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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 #2

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The Lightning Potential Index (LPI) parameterization is implemented in the HWRF model to investigate if lightning could help in reducing bias in intensity forecast. The use of lightning as a proxy for tropical cyclone intensification is of interest to the community. However, in its present form, this manuscript is not suitable for publication.

General comments

1. My first concern is: why did the authors decide to use a proxy for the lightning activity and not a complete electrical scheme (Mansell et al., 2002; Barthe et al., 2012; Fierro et al., 2013)? I guess that it is because of the high numerical cost of explicit electrical schemes. However, it should be stated clearly.

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Discussion paper

After making clear why a proxy for lightning activity is used, the choice of this proxy should be justified. Apart from the LPI, several microphysical and/or dynamical parameters have been used as proxies of the total flash rate: cloud-top height, maximum updraft speed, updraft volume, precipitation ice mass, graupel mass or volume, or even more complex parameters. Most of these parameters were discussed recently in Basarab et al. (2015), Lopez (2016) and Bovalo et al. (2019). A discussion about the choice of this parameter among all parameters available in the literature should be added.

2. One important limitation of this study is the choice of the case studies. Four different simulations are presented, but the analysis of each case study is too superficial. In my opinion, the authors should have treated only one case study: tropical cyclone Earl or a more recent case study. Concerning hurricane Igor, Figure 12 clearly shows that HWRF underestimates the analyzed intensity ($\sim$ 24 m s^{-1} at 70 h) and does not capture the rapid intensification phase. Then, comparing the LPI with the intensity of the best-track does not make any sense. Focusing on one particular hurricane would allow to evaluate correctly the simulation and to conduct more useful diagnostics to support the conclusions.

Due to the relatively poor detection efficiency of the total flash rate with WWLLN and the availability of total lightning data from GLM, a recent hurricane of the Atlantic basin could have been chosen as a case study.

3. The presentation of the results is not well structured. For example, the model validation (track and intensity only) comes after the search for correlation between the lightning activity and the hurricane intensification (for Earl and Igor).

4. The results are not convincing.

- The authors claims that a "lightning maximum occurs prior to the intensity peak", but, as shown in Figures 1, 12 and 14, there are several maxima and minima of lightning activity before the maximum intensity.
- For all cases, it is not possible to check whether the cyclone has reach its maximum intensity at the end of the forecast time.
- The evolution of the idealized hurricane structure shown in Figure 3 is very weird. After 110 hours of simulation, there is no more rainband in the system which can explain why there is no LPI in the rainband (p 8, l 29-30)!
- An eyewall replacement cycle (ERC) is obtained when changing the planetary boundary layer scheme. However, the impact of the ERC on the intensity is not visible, and it is difficult to see an ERC on Figure 6.

5. The references are not always appropriate: see the specific comments. The introduction needs to be deeply modified.

For all those reasons, I think this manuscript is not suitable for publication. However, the topic is of great interest for the community, and the authors could resubmit this paper after a substantial work.

Specific comments

p 3, l 11-12: The formation of lightning happens in convective cells in general and not only in tropical cyclones.

p 3, l 13: The references (MacGorman and Rust, 1998; Rakov and Uman, 2003) are not about convection in tropical cyclones.

p 3, l 16-18: more recent references should be added like Jiang et al. (2013), Bovalo et al. (2014) and Zhang et al. (2015).

p 3, l 28-29: the original reference for the definition of rapid intensification is Kaplan et al. (2010).

p 3, l 26-34: more physical explanations about the two different theories are expected. For example, how an increase in lightning activity in the outer rainband can be linked to a rapid intensification phase?

p 4, l 16-18: "... the lightning potential is a diagnostic tool, it cannot change the model forecast of a tropical cyclone". Of course! This must be checked out during the developing phase of the model, but it is not necessary to discuss about this in the manuscript.

p 4, l 25: the HWRF model is an atmosphere-ocean coupled model, but there is nothing about the numerical set up of the ocean model in the manuscript.

p 4, l 28: The references (Tallapragada et al., 2014, 2015) are about WRF and not HWRF.

p 5, l 1-3: what is the difference between 18-km, 6-km and 2-km resolution, and grid spacing of 0.135°, 0.045° and 0.015°, respectively? Grid spacing and resolution should not be used interchangeably: https://doi.org/10.1175/1520-0477(2000)081%3C0579:CAA%3E2.3.CO;2

p 5, l 19-20: More suitable references for charge separation could be Saunders (2008) and references included.

p 6, l 3: The LPI parameterization was introduced in the Thompson microphysics scheme while the Ferrier-Aligo microphysics scheme was specifically designed for tropical cyclones (see p 5, l 6-7). This choice should be justified.

p 6, l 30-31: The term intra-cloud flashes or (cloud flashes) is generally used to refer to flashes that do not reach the ground (see MacGorman and Rust (1998)).

p 9, l 31-37 and p 10, l 1-4: there is no link with the discussion.

Since the results are not convincing, I do not go beyond the third section in my specific comments.

Technical corrections

The manuscript needs a careful proofreading.

The authors should rework on most of the figures:

- Figures 1 and 4: remove "solid" and "dash" in the titles of the Y-axis
- Figures 1, 2 and 4: make sur the aspect ratio of these figures is 1
- Figure 2: the unit of LPI is missing
- Figure 4: it is hard to distinguish between the solid and dash lines. A grey line should be used like in Figure 1.

- Figure 5: it's difficult to read the a-h numbering of the panels
- Figure 6: there are too many isocontours for radar reflectivity. Only the most important contours (to visualize the ERC) should be kept on this figure. There are two overlaid "130h", "136h", "142h", "148h" and "154h" on some panels.
- Figure 9: check the location of the titles on the panels
- Figures 11 and 14: remove "blue dashed line" in the title of the Y-axis. Add the unit of the flash rate from WWLLN in the caption.
- Figure 13 is not discussed in the manuscript. There are not dots every 6 hours as mentionned in the caption.

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