

Editor and Reviewer 1

I asked the authors to revise their paper to fully address the reviewers' comments.

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

Reviewer 1 still recommends rejection, and reviewer 3 recommends minor revisions. In my own reading, the paper generally does meet the bar for publication; sometimes not all differences with reviewers can be resolved during the review process. However, Reviewer 1 does raise important points, which you need to address before the paper can be accepted.

We thank the editor for his understanding of the situation and for being open to advancing our manuscript. We also thank Reviewer 1 for providing more specific points of contention, which helped us resolve outstanding issues that had previously been unclear.

Reviewer 1 cited numbers from several papers we referenced that, at first glance, appear to disagree with our simulations. However, the reviewer compared daily, seasonal, or yearly averages with our result, which refers to a single afternoon; urban dry islands have diurnal and seasonal cycles (as the papers the reviewer mentioned show). Thus, a daily, seasonal, or yearly average naturally smooths out these fluctuations, and the resulting value is lower than the values we obtain. We checked every reference the reviewer mentioned and found no relevant inconsistencies with our results. We provide a point-by-point response below.

In addition, we addressed all other comments from the editor and Reviewer 1. We also enlarged the labels in almost every figure to improve readability.

1. Regarding the framing of several citations, which do not seem to support some of your claims: Please carefully review the citations and ensure you represent them faithfully.

We carefully reviewed the citations and found no relevant inconsistencies. Reviewer 1 compared values obtained from yearly, monthly, or daily averages with our values obtained from a single time step. As we clarify in the introduction and discussion, the UDI exhibits diurnal and seasonal variability, and warm-season afternoons are usually the period when it is strongest. It is not surprising that our values are slightly higher than the yearly averages reported in the other papers. In one of the papers we cite (Lokoshchenko et al., 2017), it is written that: "In the afternoon in anticyclonic conditions, however, the difference of [relative humidity] between the urban center and the park zone may be much bigger—up to 30% as was observed in Munich (Kratzer 1956)." Our 13% value is well below the maximum reported in the paper.

We provide a clarification for each reference mentioned by the reviewer.

Meili et al. 2022 found that the average UDI magnitude is 0.5 g/kg. Their figures 3 & 4 indicate specific humidity differences of less than 1.5 g/kg. From the abstract, they also claim that the max relative humidity difference between urban and rural areas is 10-11%. Both of these are smaller than the UDI (2 g/kg and 13% RH reduction) in this current study.

They found a mean reduction of 1.4 g/kg in specific humidity in the afternoon, averaged over all summers in a 29-year sample across several cities. Our maximum UDI of -2.0 g/kg, using the largest and fully urbanized (i.e., no parks/gardens) city (30 km in diameter), therefore seems reasonable.

Lokoshchenko 2017 found that the maximum relative UDI is about 8.5% with the average closer to 4%.

That average is computed over 12 months. This includes the winter months, when UDI can be reversed (i.e., higher moisture in the city). Thus, the comparison is not fair. They also say:

In the afternoon in anticyclonic conditions, however, the difference of F between the urban center and the park zone may be much bigger—up to 30%, as was observed in Munich (Kratzer 1956).

Luan et al. 2022 documented a decrease in specific humidity of 0.5 g/kg in urban areas.

We could not find the specific reference to the 0.5 g/kg reduction in specific humidity in this reference. However, Fig. 6 in Luan et al. (2022) shows a 12-month average reduction in RH of 12% (i.e., including periods of the day and year when UDI can be reversed) for 2018 and 2019. Our 13% reduction on a single warm-season afternoon agrees well with this result.

2. Regarding the urban dry island and the unusually large CAPE reduction: While it does come out of the parameterization scheme, you please add some caveats that this scheme may overestimate the degree of drying (based on the extant literature), and hence the CAPE reduction.

As with the apparent relative humidity disparities between our manuscript and the references cited by Reviewer 1, the cited references for CAPE also report averages over several days, months, and years, which smooth out diurnal and seasonal variability, whereas our CAPE reduction values are for a single afternoon in the warm season. In particular:

Figure 12 of Wang et al. (2012) indicates urban areas reduce CAPE by 10-40 J/kg.

That is the average of a simulation from 1 December 2006 to 1 January 2010.

Figure 10 of Yang et al. (2014) shows a reduction in CAPE of approximately 40 J/kg.

Those simulations were run for each summer (June, July, and August) during 2008–2012; therefore, the 40 J/kg reduction reflects a 4-year average.

In Yang et al. (2021), all of the urban-induced decreases in CAPE are smaller than 100 J/kg (their figure 5).

They ran those simulations over a 4-day period from 00 UTC on July 13, 2021, to 00 UTC on July 17, 2021. The diurnal cycle of UDI is therefore likely flattened. Moreover, the environment was different, with widespread intense precipitation.

Reames and Stensrud 2018 (their Figure 17) show a decrease of at most 450 J/kg around the urban area. At first glance, this seems closer to the current claimed reduction in CAPE. However, that is only about a 10-15% reduction of the >4000 J/kg that were present over the larger area in their simulations.

In this study, a much larger value of CAPE reduction, more closely aligned with our study, is consistent with the fact that the study was for a single afternoon in the warm season.

The Holley paper does not actually include any CAPE values, so it is not particularly useful here. I also believe this citation is incorrect. On line 455, the authors associate this study with previous works that have found reduced CAPE due to a reduction in urban moisture. However, Holley et al. did not actually show this, they merely speculated that the reduction is “most likely” due to a reduction in urban water vapor.

We modified the text to tone down the claim ("with reduced moisture availability suggested as a possible explanation"), but we do not see any reason not to cite that paper, since they found a reduction of CAPE in urban areas and suggested a reduction in moisture as an explanation, as we also did.

For these reasons, we do not feel a strong need to justify a discrepancy between our CAPE reduction and that reported in these other papers. Nevertheless, we note in the Discussion that the 1000 J/kg reduction in CAPE applies only to the largest city and may be slightly overestimated due to the choice of relatively high soil moisture (lines 453–454). As already clarified in the Introduction, this was needed to avoid excessive drying of the boundary layer in the afternoon.

3. Regarding the representativeness of the interaction: I am okay with analyzing a single hodograph, but please add a caveat at the beginning of p. 33 (where you already address potential different outcomes) that a wide variety of interactions, some of which may include storm invigoration, may occur based on the precise trajectory of the storm and the topology of the city-induced perturbations, which would depend on the ground-relative flow.

We included this caveat at line 473-477 of the Discussion.

Reply to original reviewer 1 point 3:

3. I shared my concerns about the representativeness of their chosen hodograph and how I believe it may be impacting their results. The additional experiments that have been conducted do not address my original concern about the hodograph and the presence of easterly winds near the surface. I suggested that the authors modify the hodograph—by adding a constant wind speed to each level—to remove the easterly winds near the surface. By doing this, the ‘urban plume’ will be completely to the right of the urban area at all levels and oriented southwest-to-northeast. The primary supercell will interact with it for a much longer duration than simply crossing the

plume's short axis, which is what is happening in the new simulations.

This modification to the hodograph does not impact overall wind shear and thus will not impact storm structure. In fact, this is a common technique used in idealized storm modeling to adjust storm motion. That being said, I do acknowledge that it would impact overall storm propagation speed, so the authors would have to account for that by moving the location of the initial thermal bubble. The authors would also likely want to preserve the magnitude of the near-surface winds as to not impact surface fluxes.

We acknowledge that, in a different environment, the results could differ. Testing the sensitivity of these results to the environment would be an interesting avenue for future work, but we believe there is already more than enough material in the current manuscript. Following the editor's recommendation, we included a caveat about this point at lines 473–477 of the Discussion and mention this as future work in the Conclusions (lines 538–539).

4. Finally, Reviewer 3 notes the small labels on your figures—I agree that these need to be enlarged to be viewed comfortably when printed.

We enlarged the labels in almost every figure, as you suggested.

Reviewer 2

I read the revised manuscript, and overall I am satisfied with the revisions. I only have a couple of minor comments.

Thank you for the positive feedback!

Minor comments:

1. Line 166: Here you make a strong and important statement: Any changes of the storm can be attributed to the city—can you elaborate on how you reached this conclusion?

That statement refers to the difference between the numerical experiments (where the city is present) and the no-urban simulations (where it is absent). This is clarified in the revised manuscript at lines 166-167.

The storm is simulated in all these experiments, and the only difference between them is the presence or absence of the city (or different urban parameters). Thus, we expect that any differences in storm behavior arise exclusively as a direct or indirect effect of the city. We added "reasonably" to tone down the sentence slightly.

2. Line 7: You write that the pressure perturbation triggers convection, which is a little imprecise. Suggest stating that rising motion due to horizontal convergence in the wake of the city initiates the convection (or something along those lines).

We revised line 8 to express the concept more clearly. Thank you for pointing out this inaccuracy.

3. Line 20: Perhaps use "modification" instead of "shift"?

In the revised manuscript ,we use "modification".

4. Line 33: Reword "chemical reasons" (can a reason be of chemical nature?)

We use "interactions" in the revised manuscript.

5. Line 44: Add "e.g." before the Homeyer reference; they are not the first to define a supercell

Addressed.

6. Line 86: Add “e.g.” before the Manzato reference.

Addressed.

7. Line 157: Does the flow spin down due to the presence of surface drag? Is that what you are referring to (regarding the absence of the thermal wind)?

Ultimately, yes, surface drag, together with diurnal mixing, adjusts the wind profile in this way. We have explicitly mentioned surface drag in the revised manuscript (line 157).

8. Line 441: I see mostly rising motion in 16c, rather than subsidence where $\theta' > 0$.

We are referring to the area that extends upstream (toward A, around grid-points (65,65)) at an elevation of 1500 m. A reference to 1500 m is added in the manuscript for clarity.

9. Line 442: Is entrainment enhanced, or does entrainment have a more detrimental effect?

We think it has a more detrimental effect because of the lower θ -e of the air mass at the level. Revised at line 443.

10. Line 472: Add ‘a’ before southeastward.

Addressed.

11. Line 494: Ultimately, all environments are synoptically forced, no? (Even a stagnant pattern would be the result of synoptic-scale forcing.) Use precise wording (strongly or weakly synoptically forced).

Right! Sorry for the inaccuracy. Addressed.

12. Line 517: Perhaps a matter of personal preference, but I find statements such as these unpleasantly self-serving. Let the community, and time, decide whether your study “significantly advances our understanding of the interaction” between cities and storms.

We removed that sentence.

Figures:

All figures need much larger label sizes. Even on a large screen, most are unintelligible. This pertains to axis labels and colorbar labels of pretty much every figures (sorry I didn't notice that before!).

We enlarged the labels in almost every figure, as you suggested.