

Review of nhess-2018-319 – version 2:

Title: “Preliminary results of the impact of lightning and radar reflectivity factor data assimilation on the very short term rainfall forecasts of RAMS@ISAC: application to two case studies in Italy.” by Federico et al.

Recommendation: Major revisions.

Main Comments:

(1) While the authors have gone at length to address some of my earlier concerns, I still found most of the analysis presented (especially on pages 19-22) relatively rudimentary. The text is essentially reduced to unnecessarily detailed and repetitive descriptions of rainfall plots. More targeted, concise descriptions clearly highlighting the pros and cons of each DA methods should be elaborated instead. As indicated in my original review, the authors should - in that regard - show Roebber performance diagrams in their analysis to provide clearer, concise estimates of the performance of their forecasts. Additionally and as also indicated in my original review, soundings complemented with horizontal cross-sections of RH/Qv should be provided to highlight, quantitatively, how the RH/Qv field are adjusted by each respective DA approach at the analysis time (radar vs lightning). Such analysis is highly desired and, arguably, critical for readers to gain a better appreciation of the first order impact of the DA, especially given that both observations used essentially adjust the same field (cf main comment 2 below).

(2) Most importantly, as indicated in my original review, the current data assimilation set up suffers from one major drawback in that both the VAR assimilation of radar reflectivity factor and the nudging of lightning data essentially adjust the same variable ($Q_v \sim RH$ via Q_v/Q_{vsat}) resulting in redundant overlap. A proper combined DA set up should ensure that adjustments from different observation sources are applied to different prognostic variables: E.g., lightning adjusts the Q_v field while the reflectivity factor acts on specific hydrometeor mixing ratios (even through simple linear functional relationships). With this overlap, it is also unclear how the forecast results are impacted depending on whether the nudging of Q_v is performed before or after the 3DVAR of reflectivity factor data. Until this critical point is not properly addressed, I will be inclined to maintain the current editorial decision of “Major Revisions”.

(3) To complement (2), ideally, the Doppler (radial) winds should adjust the three Cartesian components of the wind field. Stating that such data “aren’t yet ready” is, in my view, a succinct pretext to evade the (necessary) work.

(4) To provide a more equitable estimate of the performance of the DA method, the authors should also select at least one high impact weather event wherein the CTRL forecast performed reasonably well (e.g., a strongly forced case along a cold front). This point also was indicated in my original review but hasn’t been satisfactorily addressed by the authors.

(5) For radar reflectivity factor, the DA should make use of 50-km disks instead of squares. This is easy to code in Fortran by using e.g., a 2-D mask array to find the grid points fitting within the disks.

(6) Last, I still found the level of English relatively poor and generally not suited/inappropriate for the level of a peer-reviewed journal. Because these issues are collectively substantial, I opted, for now, not to dwell on such editorial comments (including grammar).

Additional comments:

- (1) Line 42: Radar data are far from being “unconventional”.
- (2) Line 44: Model “deficiencies” are by no means limited to oceanic regions.
- (3) Line 47: “high spatio-temporal resolution”
- (4) Line 75: “Lightning is another source ...”. Line 95: “convection-resolving”
- (5) Line 99: Not “extended” but “adopted”.
- (6) Line 17-18, 39-40, 47,134, 249, 258-260, 266-267, 389, 711-712, 761 (among many other instances): Consider revising (grammar).
- (7) Line 57-60: delete or include well known (seminal) studies such as those from Tong and Xue, Gao’s, Aksoy’s, Zhang’s to name a few.
- (8) Line 340: “subjectively as a compromise between increasing ...”
- (9) Line 389: downscaled ? Please elaborate/explain.
- (10) Line 414: This is reminiscent of the Gaspari and Cohn function for EnKF localization.
- (11) Line 480: “With these settings, larger weights are given to”. Line 489: “In contrast,”. Line 497 “highlights the difficulty”. Line 507: “reduce the relative”. Line 703: “room for improvement”
- (12) Lines 527-530: redundant; delete.