Review of ACP-2018-0707

Title: Impacts of upstream moisture structure on a back-building convective
    precipitation system in south-eastern France during HyMeX IOP13

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General Comments:

This study investigates the impacts of upstream moisture structure of the HyMeX IOP13 case through two sets of sensitivity tests, one altering the moisture content below 1 km in the marine boundary layer (MBL) while the other altering that over 1-2 km above sea-level (ASL). The topic is interesting and quite important and I do not have a major issue with the method and basic conclusion. However, I think that this paper can be much improved with some additional work (some of which I think is necessary). Therefore, I recommend "major revision" before acceptance. Below, some major and minor comments are given.

Major comments:

1. I do not think that the authors have picked all the important (and relevant) parameters (related to rainfall or precipitation system) to be shown and examined/discussed in this paper, in order to look deeper into what is going on physically in their numerical tests (beyond just describing their results). Right now, much of the discussion is quite descriptive, and in my opinion it does not shed enough light on the physical mechanisms leading to the differences in results as seen in the figures. I would like to see the authors put in more efforts to discuss how the changes in moisture (in MBL or in 1-2 km ASL) affect the rainfall system's structure (organization mode) and perhaps the back-building (BB) behavior in particular. One of the issues that affects the interpretation is the variables RR_{acc} and RR_{sum}, which are not defined very clearly in the text or reflective of what they mean (p.8), and I recommend the authors go through them carefully and perhaps use a table to summarize all those chosen. Some other parameters (linked to convective triggering, stability, and cold pool) that they may consider include (but not limited to): total water production (in ton or m^3) from event, the production over land versus over sea, level of free convection (LFC, beside CAPE) and/or CIN averaged over the source region, duration of linear organization and/or BB behavior, strength/size/duration of cold pool, etc. Another thing that may also help is adding four more experiments of ± 3 g/kg. Right now, jumping from 2 to 5 g/kg (which is quite a big change as shown in Fig. 5) seems too much.

2. From Figs. 6 and 12, it is clear that when the moisture is added (or removed) from the MBL (or 1-2 km ASL), some parameters change in a rather consistent fashion but others not (e.g., DRR_{15} in MST2P in Fig. 6f, or that in DRY1M in Fig. 12c). Also, some changes are easy to understand while others are trickier and not as straight forward (e.g., RR_{sum} in MST5P in Fig. 6d), I suppose. While an understanding at the physical level is important (as stated in my major comment #1 above), before that, it is also important to clarify whether such an inconsistency is indeed a response to the change in the moisture in the model, or arises simply due to the nonlinearity of the processes. Right now, this possibility is not considered by the authors but I think they should. The authors should at least examine the trajectories in the ± 5 g/kg tests to check whether the source region of the deep convection (and for the cold pool) remains the same in those runs as in CTRL (and they should also show those trajectories in the CTRL). If yes, an examination of what is going on physically would be more meaningful, but you would need a different interpretation if not.

3. This is related to my major comment #1 above. In the DRYxP runs, where the layer of 1-2 km ASL in the source region (for the cold pool) is moistened, the cold pool at 1500 UTC apparently enhances with more moisture (Figs. 15d-f vs Fig. 4c). The reason is not yet completely clear to me. The authors should elaborate on this too, if possible.

4. The English and fluency of this paper can be improved. I recommend that some of the authors can be of more help in this aspect.

Minor comments:

1. P.2, ln. 15-16 and other places: Usually, the references would be given according to the year of publication.

2. P.2, ln. 16: Is Romero et al. (1999) or (2000)? It is not consistent here with the reference list. Similar issue for Ricard et al. (2011) or (2012) in ln. 20-21.

3. P.3, ln. 11: Bielli et al. (2012) is not listed at the end. Similar issue for Ricard (2005) and Seity et al. (2011).

4. P.3, ln. 19 (also ln. 21) and many other places throughout the text: Here, a hyphen in south-eastern is not needed (just southeastern).

5. P.4, ln. 4: Has the term "Meso-NH" been defined already?

6. P.4, ln. 23: A reference for RRTM should be given here.

7. P.5: ln. 3: The acronym "AROME" is already defined two lines above.

8. P.5, ln. 22: Here, it says that the horizontal wind (u/v) is at 500 m ASL, but at 925 hPa in the caption (p.24, ln. 5). Please check and correct the wrong one.

9. P.7, ln. 12-17: From Fig. 5, I suppose that the mixing ratio (BTW, few would use an acronym for it, just the symbol *r*) is bounded by 0 and the saturation value. While the lower bound of 0 g/kg is noted here, the upper bound is not (and should be).

10. P.7, description of Fig. 5b: The mixing ratios converge back to the observed value at 2.1 km instead of 2.0 km similar to the MST tests. It is a minor point but is there a reason for this?

11. P.8, ln. 4-11: Some of these parameters are not clearly described, especially RR_{acc} and RR_{sum} (please also see major comment #1). I understand that RR_{acc} is the peak 6-h accumulative amount but its name should reflect this (by comparison, RR_{max} would be better). For RR_{sum}, I suppose it is an averaged amount, but for where and for what accumulation period?

12. P.8, ln. 12-20: Here, the authors do not need to go to such a detail and say which figure shows what later, but what will be shown (and for what purpose) should be enough.

13. P.9, ln. 1: The "x" in the experiment names (which represent 1, 2, or 5), at their first appearance in text, should be explained more clearly, if possible.

14. P.9, ln. 2: Shouldn't you use 0.67 and 0.4 for the factors here, instead of their reciprocals of 1.5 and 2.5?

15. P.9, ln. 3-4: … the time for precipitation ≥ 5 mm …

16. P.9, ln. 20: … mountainous region close to …

17. P.9, ln. 25-28: The description here is not clear.

18. P.9, ln. 29: Here, θ_e has already been defined, so why not just use it.

19. P.10, ln. 4 (and other places): Please change vertical wind into vertical motion (or in this case, upward motion) for better clarity.

20. P.10, ln. 5-6: The strength of cold pool and precipitation amount is a chicken and egg relationship in terms of the cause/effect, in my opinion.

21. P.10, ln. 20-23: The sentence here is not clear and should be improved.

22. P.11, ln. 1-11: Some of the description here is quite repetitive.

23. P.11, ln. 23-26: This sentence needs to be revised for better clarity.

24. P.12, ln. 23: What does the part "a more realistic location" mean? Please clarify.

25. P.13, ln. 3-5: Can the authors elaborate on the reason why? Perhaps, it is related to the entrainment process (please see my major comment #1). Also, the authors use the term "bubble" too much, and it can be confusing.

26. The authors may want to consider some plots showing the location of first convection initiation in different runs, if they can help to shed more light.

27. P.15, ln. 24-27: Here, I suppose that the authors should broaden their scope and not focus on another single event.

28. P.23, Fig. 2b and other similar plots: The last point should also be labelled for

better clarity, if possible. Also, if the color shades for topography can be lightened (since you already have Fig. 1), that may also help.

29. P.26, caption of Fig. 6: The description here for RR_{acc} and RR_{sum} is not the same as that in the text (and not clear either). Please also see major comment #1 and minor comment #11.

30. P.27, Fig. 7: What are those ellipses in the panels (also in many other figures)? I suppose that they depict the area of interests, but they are never explained in the caption of any figures.

31. P.28, Fig. 9: The y-axis is missing a title. And, is the CAPE calculated for surface parcels (same for Fig. 10)? Please clarify. Also, sensitivity area is better than sensitivity bubble…