

Editor

Additional private note (visible to authors and reviewers only):

Dear Dr. De Martin,

I returned your manuscript to Reviewer 1, who now recommends rejection; Reviewer 2 was unable to provide another review, so I recruited a new reviewer (Reviewer 3) who recommends major revisions. Reviewer 1 notes that you have ignored most of his comments, including e.g., the fact that not all cities lead to UDIs, and the fact you're your simulated supercells don't interact with the areas where the environment is modified the most. I also noticed that while you have provided rather detailed responses to the original Reviewer 2, most of that material has not been included in the revised manuscript. The new reviewer, Reviewer 3, has not identified any showstoppers, but has a number of comments, requesting additional information or clarification, as well as more honestly discussing the limitation of the study.

Taken together the paper now has garnered two rejections and two major revision recommendations, with no clear trend between the initial submission and the revision. For the paper to be published, it is critical that you make an honest effort to not just "argue away" the comments by the reviewers, but meaningfully include them in the revised manuscript. Please let me know if you have any questions (johannes.dahl@ttu.edu).

Best regards,

Johannes Dahl (WCD co-Editor)

Dear Dr. Dahl,

We thank you for giving us the opportunity to revise the manuscript. We took all the referees' comments very seriously, and all of them have been addressed in the revised version.

A point-by-point response to referee 3 is provided in a separate attached document. All of their comments have been addressed through additions, modifications, and deletions in the manuscript.

Below is a summary of the further modifications made to address the primary concerns you have highlighted and other outstanding issues identified by Reviewers 1 and 2.

Regarding Referee 1, one critical point was the lack of discussion on the fact that not all cities lead to a UDI (comment 4). This point is now addressed in the Introduction, at lines 21-27, and acknowledged in the Discussion at lines 456-458. Even though Urban Wet

Islands exist, several studies (cited in the added discussion) note that they are more common in winter, at night, or in dry regions. Urban Dry Islands, instead, are more common in the warm season and in the late afternoon. Thus, during the typical season (warm season) and time of day (late afternoon) for severe storms, UDIs are recognized as a common feature. Lastly, we would like to point out that we did not impose the UDI as a precondition: this feature was independently developed by the model. The reviewer was also skeptical about the reduction of CAPE induced by the UDI. In the revised manuscript, we cite two articles at line 453 of the Discussion that, as early as 2012 and 2014, linked the reduction of moisture in urban areas to a reduction in CAPE.

The other critical point was that the storm does not interact with the area where the environment is most modified (comment 1 of reviewer 1). We ran an additional experiment to address this concern, called "storm_shifted", in which the storm crosses the urban plume more directly. The results are shown in a new Figure 16 and discussed in a new section of the manuscript (Section 4.5, lines 424-433). The results do not change in that case, remaining consistent with our overall analysis.

The referee was also skeptical that the results are specific to the wind profile chosen. We acknowledge in the Discussion at lines 468-473 that a storm with a strongly veering hodograph may propagate into the region with positive SRH perturbations, potentially leading to storm intensification. This limitation is also highlighted in the Conclusion at line 514.

Comment 3 of referee 1 (how deep is the UHI?) is answered with the new cross sections shown in Fig. 16b,c.

Comment 5 of referee 1 is further addressed by a description of the urban parametrization used in this study, at lines 104-112 of the Methods section. This aspect is recognized as a potential limitation in the Conclusions at lines 521-525.

Regarding reviewer 2, the additional analysis conducted to address their comment 1 was further deepened (despite reviewer 3 suggesting that this part be removed), and cross sections along the storm motion were analyzed. These cross sections show the upstream propagation of urban perturbations, which we believe satisfactorily explains the early storm demise in some ensemble members. We believe this analysis was relevant for interpreting the results, so we decided to include it as an additional Figure 16b,c. We added a discussion of these results to the new Section 4.5, at lines 434-444.

Figure R9, which was used to address comment 2 of reviewer 2, is now included in the main manuscript as Figure 12a.

We acknowledge that the manuscript has significantly improved during this revision process. The limitations and different nuances of this complex problem are now disclosed, making the article more complete, clear, and thorough. We hope you appreciate all our efforts to address the reviewers' comments and that you will still consider it for publication.

Thank you,
Francesco De Martin
on behalf of all the authors

Reviewer 3

Recommendation: Major revisions

Summary:

The authors use a rather sophisticated urban parameterization scheme to assess the impact of an urban area on simulated supercells. The results suggest that the overall effect of a city is to weaken the supercell. I found the analysis interesting and have not found any significant methodological flaws; the presentation is reasonable clear overall, though there are a lot of statements that need to be backed up by figures (or omitted). I do think a lot of faith is put into rather minute details such as building height and e.g., roof material, and it is hard for me to believe that all the effects are accurately represented in a simulation with such coarse grid spacing ($dx = 1$ km), but admittedly, there is not much one can do about it (except discussing potential shortcomings). I do have a number of specific comments, and a few overarching ones that I hope the authors will consider. Overall I recommend major revisions.

We thank the reviewer for the very careful analysis of our manuscript. We found the comments very constructive and helpful in improving the quality of the manuscript. We have addressed all of them through additions, modifications, and/or deletions. A point-by-point reply to each comment is provided in this document, and references to the lines where the modifications were made in the manuscript are reported.

Main comments:

1. My main concern is that the authors take the output based on the urban parameterization at face value. I think some caution is warranted. Can we be completely confident that the effect of building height is accurately represented at a grid spacing of 1 km? I acknowledge that to some extent one has to trust the parameterizations, but adding a bit of discussion with appropriate caveat emptors in the discussion section would be beneficial.

In BEP/BEM, the building-height information is converted into a vertical urban morphology: the scheme builds a height distribution for the urban canopy and derives quantities such as the probability of buildings extending above each canopy level, frontal area density, urban volume fraction, and wall/roof/ground surface areas. These quantities directly affect the momentum, heat, moisture, and TKE source terms passed from the urban canopy to the atmospheric model.

Therefore, building height is not simply a static input or a roughness proxy. It modifies canopy drag, radiative exchange between walls/roads/roofs, heat storage in building materials, anthropogenic/building energy fluxes, and the vertical distribution of turbulent mixing. These tendencies are then coupled to the boundary-layer, surface, and radiation calculations. Thus, at 1 km resolution the model can accurately represent the aggregate, grid-cell-scale effect of building height, which is the scale relevant for our simulations. For instance, see Zonato et al. (2021) for the effect of building heights on temperature using BEP-BEM.

In the revised manuscript, BEP-BEM is introduced and briefly explained in the Method section at lines 104-112. Also, the use of a parametrization instead of direct solution of small-scale building-induced processes is acknowledged as a limitation of the work in the Conclusions at lines 521-525.

2. Carefully consider if all of the experiments need to be presented. I got a little confused by the end of the methods section because of the sheer amount of experiments and all the parameters that were varied. E.g., is the bubble experiment needed to get your point across? Maybe the answer is yes, but whatever you can simplify/shed would help.

The bubble experiment (now called storm_shifted) is crucial to address the concerns of another reviewer. So we kept it in the article, and we needed to move the supplemental figure with its results to the main manuscript (now Fig. 16).

To address the reviewer's comment, we removed the NYC experiment from the article, since it's not fundamental to the narrative and conclusions of the article.

3. Naming convection of experiments: Please explain what the different numbers mean (30km_2); also, these just seem to be the main classes of experiments since the names don't include all parameters that were varied. A few sentences about the naming philosophy would be helpful.

We modified the naming convention to make it clearer. The "city size 1" set of experiments is now simply called "city size," and "city size 2" is called "city size uf1," since the only difference with respect to the other set is the urban fraction (1.0 instead of 0.63). We also changed how the individual experiments are named: "uf0.63_xkm," where "x" is the size of the city in that experiment, for the "city size" set, and "uf1_xkm" for the "city size uf1" set. Hopefully, this makes the naming convention more consistent and less confusing.

4. I'm confused about the seemingly random use of numerous supplemental figures. Everything relevant to tell your science should be included in the manuscript. The supplemental figures are really just for ancillary information (maybe an animation).

We moved a revised version of Fig. S5 to the main manuscript (now Fig. 16a) and removed Figs. S3, S8, and S9 to reduce the number of supplementary figures. The other five supplementary figures were not removed, as we believe they provide deeper context into our results for a researcher wishing to explore the topic more deeply beyond the primary research study presented in the paper; however, they are not included in the main manuscript to avoid excessively increasing the number of figures there.

5. Can you show the hodographs at the time the storms interact with the city? Usually when initializing the model with surface drag switched on, an inertial oscillation results, which will affect the low-level hodograph down the road.

The hodograph is shown in the new Fig. 13. It's roughly the same as shown in the manuscript in Fig. 1b. No significant modifications occur in the simulations between 4 and 5.30 pm in the parts of the domain not affected by the storm. However, we confirm that the hodograph changes after model initialization (at 10 am), as can be seen in Fig. 1. We discuss this at lines 155-158.

6. Can you show an urban sounding to demonstrate that the CAPE reduction indeed comes from the reduction of low-level moisture?

Fig. 13 has been added to address this comment, and a sentence to emphasize this point has been added (lines 391-393).

7. I was confused about the experiments using city sizes 1 and 2, which seem to have the same diameters per tables 4 and 5.

Following also comment 3, we modified the naming convention of those experiments. The "city size 1" set of experiments is now simply called "city size," and "city size 2" is called "city size uf1," since the only difference with respect to the other set is the urban fraction (1.0 instead of 0.63).

Specific comments:

1. Line 22: Also mention that there are urban wet islands, in general. Also, can you add some information about why sometimes there is a dry and sometimes a wet island over cities (I found that aspect interesting)? This would be helpful for readers not as familiar with this topic.

We added a discussion in the introduction to present UWIs and UDIs at lines 21-27.

2. Line 35 and line 327: I'd avoid the term storm splitting, which is a specific process tied to the pressure field at the flanks of supercells. In my reading the referenced analysis merely

noticed that storms bifurcated, which could happen in a variety of ways that don't involve supercell dynamics.

We changed from "storm splitting" to "storm bifurcation".

3. Line 60: Broken grammar (the similar model settings...)

Addressed.

4. Line 76: Report the grid spacing here.

Grid spacing is reported at line 80.

5. Line 102: Add a few sentences describing these schemes to make the paper self-contained.

BEP-BEM models are now presented in the method section (lines 104-112) to address this comment.

6. Line 111: Is the first sentence needed (...choices carefully addressed)?

Not really. It has been removed.

7. Line 135: Previous Preceeding; also last two clauses are repetitive.

We are sorry, but didn't understand this comment.

8. Line 143 and elsewhere: Usually these are called "left/right mover" or "left-/right- moving supercells" but not "left-/right-mover supercells".

We modified "left-/right-moving supercells" everywhere in the manuscript. Thank you for pointing that out.

9. Line 149: This is the third time you are stating this.

We removed that sentence.

10. Line 163: Add "as input" after parameters; I struggled understanding what you meant at first.

That sentence has been rephrased to address this comment (lines 167-168).

11. Line 169: Please explain in words what λ_b and λ_p mean.

λ_b and λ_p are now explained in words at lines 183-184.

12. Line 175: Add "Italy" after Milan.

Added.

13. Line 178: I think you haven't mentioned the urban fraction of 0.63 yet. I'd first state that this is the default value, and then that you vary it.

A sentence was added at lines 189-190 to state which default parameters are used in the “city size” experiments.

14. Line 196: I didn’t follow this argument. How does this experiment separate the effects of the modified environment and storm dynamics? Arguably, the dynamics depend on the environment? Simple reword might fix this.

That experiment does not separate the effect of the modified environment from storm dynamics. It instead separates the dynamic effects of the city (i.e., enhanced drag, building-barrier effects...) from its thermodynamic effects (UHI, UDI...) on both the pre-storm environment and the storm itself. We modified the sentence, hoping it is now clearer (lines 208-210).

15. Line 205: Add a few sentences about how the ensemble was generated to make the paper self-contained.

The SKEBS approach is now briefly explained at lines 217-218.

16. Line 208: Wavenumber of what field?

Of both potential temperature and streamfunction. This has been clarified now in the manuscript at line 221.

17. Line 219: Not sure I understand how the left mover triggers a new supercell.

Removed to avoid confusion.

18. Line 238: I think “the variables” haven’t been introduced yet (or maybe I missed it?)

Yes, variables are mentioned previously at lines 237-238 and referred to Table 9.

19. Line 247: Not sure what you mean. E.g., $w = 0$ has a physical meaning (no storm)

As suggested by the parentheses, that problem emerged while studying potential temperature and equivalent potential temperature. Having noted that, we removed null values for all the other variables as well (including those like w , where, we agree, there is physical meaning), and found that statistical significance improved. We therefore decided to keep that filtering.

20. Line 271: I didn’t understand the last sentence (This occurs...).

That sentence has been removed to avoid confusion.

21. Line 308: Refer to a figure here.

The results discussed in that line refer to Table 11, which is now cited in the text (line 316).

22. Line 310: Why do you expect taller buildings to lead to stronger low-level storm updrafts?

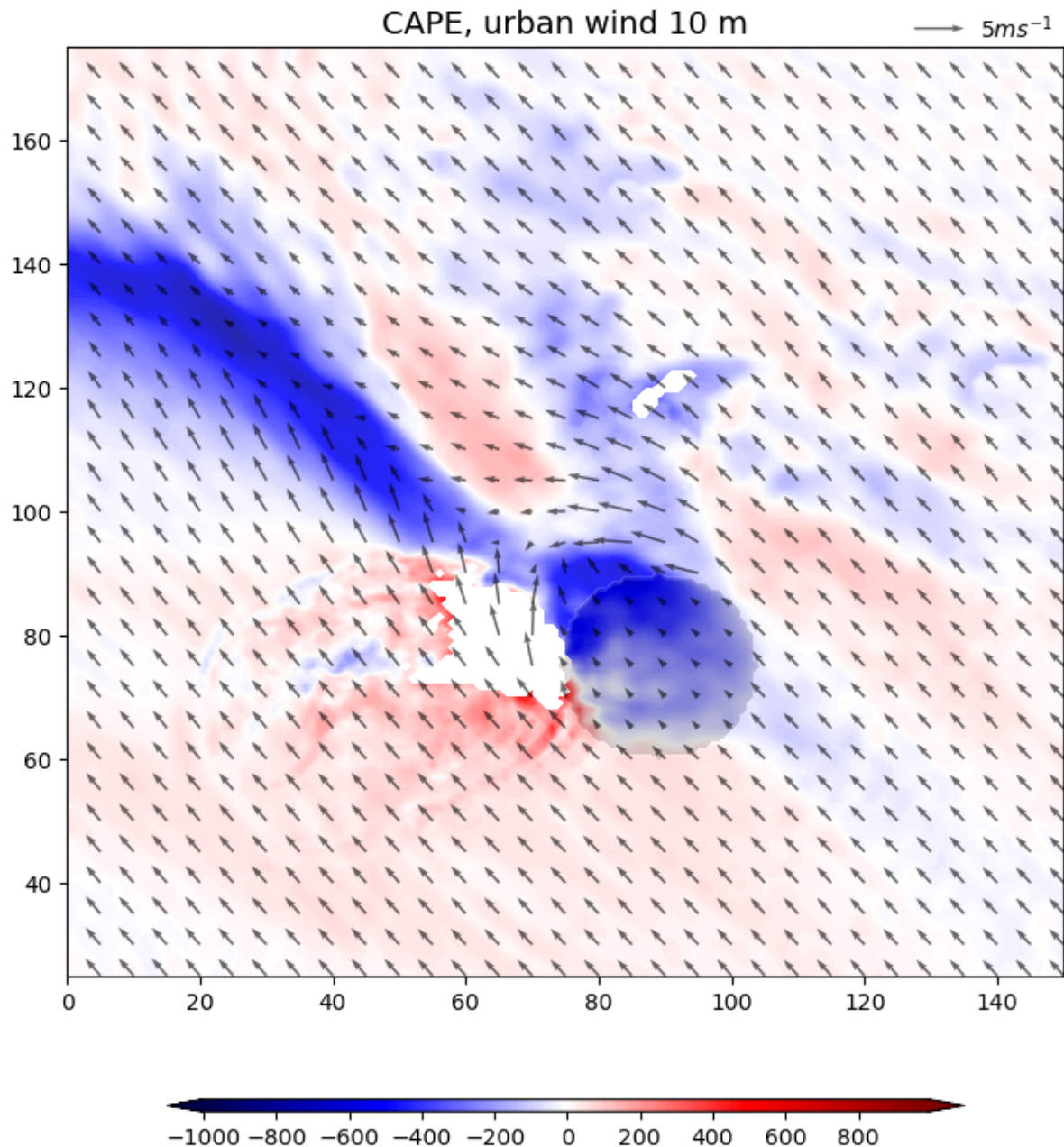
This should be a consequence of the building-barrier effect on the surface flow. The convergence induced by tall buildings should promote vertical motions, due to mass conservation. This has been clarified in the revised manuscript at lines 318-319.

23. Line 312: Please refer to a figure here as well.

The result is observed from Table 11, which is now cited at line 322.

24. Line 330: Why are the environmental parameters considered at initiation time and not when the storm approaches the city, so that you can sample the actual storm environment?

Environmental parameters do not change much between 4:00 pm (when the storm is initiated) and 5:30 pm (when the storm hits the city), as can be seen in Fig. SM1. By 5:30 pm, the environmental variables are modified by the approaching storm, as shown in the following plot, where the CAPE difference between the control_30km and no_urban experiments is displayed (to be compared with Fig. 12a). To disentangle the storm's modification of the environment from the urban modification of the environment, we chose to plot the variables at convective initiation instead.



25. Line 343 ff: The argument involving gravity waves is incomplete. If you wish to make this point, a deeper analysis and additional plots are necessary. You'd need to demonstrate the pathways leading from from gravity waves to entrainment and stronger CINH. Alternatively, you could omit this argument altogether.

That additional analysis was important for addressing another reviewer's concern, so we need to retain it to explain the early storm demise in some ensemble members. Instead of plotting horizontal sections, we now present vertical cross sections, which we believe provide a clearer representation. We do not refer to gravity waves; rather, we observe that the urban-induced modifications propagate upstream. Explaining the underlying mechanism is beyond the scope of the present article, but this behavior provides a plausible explanation for the early storm demise. We removed the corresponding supplemental figure and incorporated the vertical

cross sections into the main manuscript as Figure 16. These results are discussed at lines 434-444.

26. Line 361: Please explain that an understanding of SLP is needed to understand the flow perturbations. As it stands, considering pressure perturbations seems rather unmotivated.

Thank you for pointing that out. A sentence to introduce SLP was added in the revised manuscript at line 362.

27. Line 371: How was the pressure decomposition performed?

The elliptic partial differential equations that define p_d and p_b are solved using a Gauss–Seidel iterative method, using as p_0 the `no_urban` pressure field. This information has been added in the revised manuscript at lines 373-374.

28. Line 380: Write SBCAPE out once?

SB CAPE is now written out at line 382.

29. Line 390: Was this analyzed before the storm developed? Else the w field of the storm would contaminate the analysis. Please add a few details about how the fields were calculated.

Yes, also these fields are analyzed at 4.00 pm to avoid storm contamination. This important aspect is highlighted at lines 396-397.

30. Line 401: Why “therefore”? Please (uniquely) connect the effect of building heights with the fact that there are no upward velocities in the `no_thermo` experiments.

We agree that “therefore” was not appropriate in that context. The whole paragraph has been rephrased to make this argument clearer (lines 407-411).

31. Line 403: Refer to a figure here.

That result is a consequence of Table 11 that is now cited here (line 411).

32. Line 417: Define downstream: Relative to the 10 m winds? Also, whether a supercell weakens or strengthens could depend on which lobe of the SRH perturbation the cell interacts with. Consider including caveats such as these in the discussion.

It’s downstream with respect to the storm motion. We modified “downtrack the city” to differentiate with respect to the surface winds (line 447).

We also agree about the caveat that the reviewer mentioned. We discuss these possibilities at lines 468-473 of the Discussion, connecting our results to the findings of a previous case study (Lin et al., 2021).

33. Line 427: Do rivers meaningfully moisten a city? Wouldn’t they also cool the city?

We examined that case study more closely and identified several interesting similarities with the urban-induced supercell that develops in some of our simulations. Consequently, we revised that section and reframed the discussion of that study in the context of our results (lines 468-473).

34. Line 443: Add “Germany” after Berlin.

Added.

35. Line 446: I agree, meaningful modifications in storm behavior might be more likely in weakly forced situations; But I don’t understand the last clause “and can also occur under strongly forced conditions”. Do you have references to back that statement up?

We are referring there to the possibility of producing severe storms along the convergence downstream of the city. We rephrased that sentence to make the message clearer (lines 482-483). References have also been added.

36. Line 449: You state that this aspect hasn’t been emphasized but then cite two paper that have in fact emphasized it.

That sentence has been removed and merged with the previous one that was addressed in comment 35.

37. Line 480: Is “considered” the best choice of word? You have demonstrated that within the limits of the analysis, the effect is negligible.

We removed “is considered”.

38. Line 488: Calling your results robust sounds a little self-serving (I’d let the reader, and time, determine how robust the results are).

We removed that sentence.