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Natural Hazards and Earth System Sciences
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Discussions

Interactive comment on "Synoptic-scale conditions and convection-permitting hindcast experiments of a cold-season derecho on 3 January 2014 in Western Europe" *by* Luca Mathias et al.

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In the manuscript Mathias et al. present a case study of a linearly-organized MCS and its representation in convection-resolving and convection-parameterizing simulations. Apparently, the case was unusual because a derecho developed in a postfrontal air-mass of an extratropical cyclone. They start by describing the case in detail based on ERA5 reanalysis and subsequently perform a set of simulations to assess model and configuration sensitivities. The three main findings are as following: (1) Explicitly resolving convection leads to added value when forecasting this derecho. (2) Further

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added value has been found when refining grid spacing from 2.8 km to 1.1 km. (3) In limited-area models, lateral boundary conditions need to be updated hourly to represent the case correctly.

The manuscript is novel, interesting and fits well into the scope of NHESS, but could benefit from addressing style, syntax, and conciseness.

Major Comments

1. I think the claim that this case was “not well anticipated” is exaggerated. While similar statements are repeated throughout the text, they are not well justified since storm-force wind was actually mentioned in the weather reports (P2L36 – P2L43). Furthermore, since some features are represented in ERA5, the identified deficiencies in the NWP simulations (Figure 8) point towards an issue in the observational analysis at the time (as mentioned in the summary).

I suggest to remove this entire discussion and shift the motivation towards added value of explicit convection for these events. It is interesting enough, as so far most of the discussion about resolving convection explicitly has been about diurnal convection (see Prein et al., 2015) or the tropics. Further motivation, specific to the NHESS audience, could be based on the discussions around global early warning systems (see Copernicus systems) and the question whether convection-resolving resolution is needed, or if resources should be invested in more ensemble members.

2. I would choose to configure the simulation configurations as identically as possible. Apart from the product to derive the initial and lateral boundary conditions, you have chosen to vary the number of intermediate nests, the init time, the microphysics scheme and the number of vertical layers. That makes it hard to pinpoint the observed differences to specific changes in configurations. For instance, at P8L256 you can’t distinguish between differences in vertical and horizontal resolution. I am not sure if the vertical resolution is the key issue.

P4L120- P5L135: I am a bit confused by the rather complicated setup chosen (maybe add a Gantt-chart-type figure outlining the init and update time?).

P4L126: Delete: "The aforementioned temporal setup is used to permit a most realistic simulation of the derecho and allow a direct comparison of the simulated data with the observational data."

3. I find the mix of panels showing different fields from different simulation resolutions in the same figure a bit confusing. I would switch between two modes. (i) When comparing model resolution I would always show the same fields for 7km, 2.8 km, and 1.1 km. (ii) When comparing between driving datasets, I would show the same panels for 7 km and 2.8 km.

Also, I would discuss the validation in Fig. 12 before the sensitivity studies.

3. P7L224-P8L228: “would have been the key factors to successfully forecast this cold-season storm.” Either I am confused, or you jump to conclusions too fast. You mix the influence of LBC update frequency and forecast skill in ECAN, which is a global simulation. I guess you arrived at your conclusion because Figure 9e looks a bit like Figure 10b, right?

Would it be possible to show time series of the driving fields (ERAI, ERA5, ECAN)? Maybe in the supplement?

Minor Comments

1. P1L26: There are earlier references introducing the concept of extra-tropical cyclones than Ludwig et al. (2015) and Gatzen (2018).

2. Section 3: Provide a concise summary of the criteria needed for an event to classify as a derecho and how they apply here. Now, this discussion is scattered throughout the manuscript.

3. P7L194-P7L200: Maybe outline if the environment was at least predicted correctly?

Explain why the underestimation of wind speed cannot be attributed to the wind gust parametrization.

4. Figure 5: Why do you show fields from two different resolutions: (left) 7km and (right) 2.8 km (see above)?

5. Figure 12: Add 2.8 km reflectivity?

Technical Changes

1. P3L70 – P3L86 These paragraphs need work. Your co-authors should be able to tell you how to make it more readable.

2. P2L44 – P3L66: Reading a list of papers (a reader probably doesn't know) without much context does not motivate to continue reading. Put the literature in context, explain where the gap in research is and why you think it is interesting.

3. P5L37 - P5L153: Although I enjoyed reading the detailed description of the evolution of cyclone Anne, there might be potential to shorten the text here.

4. P5L154-P6L187 While it is certainly a good idea to spend a bit of time explaining convection precursors to NHESS readers, there might be some potential to shorten this part too.

5. P7L217 – P7220 From a technical perspective this is an interesting result you may want to highlight more. There is an ongoing discussion about resolution jump vs. LBC update frequency for limited area modeling. Also, specify the employed upper boundary condition (Relaxation or w=0?) in Section 2.

Figures 2 -7: In the beginning, 5 figures are shown to set the stage for the main ideas following. Are all of them needed? Along with the shortening potential in (3) and (4). There might be potential to remove some of the panels or move them to the supplement.

P1L11: trough favored → trough, and was favored

P1L12: conditions were → environment was

P1L14: You need to mention that these are limited-area simulations

P1L13: latent instability. Maybe use the more common term conditional instability (also rest of text)?

P1L15: datasets → reanalysis datasets

P1L15: I would write “initial and lateral boundary conditions derived from ERA5”

P1L17: (i) convection-resolving scale → convection-resolving resolution (ii) At P1L14 you use the term convection-permitting. I would try and use just one of the two. We usually use convection-resolving, since the

P1L18: This case study is testimony to the usefulness of ensembles of convection-resolving simulations to . . .

P1L21: affect ← wrong word

P1L22: The style of the manuscript is unnecessarily cautious (hedging), which is legitimate to protect your claims, however, in most cases, it is actually not needed. For example: “In some cases, MCSs can exhibit”. There is no need to add another can here. Check in the entire manuscript if vague language is really needed.

P1L26: (i) remove: "which is" (ii) linearly organized → linearly-organized (also address hyphenation mistakes in rest of text)

P1L28: remove: "region"

P2L45: It is pointed out ← rewrite

P2L50: AGL ← define

P3L83: grid interval → grid spacing

P4L95: realised → conducted

P4L95: taken → derived

P4L110: Citing Baldauf et al (2011) may be warranted

P5L138: intensive → deep

P5L149: analysed → diagnosed

P7L206: “displaced” Wrong word, since in this Section, we don’t know (yet) the true location.

P8L231: the highest-resolution run → the simulation with 1 km grid spacing

P8L235: “convection-initiating boundaries”. Maybe choose a different term as it can be confused with the lateral boundaries, which you also discuss. Maybe “lifting mechanism”?

P10L308-L310: Maybe mention that at least DWD and MCH employ such systems these days.

Table 1: Specifications about the physical parameterisations used in the different CCLM domains. → Simulation configurations

Figure 2 caption: I usually use the term “computational domain” (also in rest of text)

Figure 4: diagnosed 700 hPa upward motion.

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