

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC2
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Comment on nhess-2022-39

Eric Bruning (Referee)

Referee comment on "A satellite lightning observation operator for storm-scale numerical weather prediction" by Pauline Combarrous et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2022-39-RC2>, 2022

The authors describe a method for building a lightning observation operator for eventual use in assimilation of MTG LI flash extent accumulation data in the AROME NWP model. They use synthetic MTG LI data built on prior work. They reach a clear recommendation that is ready for use in preparing an operational modeling system for future observations. The practical aspects of the authors' study are relevant to global operational weather centers. The techniques used are comparable to current state of the art efforts.

Furthermore, the obvious difference in the spatial coverage of the so-called microphysical and dynamical proxies is a valuable scientific result, and one I was not expecting to see emerge from a model-observation comparison where the model does not include electrification process. I would interpret this result as telling us that the integrated action of all updrafts in forming ice-phase precipitation is more important than any one updraft, consistent with the patchy appearance of the dynamical proxies. And of course, ice-phase precipitation is widely accepted as the primary ingredient in thunderstorm electrification, so it makes physical sense from that point of view, too.

I commend the authors on a comprehensive study that is also very concisely and clearly written – especially for the large number of parameters compared and the number of methodologies employed. Below, I offer some minor comments that could help clarify a few details.

Line 31: The lightning jump is coincident or slightly lags some aspects of intensification (updraft volume, number concentration of precipitation in the mixed phased), but leads other intense phenomena at Earth's surface. Which kind of intensification is meant?

Line 44: Please add a reference for MTG LI.

Footnote 1 (p. 2): An expanded discussion of the authors' reflection on this topic would be valuable. Is the reason for the change sufficient to risk the confusion that could result from two, actively-used names for the same product?

Line 104: are the vertical winds not among the prognostic variables?

Line 150: Is the limitation in maximum vertical velocity due to model resolution/numerics? Model integration typically starts to act as a low-pass filter at about 6 times the grid spacing. If it is not due to this, what is the cause?

Line 214: "others" should be "other"

Line 235: here, proxy refers to the NWP parameterizations, correct? The FEA grid is also a proxy for real MTG LI measurements, so it might help to state which proxy is meant.

Line 240: When discarding values equal to zero, is any rounding or other rule applied to determine what counts as zero?

Somewhere between lines 240 and 245, I don't quite follow how the stacked, ranked ordered data (the histograms in fig. 4?) are used to perform the regressions. On line 250, is the value of "proxy" a normalized count at some flash rate, and not the flash rate itself? I'm probably missing something obvious, but could the authors illustrate / visualize the regression for the simple, linear case? What goes on the x axis and y axis of the regression? How many samples are there?

Line 261: I thought the authors removed spatial considerations from their method (line 238), so how is FSS calculated?

Line 400: the combination does seem to do somewhat better over Corsica, a region highlighted by the authors as performing poorly on line 313.

Fig. 14: It's somewhat interesting that a multivariate proxy makes the 1 fl/min performance worse, but the 10 fl/min performance is slightly improved. I agree with the authors that it's hard to see a benefit over simply using graupel mass.

Line 434: the decision to not accumulate graupel mass may seem strange to some

readers, but I think it makes sense if the goal is data assimilation. In DA systems, observations are often assimilated against the model state at a single time step, and so the goal here is to find a representative accumulation window for use in DA. Is this in fact the authors' motivation for the design of the experiment in this section?

Lines 461-3: This sentence is missing the results of the bootstrap test.

Line 465-6: High flash rates are not frequent in these data, so the authors' conclusion here is, in general, justified if the goal is data assimilation for regional convective structure. However, there are some modeling systems (e.g, the US NSSL's Warn-on-Forecast system) that have the aim of correctly assimilating the state of individual storm cells, where high flash rate fluctuations that capture storm state on short time scales are of more importance. Fig. 19a diverges at high flash rates, indicating that the authors' conclusion might not apply if the goal is high-flash-rate single cells. In that case, would the recommendation be to use a shorter accumulation interval, consistent with the observation that the curves converge at shorter accumulation intervals?

Line 482: I was briefly confused that the authors were recommending a multi-parameter estimation method and were not using the single-variable graupel mass operator that their analysis to this point seemed to prefer. Please clarify that *either* IWP or F2 is preferred, because that single proxy variable is calculated from more than one underlying model state variable.