Replies to Referee Comment 3 on "Evolution of squall line variability and error growth in an ensemble of LES" [Groot and Tost(2022)]

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As authors we would like to thank the reviewer for the thoughtful reading of (and extensive comments on) the manuscript. The comments are in our eyes is useful to further clarify the content of the study.

1 High level comments [RC3(2022)]

- *"The key messages of the study and the motivation for it aren't made sufficiently clear at the onset. Although there is a discussion of the mechanisms that will be studied, it is not clear at the onset what motivated the study (what questions were left open by previous studies). A clearer focus and more guidance would also help to streamline the results section."*

  We thank the reviewer for this comment. The authors hope that by addressing the aim of the paper and the gap in literature explicitly in a few sentences at the start of the introduction helps guiding the readers better.

- *"Several sections are difficult to read/interpret. Some sections could be reduced as they contain too much detail. A previous reviewer has already given some very helpful feedback here, which could be exploited further.*
  - Section 2.4 is hard to interpret without more context.
  - Section 3 as a whole contains many numbers and details, which make it hard to read. The key messages are given in section 4. It would be better to only retain what is needed to support section 4. It is probably best to integrate these sections, so that results and their interpretation are presented together.
  - The supplementary material could likewise be reduced further (at this point, I have focused on the main text)."

  In the revised manuscript, further context has been added to Section 2.4. It was not clear that statistical assessment was needed to test the obtained ensemble sensitivity for robustness, and now this has been clarified.
  The urgency of some results in Section 3 has been reconsidered and some have been removed from the main text. The reviewer wondered if Figure 5 was really needed, amongst others, and the authors believe that it is. It allows us to point out differences in cold pool propagation: Figure 5 in Section 3.2.2 provides a good bridge towards Sections 3.2.3 and 3.3.1. However, pointing out the upper tropospheric patterns here is probably indeed unnecessary (as widespread flow patterns are evaluated in 3.3.2 and all direct dynamical diagnostics of variability are wrapped up in Section 3.3.3). To canalise the results, discussion around Figure 5 specifically has been partly removed from the main material.
  Merging Sections 3 and 4 has been tried earlier (before the initial submission, in the early manuscript writing). However, for the same reason of canalising the focus of readers, we think that this solution would not be of benefit for the majority of readers. In a study with supporting results based on various analyses that lead to two main messages about characteristics of the error growth of the squall lines, a synthesis is in our eyes a better choice to bring the results together. The synthesis section has been appreciated in the earlier round of review RC1,RC2.

- *"The figures need some improvement (both in terms of presentation and clarity).*
  It would be clearer to plot the tracer in each simulation (and only keep the difference plot if the differences are not clear from a direct comparison) in figures 4 and 5. In figure 5, I wonder if both tracers

are needed to tell the story.
For figure 6, why not simply show the trajectory over time for each of the simulations and use that as a starting point for discussion? The lag-correlation is harder to interpret.
For figure 8 and 9, using equations rather than words in the text would make it clearer what precisely is plotted. In terms of presentation style, some axis labels are missing, and some text overlaps. Time units switch between minutes and hours."
Presentation of Figures has been improved in the updated manuscript. The Figure 4 now shows both tracer difference and actual tracer concentrations themselves for $t = 30$ minutes. This is indeed of benefit to the readers for a quicker interpretation of the Figure. However, once readers have seen the ordinary tracer concentration plot (provided now with the update), we believe that the readers can efficiently interpret the difference Figures in the following panels of Figure 4.
At 30 and 35 minutes of simulation time, both the difference and the actual tracer concentration plot give a satisfying picture according to the belief of the authors (which is because the tracer difference is caused mostly by displacements, where-ever a difference occurs). However, after 25 minutes, one is searching for patterns even more subtle (generally not shifts in location, but differences in local dilution of tracer concentrations). This means that the difference concentration is much more useful than the concentration of tracers, and the difference concentration is the best choice to visualise, in our eyes. Furthermore, guiding the reader by providing an X in the figure assists the reader on the way to recognise the most important patterns. If adding the X in the display of original concentrations, the authors believe that the Figure becomes less readable.
The necessity of Figure 5 has already been addressed in the reply to the previous main comment.
Regarding Figure 6, the authors believe that the added value of the lag correlation lies in its quantitative information on the cold pool edge location, which is essential as part of the overall quantitative analysis. However, we agree that an overview of both qualitative (actual cold pool edge locations) and quantitative (lag correlation of its location) representations of cold pool locations adds value to the overall story. Therefore, Figure 6 has been updated to include visualisations of both types of information. Furthermore, the previous version of Figure 6 (now: Figure 6, panel b) has been updated with an additional curve to supplement the information on the cold pool edge location.
The authors think that using equations to explain the computations done to obtain Figure 8 does not simplify interpretation of the Figure: it is more likely that this leads to a couple of additional sentences needed to explain symbols used in such an equation. These sentences would be easier to interpret than the currently included sentences - they would not differ substantially from the yet existing text. Furthermore, the explanation of the ensemble sensitivity analysis (the information at the start of Section 3.3.2) is supposed to explain the exact procedure of the calculation sufficiently well. The computational result of the procedure consists of local correlations and co-variance in u_{avg} and w_{loc}, of which the former two are presented in Figure. We have added "co-" before variance, to emphasize that Figure 8 shows the co-variance within the ensemble of u_{avg} associated with w_{loc} at location of the cross in Figure 4b (updated manuscript) resulting from the ensemble sensitivity analysis. Computations needed for Figure 9 regard the definitions of updrafts and downdrafts earlier in the same Section. Based on the detailed comments of the reviewer (e.g. " *"(positive for updraft detection)" → remove"* [RC3(2022)]), the authors suppose that this definition is clear. Similarly, the authors assume that the procedure to extract analytical quantities displayed in Figure 9 is clarified in the accompanying text of Section 3.3.3. Lastly, flow has been averaged along the y-direction for Figure 9, but this averaging is not specific for Figure 9 and are supposed to be clear from other Sections of the manuscript (e.g. Sections 2.3, 3.3.2). The problems with the lay-out of Figures 8 and 9 (e.g. "axis labels") have not been identified by the authors: axis labels ("x (km)"; "z (km)"; "u (m/s)") are present and neither have time indications in hours been found in Figures 8 and 9.

- *" Details about the differences between the simulations that form the ensemble are unclear.*
  As I understand it, the simulations differ by the height of the zonal shear layer, but it is not clear which member corresponds to which interface height. Though the authors argue the interface height does not monotonically relate to e.g. "w_{loc}", it would still be good to order the simulations by it." The reviewer is right about the differences in initial conditions between the ensemble members. As the de-correlation from 0 to 30 minutes of simulation time is one of the two key points of the work, conveying this message has priority. Using coloring schemes in two Figures (6a and 10) where all ensemble members are addressed individually in the updated manuscript and by adding a colouring as an indication of the similarity between various members' initial conditions (thereby referring to z_i), further clarification is hopefully achieved. Furthermore, by adding the correlation value between initial conditions and w_{loc}

to the only table in the manuscript, the authors hope that the issue is completely clarified (after having been addressed by a previous reviewer ([RC1(2022)] and [RC2(2022)]), and subsequent first step in the clarification).

- *"In section 3.3.2, the precursors may not always be driving the target. For example, a higher precipitation flux may cause higher evaporation and then faster cold pool propagation. That said, faster propagation could indeed also lead to more intense convection. In this context, it may be worth looking at a paper by Alfaro (2017) in JAS "Low-Tropospheric Shear in the Structure of Squall Lines: Impacts on Latent Heating under Layer-Lifting Ascent"."*
  We thank the reviewer for pointing out this interesting paper ([Alfaro(2017)]). The occurrence of the de-correlation phase of initial perturbations within the first 30 minutes of simulations implies that the initial shear layer top does not relate to eventual perturbations in latent heating and cold pool propagation directly, after the squall lines have formed. However, without the initial de-correlation phase as a result of gravity waves that is identified in our work, the work pointed out by the reviewer suggests that the low-level shear differences could directly affect the expected ensemble variability in terms of latent heating for example (by a small amount, almost 5%, interpreting [Alfaro(2017)]). This potential implication is now mentioned in the revised manuscript.
  Nevertheless, as none of our diagnostics assesses the temporal evolution of the depth or strength of the shear layer and its variability later on in the simulations, we did not assess the specific relationship of the contemporary low level shear with cold pool propagation and latent heating rates.

2 Detailed comments[RC3(2022)]

- *"Check the text for compound (multi-word) adjectives, and hyphenate these: e.g. "three dimensional" → "three-dimensional"; "high resolution simulations" → "high-resolution simulations"."*
  Multi-word adjectives have been hyphenated in the updated manuscript.
- *"Remove/replace words that can be left out with no loss of information, e.g: "Presented diagnostics" → "diagnostics" ; "used scheme" → "microphysics scheme"; "The applied initial conditions" → "The initial conditions"."*
  The manuscript has been re-read in detail by the authors and has been checked for such issues. In general, in combination with the streamlining (as mentioned under the second bullet point of Section 1 of this reply), the authors hope that solving this issue has further clarified the manuscript and made it easier to digest for most readers.
- *"The subject "One" is overused in the text. I realise some authors try to avoid "we", but the use of the first person makes it clearer whether the authors agree with a line of thought or not."*
  This is correct for the previous version. During the re-read through the manuscript, we have addressed this issue and hopefully resolved it by replacing occurrences of "one" with first person and passive voice counterparts.
- *"Where two references are given outside parentheses, replace ";" by "and" (e.g. line 605)."*
  The authors believe that this issue has been resolved, by (where-ever appropriate) replacing those instances with a comma or "and".
- *"In several places in the introduction, the text is vague/general/unclear, for example:*
  - Line 17: "Given the increasing computational resources"
  - Line 21: "It also includes the aspect of representation"
  - Line 25: "How squall lines depend on microphysics, shear and instability has been investigated rigorously by now, (e.g. Morrison et al., 2009; Grant et al., 2018; Adams-Selin, 2020a, b)." (also note the comma here)
  - Line 35: "This was the core feature of both sensitivity studies."
  - Line 69: "A sensitivity of these discrete convective cell was identified, which lead to a dependence of initiation on the active treatment of radiation."
  As for the other issues, these occurrences of "vague text" (Reviewer 3, [RC3(2022)]) have been clarified according to the author's beliefs in the revised version: by a replacement with statements and descriptions that are more specific.

Line-by-line comments have all been addressed by replacing the words and sentences pointed out by the reviewer with corresponding suggestions provided by the reviewer, or by another similar revision of the textual details. In a few cases additional details have been provided in the manuscript, as requested by the reviewer. The authors think that the way most of these comments have been addressed and resolved is clear from the adjustments in the manuscript.

A few of the detailed comments need to be addressed specifically:

- *"Line 39: Mentioning the work of Lorenz (1969) here already would be beneficial."*
  The authors hope that this issue is addressed in accordance with the expectations of the reviewer. Although it was not 100% clear if the reviewer meant to include the point of shorter and shorter predictability time scales associated with smaller and smaller spatial scales identified in [Lorenz(1969)], we have assumed so and added this.

- *"Line 166: "Furthermore, the ensemble members all have slightly different boundary conditions, as controlled by their own evolution nearby/at the boundaries. The boundary conditions are solely based on their conditions, with the first derivatives set to zero right at the boundary." → This is unclear, possibly what is meant is that the values at the boundary are different, even though the same type of boundary conditions is applied."*
  This interpretation by the reviewer is correct and adjustments have been made accordingly.

- *" L 385: "would definitely pass the statistical significance test" → why not simply check it passes."*
  The authors have developed the statistical test to assess significance of the patterns within the squall line for the ensemble sensitivity analysis. Afterwards, the authors noticed that the same statistical test would have been passed ahead of the squall line by gravity wave signals, if the authors would have targeted at this area on beforehand. However, the authors believe that they should not test a hypothesis a posteriori, as statistical tests are generally specifically designed to have a definition on beforehand, suited to the appropriate requirements. Subsequently the test is executed.
  Unfortunately, the test was undertaken for only one of the two patterns identified with the ensemble sensitivity analysis and not for both. Therefore, we state the findings in the specific way we do, implying that if we had designed the test the same way for both gravity waves ahead of the squall lines and the flow within the squall lines on beforehand, both would pass the statistical test.

- *"- Line 24: "true convergence" → note that there may be convergence of bulk properties (see e.g. work by Wolfgang Langhans and others), even if there is no numerical convergence."* We agree that difference between local numerical convergence and statistical/bulk convergence is relevant for the paragraph about convergence and increasingly high-resolution simulations of squall lines: hence, we cite the paper [Langhans et al.(2012)Langhans, Schmidli, and Schär].

References

[RC1(2022)] Referee comment 1 on "evolution of squall line variability and error growth in an ensemble of les", 2022.

[RC2(2022)] Referee comment 2 on "evolution of squall line variability and error growth in an ensemble of les", 2022.

[RC3(2022)] Anonymous referee 3 comment on "evolution of squall line variability and error growth in an ensemble of les", 2022.

[Alfaro(2017)] Diego A. Alfaro. Low-tropospheric shear in the structure of squall lines: Impacts on latent heating under layer-lifting ascent. *Journal of the Atmospheric Sciences*, 74(1):229 – 248, 2017. doi: 10.1175/JAS-D-16-0168.1. URL `https://journals.ametsoc.org/view/journals/atsc/74/1/jas-d-16-0168.1.xml`.

[Groot and Tost(2022)] Edward Groot and Holger Tost. Evolution of squall line variability and error growth in an ensemble of les. *EGUsphere*, 2022:1–34, 2022. doi: 10.5194/egusphere-2022-515. URL `https://egusphere.copernicus.org/preprints/egusphere-2022-515/`.

[Langhans et al.(2012)Langhans, Schmidli, and Schär] Wolfgang Langhans, Juerg Schmidli, and Christoph Schär. Bulk convergence of cloud-resolving simulations of moist convection over complex terrain. *Journal of the Atmospheric Sciences*, 69(7):2207 – 2228, 2012. doi: 10.1175/JAS-D-11-0252.1. URL `https://journals.ametsoc.org/view/journals/atsc/69/7/jas-d-11-0252.1.xml`.

[Lorenz(1969)] Edward N. Lorenz. The predictability of a flow which possesses many scales of motion. *Tellus*, 21(3):289–307, 1969. doi: 10.3402/tellusa.v21i3.10086. URL `https://doi.org/10.3402/tellusa.v21i3.10086`.