Nat. Hazards Earth Syst. Sci. Discuss., 2, C1044–C1047, 2014
www.nat-hazards-earth-syst-sci-discuss.net/2/C1044/2014/

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Simulating lightning into the RAMS model: implementation and preliminary results" *by* S. Federico et al.

Anonymous Referee #3

Received and published: 9 June 2014

This paper presents results of implementation of a previously published lightning scheme into the RAMS model, and its application to several case studies of thunderstorms in central and northern Italy. It is certainly within the topics which NHESS deals with, and is well within the international standards that the journal aims for.

The paper is well-written, logically organized, thorough and lucid. The graphs are clear and properly show the main results, and the tables nicely summarize all the storms that were studied and the skills of the model. The authors discuss the results of 2 case studies in depth, and evaluate the performance of the model and the lightning prediction scheme, and offer possible reasons for the inconsistent accuracy for different cases. They also set a clear course for future development and it is likely that the insights gained in the initial study will be used to improve the performance of the RAMS

and make it an operational tool for lightning forecasts, and to gauge the nature of convection based on offsets in the model-predicted and observed lightning activity.

I have some comments and questions which the authors need to consider. Apart from some minor revisions, I recommend that this paper is accepted for publication in the NHESS.

Major comments

1. The Dahl et al. (2010) scheme over-simplifies the complex charge structure of thunderstorms and portrays it as two plates at different altitudes and temperature regimes within the cloud, each composed of a different class of particles and assigned with a different polarity. This should be clearly mentioned in section 2.2, and compared with "real-life" charge structures, such as those reported by Marshall et al. (JGR, 1995) and Stolzenburg et al. (1994). This can be one of the sources of the discrepancies found between the computed lightning activity and the observed one. Also, assigning a "fixed" polarity to each species is not accurate, as many laboratory experiments show (reviewed in Saunders, 2008), and certainly using a constant lightning efficiency of 0.9 all throughout the storm life-cycle is inaccurate. In the discussion section the authors need to evaluate their results in light of these shortcomings of the scheme (perhaps in p. 22, after lines 11-22).

2. Additional factors that may play into the performance of the lightning scheme is the time interval between two calls of the scheme – 10 minutes (p.10, lines 7-9) seem to be a very long time. Since the authors claim that the scheme is computationally efficient (p.22, lines 31-33), one wonders what shortening this interval will do in terms of efficiency and would it be critical. Have the authors conducted a sensitivity study for the performance of the scheme with varying times? If not, it is highly desirable to run al least one of the case studies (preferably the October 15th 2012 case, which had rather poor agreement in timing and total lightning densities).

3. The difference in model performance for the two case studies is puzzling and the

authors rightly discuss it. However they seem to place the reason with the failure of the RAMS to correctly portray the convection, and hence the development of the thunderstorm cells and the ensuing lightning activity. This specific issue should be further discussed and there needs to be a suggested explanation: is this because the initialization was inaccurate? What in the synoptic condition caused such a difference? Why is the onset of convection on the October 15th 2012 case "missed by the model"? (p.16, line 15).

4. In their explanation of the persistence of lightning long after the actual end of convection (Figure 10, and p. 17, lines 3-9) the authors suggest that there is a problem with the sedimentation scheme of the RAMS for the cell residing over the sea. This explanation needs further justification, because it is not entirely clear how the surface properties affect the microphysics aloft so strongly.

5. The binning of lightning data from the LINET network assumes that all strokes occurring within 1 second and 10 km radius from an initial strike point are defined as a single flash (p. 11, lines 25-27). These are the conservative values used by the NLDN. Have the authors considered other thresholds for grouping strokes into flashes? (For example, in a recent paper Yair et al. (NHESS, 2013) showed that using stricter ranges (5 km, 0.5 s) is sufficient to discriminate between successive flashes in most cases). Could it perhaps improve the skill if the actual numbers reported by the number were different?

Minor comments

1. P. 4, line 18: suggest including Barth et al., 2010 2. P.4, line 20 and throughout: intra cloud -> intracloud 3. P.10, lines 23-27: please explain how the distribution of the lightning is made to follow the shape of the convective cell. How do you determine the shape? Is it contours of certain values of graupel concentrations, radar reflectivity or. . .? 4. P. 19, line 15: studies that occurred in fall 2012. . . 5. P. 21, line 22: six case studies that occurred over the. . . 6. P. 21, line 23: The fist one occurred. . ..

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 3351, 2014.