do so.

V. The lightning scheme used is very basic as it is 100% diagnostic and does not take into account any fundamentals of lightning physics; e.g., 3D electric field solve, computation of polarization and noninductive charging rates, charge advection/sedimentation, lightning discharge processes etc . . .all of which were shown – in the context of a bulk discharge scheme - to be computationally efficient in the WRF framework (Fierro et al. 2013) and, thus, could easily be implemented in the Thompson scheme herein as contrarily indicated in the conclusion section. The authors should discuss this in more detail and work towards this goal for a more physically sound approach to forecast lightning in TCs or any other convective modes.

VI. From experience, I would argue that the McCaul diagnostic lightning scheme offers a more physically sound approach (better alternative) to lightning diagnosis than the LPI code via its inclination to e.g., give explicit consideration to stratiform and convective lightning (using graupel and ice fluxes). Furthermore, McCaul's scheme (implemented in WRF-ARW) has been battle tested in real time over several (∼8) years by many operational centers over the US and abroad for convective phenomena ranging from airmass thunderstorms to MCSs (including TCs). Thus, I fundamentally and respectfully differ that this is the "first" study investigating lightning in an operational model in general (but in HWR alone yes).

VII. What is the rationale for focusing on cases that are ∼10 years old for which no total lightning data from the GLM are available? The 2017 year was very active with a near record number of major TCs (cat 3 or greater) many of which undergoing RI periods, ERCs, etc (Klotzbach, 2018). A shining example is Hurricane Maria, which total lightning activity with the GLM was studied in detail (Fierro et al. 2018) – including during its ERC - and contrasted to that of WWLLN's.

VIII. A. The distinction between intracloud and CG lightning is critical when studying lightning in any types of convective systems and should thus be carefully distinguished in the current study [in addition to outer region vs inner core in comment #II]: i.e., the model produces a surrogate for total lightning activity while WWLLN provides an estimate for total CG activity; especially over remote oceanic regions where DEs are low. Why would intracloud/total vs CG activity this be relevant for TCs ?: The aforementioned study on hurricane Maria, for example, underlined that intra-cloud to CG (or "Z") ratios could far exceed 10:1 in the inner core - which could change our perception on how lightning evolution relates to TC intensity. This is perhaps best exemplified by (and consistent with) the lightning jump algorithm for severe threat prediction (Schultz et al. 2011) almost entirely dependent on IC flash rates (See MacGorman and Nielsen 1991, MacGorman et al. 1989, Rutledge and Lang's seminal works etc) as CG flash rates alone only are indicative of the demise of an updraft (via reflectivity core descent). Boccippio et al. 2001 and Medici et al. 2017 found that in deep continental convection, IC flashes always outnumber CGs by a ratio sometime exceeding 10:1. Thus, it would make sense that when a VLF instrument such as the WWLLN detects a CG burst in the inner core, the updrafts are in their weakening stage as indicated in Fierro et al. (2011) for Hurricane Rita – and, thus the TC will undergo imminent weakening.

B. A more appropriate surrogate for evaluating the simulated total lightning activity from the model (either with LPI, McCaul or Fierro's explicit scheme) would be GLM lightning rates. The GLM instrument aboard GEOS-16/17 provides continuous day/night coverage of total lightning at $\sim$90% detection efficiency (DE) over a large domain covering the Americas (Gurka et al. 2006; Goodman et al. 2012, 2013, Rudlosky et al.

2018). Similar space-borne technology to detect lightning have been developed by China (Feng-Yun-4, yang et al. 2016). Apart from their propensity to detect total lightning at a high DE, the chief advantage of this technology lies in its ability to retrieve lightning over remote oceanic regions where all TCs form and, eventually, intensify.

C. A and B and comment #II above illustrate that particular care should be given to total lightning in the inner core versus total lightning in the outer region / CG lightning in the inner core and outer region / CG lightning over the entire storm (r=0-500km) / total lightning over the entire storm. A proper study on TC lightning should make such distinctions very clear and evaluate these with available modeling and observational studies on TC lightning.

D. In the context of C, the authors should also provide statistics on which lightning behavior(s) listed above is (are) more systematically seen in the model during TC development, RI/intensification/weakening and why [using physical explanations based on eg internal dynamics or environmental factors]?

IX. The title is very generic and does not properly reflect the work done. I'd suggest something along the lines of: "Diagnostic forecasts of lightning activity within idealized and selected real tropical cyclone cases: preliminary results"

X. The results from Price et al. 2009 have been recently criticized by:

Whittaker I.C., E. Douma, C.J. Rodger, T.J.C.H. Marshall: A quantitative examination of lightning as a predictor of peak winds in tropical cyclones. J. Geophys. Res. Atmos., 120 (2015), pp. 3789-3801, 10.1002/2014JD022868

Which should be included/discussed wherever appropriate.

XI. The results section is almost completely devoid or references to previous modeling and observational works (e.g., TC Earl for which the lightning activity was studied in detail). Regarding the modelling (page 5, top) + no references whatsoever are provided for the various modules/parameterizations and vortex bogusing code used.

Because these issues are collectively substantial and would require thorough rewriting of the manuscript in many places, I opted not to dwell on editorial comments for the time being.

Figures:

There is no need to repeat in the body text what belongs to the figure captions. Please revise accordingly [eg page 9 bottom]. Figure 6 is very difficult to interpret due to the cluttering of contours.

Minor/Editorial comments:

Intro: Include a discussion on the effects of shear on TC lightning (modeling and observations).

Page 6: What is an inactive sounding ? initially at rest ?

Section 2.4, third line: consider revising (grammar).

Page 9: Please show how the secondary wind max “cuts off” the heat supply in HWRF. Invoking a reference is not sufficient. Either show it in your model data or delete the statement.

Respectfully, End of Review-

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2019-139/gmd-2019-139-RC1-supplement.pdf

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