Summary

This paper investigates the effect of urban areas on supercells using a semi-idealized approach. The main findings are that supercells weaken when passing over an urban area. Overall, the paper is interesting but I believe that the authors' are overselling their results and are misrepresenting previous studies in an attempt to validate their current findings.

We thank the referee for the thorough review of our manuscript. In the revised version, we will place greater emphasis in both the Abstract and the Conclusions on the fact that our results represent one possible scenario of the interactions between urban land use and storms, rather than necessarily the most common one. Nevertheless, some radar climatologies suggest that this type of behavior may be typical in strongly forced environments (Lorenz et al., 2019; Kingfield et al., 2018). This prior finding is noted in the Discussion section (lines 421–428) and in the Conclusions (line 458).

The authors state that previous studies of interactions between supercells and urban areas 'lack generality' and rely on 'simplified urban representations'. This seems to imply that this current study 'fixes' those issues. However, this current study suffers from the same limitations.

This is the first study to employ the Building Effect Parameterization and Building Energy Model (BEP-BEM) to systematically investigate interactions between urban land use and severe storms. Previous studies have relied on highly simplified representations of urban areas (e.g., Naylor et al., 2024), single-layer urban canopy models (e.g., Reames and Stensrud, 2018), or have applied BEP-BEM only to case studies (e.g., Lin et al., 2021). While we cannot claim that this approach fully resolves the limitations of earlier work, it represents a step toward more comprehensively accounting for the complex processes occurring in urban environments. BEP-BEM better resolves the vertical structure of urban PBL, making the representation of urban environment more reliable.

The authors attribute their findings primarily to the presence of an urban dry island which reduces CAPE over the city. This is not a universal truth. There are many studies that have found that urban areas can increase CAPE. The driving factor for this difference is the overall synoptic pattern—dry conditions tend to lead to UDIs and a reduction in CAPE, while moist seasons can produce increased CAPE over urban area.

The emphasis on the urban dry island (UDI) arises from our numerical simulations rather than from an arbitrary choice. Previous studies have reported both higher and lower CAPE values over urban areas, indicating that the response is not uniform. We plan to mention this aspect in the revised manuscript. Furthermore, we are not certain that UDIs are characteristic of dry periods. During such periods, vegetation in rural areas tends to dry out, whereas urban vegetation may remain relatively green due to irrigation, potentially reducing or even eliminating UDIs.

Additionally, I believe that the results of this current study are largely a result of the environmental wind profile chosen. The wind profile is producing southeasterly flow near the surface, which is advecting the urban air to the northwest of the city. While this type of wind profile can be observed in the Great Plains of the US, it is not particularly common in other areas of the US. While studies in Europe are less common, there are several examples of severe weather environments that do not contain easterly wind at the surface.

While it is true that not all environments producing supercells are associated with southeasterly surface flow, this configuration represents one of the most common severe weather environments in the Northern Hemisphere. The presence of easterly low-level flow is important for generating sufficient veering in the wind profile, given the typical westerly flow aloft. For this reason, it is commonly used as a reference hodograph in studies of this type (e.g., Coffer et al., 2017).

Furthermore, the domain and/or hodograph can be rotated, making these results more general. Thinking of the results as city-relative make them more general than to only a situation with southeasterly surface flow.

Coffer, B. E., & Parker, M. D. (2017). Simulated supercells in nontornadic and tornadic VORTEX2 environments. Monthly Weather Review, 145(1), 149-180.

Since the right-moving supercell is not even passing over this ‘urban plume’ that produces positive vertical velocity and initiation of a new cell, I believe that a different wind profile would produce drastically different results.

The storm is actually passing over the urban plume (see Fig. R1). A sensitivity test moving the supercell further north, crossing a different part of the urban plume gave the same outcome (see Fig. S5).

In addition, I question some of the methodology, particularly the grid spacing. It has been known for over 20 years that sub-grid turbulence is not well represented at grid spacing of 1 km. The authors claim that they are addressing the physical processes that result in storm modification—something that they claim is lacking in previous studies—yet they employ a model configuration that is known to be deficient at resolving storm-scale processes. I do not understand the purpose of simulations with 1 km horizontal grid spacing. This is too coarse to study storm dynamics yet to fine to mimic operational convective-allowing models.

This is an important point that is discussed in more detail below in our response to Specific Comment 6.

Specific Comments

1. I think the results are only valid for this specific environmental scenario. For example, it is easily seen in figures 13 & 14 that the largest perturbations to CAPE and upward motion are found ‘downwind’ of the city at 950 mb and the primary supercell in your simulations is not passing over that area. In an environment without a near surface easterly wind, this area of upward motion (and strongest SRH anomalies) would be to the east of the urban area. A storm passing over this may be amplified. A similar argument can be made for SRH. Perturbations in SRH appear to be due to obstacle flow around the urban area. These perturbations would be different with a different wind profile.

We agree with the referee that extending these results to other environments may be questionable. We cannot claim that our findings are universally applicable. As such, the manuscript has highlighted new aspects of the interactions between urban land use and severe storms that contribute to expanding current understanding (see lines 421–428 of the Discussion and line 458 of the Conclusions). With that said, we had hoped not to make conclusions that are too sweeping. In our revision, we will further emphasize the reach and limits of our study’s findings in the Conclusions and in the caption of Fig. 16.

We wish to emphasize some points about the urban plume and its strong perturbations. The simulated supercell crosses the region with the largest CAPE perturbation (see Fig. R1). While it is true that it does not intersect the area of strongest upward motion, the additional “bubble” experiment (Table 3) was specifically designed to investigate this aspect. The results show that the supercell dissipates in this case as well, as noted in lines 287–289 and illustrated in Fig. S5. Furthermore, the perturbations in SRH arise because of the UHI (lines 387–389) and are not dynamically induced by the city itself (see Fig. 15f). For this reason, we do not expect remarkable differences in the sign of SRH perturbations changing the wind profile.

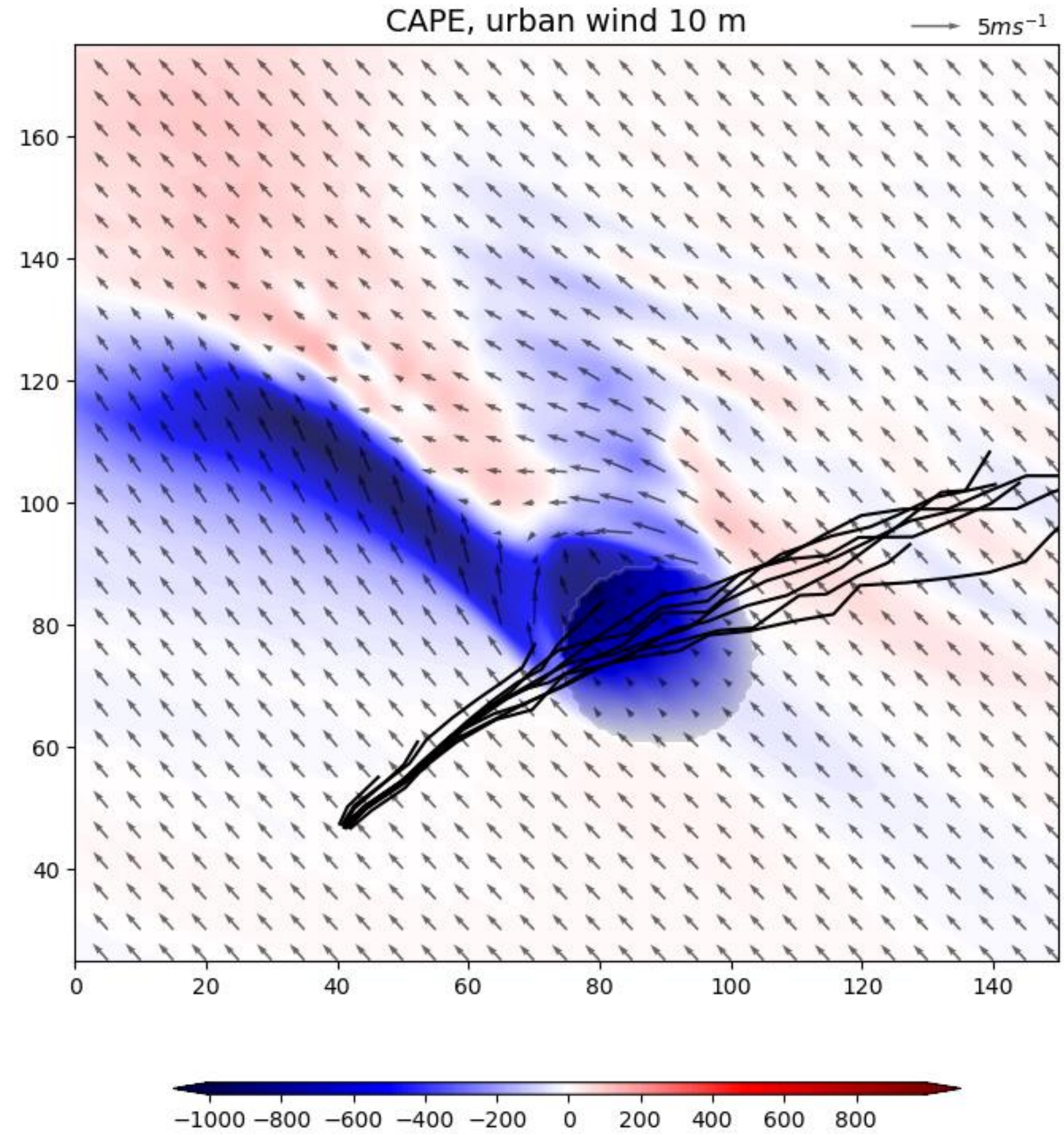

Fig. R1: mean ensemble difference of SB CAPE between the 30 km simulations and the no_urban case. Wind at 10 m AGL in the urban case is plotted. The black lines are the tracked storms in the 30km run.

2. UHI magnitude of 1.5 K, which the authors' state is in-line with a previous study (Hidalgo et al. 2010). However, that study is based on theoretical scaling arguments. The first line of that paper says that nighttime UHI magnitude can be as high as 5-8 K in European cities. The stated UHI value of 1.5 K seems to be what they calculated the UHI to be for a specific urban breeze—they then stated that this breeze is close to that observed in a specific city on a specific day. My main point is that this value seems small. Naylor and Mulholland (2023) and Naylor et al. (2024) found that a stronger UHI was necessary to induce substantial modification.

While UHI intensities of 5–8 K can be observed at night, our simulated UHI intensity of 1.5 K is within the expected range for daytime conditions. See, for example, Fig. 12 in Zonato (2020), Fig. 7 in Ribeiro (2021), Fig. 4 in Basara (2008), and Figs. 5–6 in Di Sabatino et al. (2020).

Naylor and Mulhholand (2023) and Naylor et al. (2024) assumed a prescribed UHI. We let the model generate the UHI by itself.

Basara, J. B., P. K.HallJr., A. J.Schroeder, B. G.Illston, and K. L.Nemunaitis (2008), Diurnal cycle of the Oklahoma City urban heat island, J. Geophys. Res., 113, D20109, doi:10.1029/2008JD010311.

Di Sabatino, S.; Barbano, F.; Brattich, E.; Pulvirenti, B. The Multiple-Scale Nature of Urban Heat Island and Its Footprint on Air Quality in Real Urban Environment. Atmosphere ***2020****, 11, 1186. https://doi.org/10.3390/atmos11111186*

Ribeiro, I., Martilli, A., Falls, M., Zonato, A., & Villalba, G. (2021). Highly resolved WRF-BEP/BEM simulations over Barcelona urban area with LCZ. Atmospheric Research, 248, 105220.

Zonato, A., Martilli, A., Gutierrez, E., Chen, F., He, C., Barlage, M., Zardi, D., and Giovannini, L.: Exploring the effects of rooftop mitigation strategies on urban temperatures and energy consumption, Journal of Geophysical Research: Atmospheres, 126, e2021JD035 002, https://doi.org/https://doi.org/10.1029/2021JD035002, 2021.

3. How deep is the UHI in your simulations? Does the depth match observations?

It is approximately 1.8 km, corresponding to the depth of the PBL. This is in good agreement with expectations (depth of the PBL from the sounding) and previous studies (see, for example, Fig. 16 in Miao et al., 2009).

Miao, S., Chen, F., LeMone, M. A., Tewari, M., Li, Q., & Wang, Y. (2009). An observational and modeling study of characteristics of urban heat island and boundary layer structures in Beijing. Journal of Applied Meteorology and Climatology, 48(3), 484-501.

4. There's a lot of emphasis on urban dry island. I would like to see more justification of the dry island assumption as well as the magnitude of the UDI, especially since several studies have noted that urban areas can increase CAPE. The authors are justifying the UDI magnitude with results from a single study (Meili et al. 2022), yet another study from the same journal shows that UDI and UMIs both occur with regularity (Huang and Song 2023). Other previous studies have noted (e.g., Huff and Changnon 1973) noted that urban

modification to convection is most prominent during wetter summers. In addition, the reduction in CAPE noted by the authors is much greater than that observed by Rozoff et al. 2003 and Reames and Stensrud (2018). Reames and Strensrud also note that the reduction in CAPE is 'generally neutralized as the storm approaches', while Rozoff et al. noted an *increase* in CAPE near the urban center. How can you be confident that your observed reduction in CAPE is a reasonable representation of reality?

In the present work, the UDI is not prescribed; rather, it is generated by the model. Although a UDI is not present in all cities, it is a common and well-recognized feature.

Reames and Stensrud employed a much more simplified urban model (a single-layer urban canopy model), whereas we used a multi-layer urban canopy model that was developed over 25 years ago and has been extensively tested (see, for example, Ribeiro et al., 2021).

While it is true that some studies have reported increased CAPE values in certain cities, other studies have found the opposite (e.g., Yang et al., 2014; Yang et al., 2021). We believe that some cities may exhibit higher CAPE values—particularly those in dry regions, where urban vegetation can enhance moisture and, consequently, CAPE. However, in many other cities, urban land use may reduce CAPE. This occurs because urban surfaces tend to suppress evapotranspiration relative to the surrounding vegetated rural areas.

Ribeiro, I., Martilli, A., Falls, M., Zonato, A., & Villalba, G. (2021). Highly resolved WRF-BEP/BEM simulations over Barcelona urban area with LCZ. Atmospheric Research, 248, 105220.

*Yang, L., Tian, F., Smith, J.A. & Hu, H. (2014) Urban signatures in the spatial clustering of summer heavy rainfall events over the beijing metropolitan region. Journal of Geophysical Research: Atmospheres, **119**, 1203–1217.*

*Yang, L., Ni, G., Tian, F. & Niyogi, D. (2021b) Urbanization exacerbated rainfall over european suburbs under a warming climate. Geophysical Research Letters, **48**, e2021GL095987. Available from: https://doi.org/10.1029/2021GL095987.E2021GL095987.2021GL095987*

5. How is your methodology any more sophisticated than that of Naylor et al. 2024? You are performing idealized simulations similar to Naylor et al. 2024 with a simple circular city (I believe with horizontally homogeneous characteristics) but at a reduced horizontal and vertical resolution. I do not understand how the use of BEP makes the simulation more 'realistic'. Changing the BEP parameters such as urban fraction, building height, etc are simply modifying surface fields. For example, changing the building plan area fraction modifies the surface drag coefficient. These experiments seem conceptually similar to that

of Naylor and Mulholland 2023 in which the surface roughness and skin temperature were altered.

In Naylor (2024), the city is represented simply as a thermal and roughness perturbation added to the domain. In contrast, we do not prescribe any surface–atmosphere perturbation. Instead, we explicitly simulate the effects of urban land use on the atmosphere. Additional model levels are introduced near the surface within the urban area, where the BEP–BEM subroutines resolve exchange processes among buildings, trees, vehicles, and streets. Thermal exchanges between buildings, including heat released by air-conditioning systems and reflection and absorption by building walls, are taken into account. Momentum loss due to wake diffusion is also represented. Since it's a multi-layer model, it parameterizes not only the level close to the surface, but all the atmospheric layers where the city is immersed. Therefore, the vertical diffusion of wind, temperature, and humidity is more strongly modified than in other approaches.

Admittedly, many of these processes are parameterized. However, it is preferable to include them in parameterized form rather than neglect them entirely. We argue that this approach is more appropriate, particularly given that the model has been extensively validated and shown to be reliable. Furthermore, this makes the simulations much more realistic.

6. Why is 1 km horizontal grid spacing appropriate for this study? It has been known for over 20 years that turbulence is not well resolved at this grid spacing and that that convection needs grid spacing of 250 m or less (e.g. Bryan et al. 2003).

   That is a good point, and of course a finer resolution allows for a better representation of deep moist convection. There are several reasons that led us to consider 1 km resolution an acceptable compromise for this type of study.

   1. The BEP–BEM scheme is not suitable at resolutions finer than 500 m. We are not aware of any studies that have implemented this model at resolutions of 250 m. Therefore, one may question whether running a very high-resolution simulation, capable of better resolving deep moist convection, together with a bulk urban parameterization would improve our understanding of the interactions between urban land use and severe storms. We have serious concerns about this.
   2. Simulations with horizontal resolutions of 100–1000 m fall within the gray zone ("terra incognita"), where PBL schemes are no longer reliable and LES approaches are not suitable, as the coarse resolution does not explicitly resolve all coherent eddies. Consequently, although deep moist convection may be

better represented, shallow convection within the PBL may not be realistically simulated.

3. At 250 m resolution, the required domain size becomes too large. A sufficiently large domain is needed to allow the supercell to develop (without being too close to lateral boundaries), interact with the city, and then evolve after passing over it (again, without boundary effects). In addition, we performed several experiments with 11 ensemble members each, which would require a level of computational resources beyond our capabilities. Markowski and Dotzek, who studied the interaction of a supercell with a mountain and faced similar constraints, used a resolution of 500 m (noting that they did not employ an ensemble approach).
4. It is true that turbulence is not explicitly resolved at this resolution, but it is still parameterized. While less accurate than explicit resolution, it is not absent.
5. We are studying the interaction of a supercell with an urban area, not the interaction of a tornado with individual buildings. We fully agree that in the latter case, much higher resolution would be required. However, for supercell dynamics, 1 km resolution is generally sufficient to capture the main processes. The pioneering work of Rotunno and Klemp (1985), which explained the fundamental dynamics of supercells, used a 1 km grid spacing, and their results remain a benchmark. One of the main limitations of such early simulations was the microphysics scheme (see Davies-Jones, 2015 for a review), which tended to produce excessive cooling. This highlights the importance of accurately representing subgrid-scale processes, such as urban exchange processes, in addition to simply increasing numerical resolution.

We plan to mention some of these points in the method section to motivate our choice.

Davies-Jones, R. (2015). A review of supercell and tornado dynamics. Atmospheric Research, 158, 274-291.

Markowski, P. M., & Dotzek, N. (2011). A numerical study of the effects of orography on supercells. Atmospheric research, 100(4), 457-478.

Rotunno, R., & Klemp, J. B. (1985). On the rotation and propagation of simulated supercell thunderstorms. J. Atmos. Sci, 42(3), 271-292.

7. Your input sounding has a rather sharp low level inversion. Is it possible that this inversion is preventing UHI air from entering the storm updraft? Have you ensured (via trajectory analysis) that urban area is being ingested by the storm updraft?

The inversion is not present anymore when the storm approaches the city (see Fig. 1b). We are confident that the storm is ingesting the surface air mass because the most unstable parcel is surface-based, as it also shown in Fig. R2.

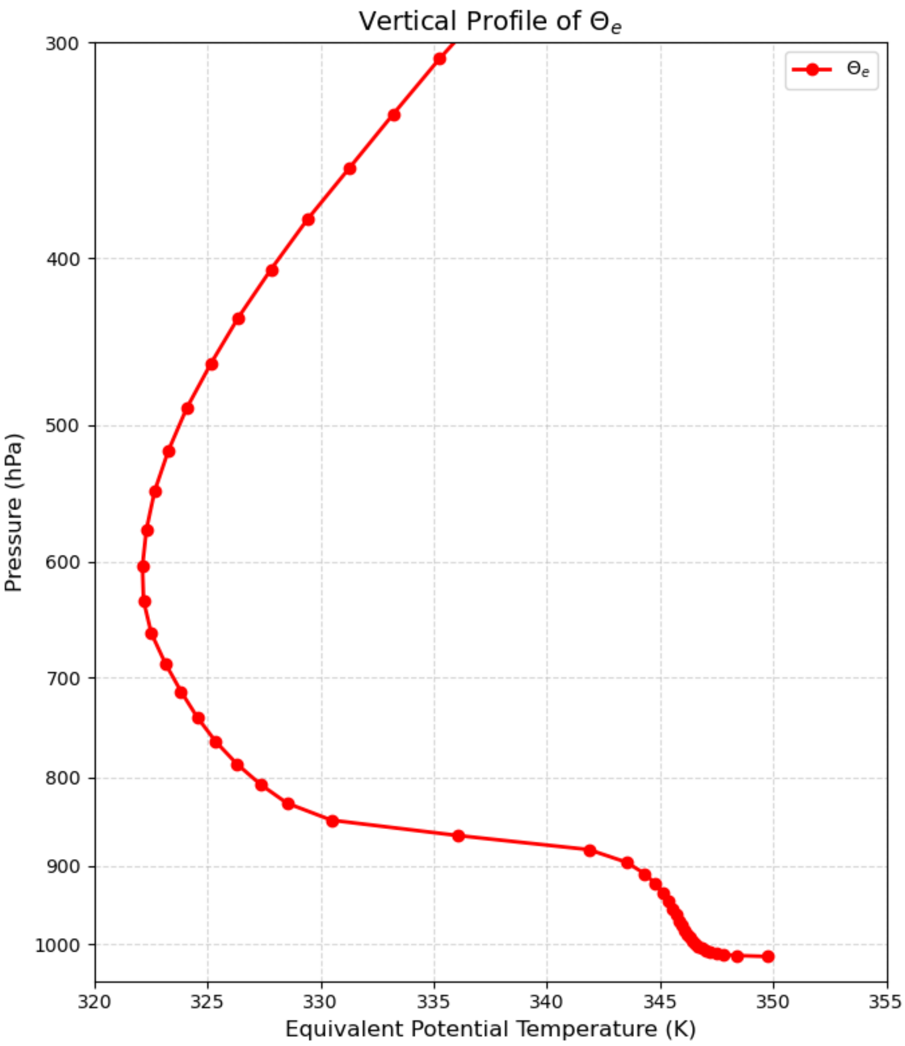

Fig. R2: vertical profile of equivalent potential temperature upstream the city in the control run of the 30km simulation.

8. What is the integration depth for your UH calculation? In addition, have you looked at 0-1 km SRH in addition to 0-3 km SRH? How are you calculating CAPE? Are you using a surface-based parcel or a mixed layer? Are you calculating CAPE with temperature or are you using virtual temperature?

The integration depth for UH is 2–5 km. CAPE is calculated using the most unstable parcel, which is surface-based during storm development. A virtual temperature correction is applied. The 0–1 km SRH field shows some differences compared to the 0–3 km SRH field; however, the main pattern remains consistent with that of the 0–3 km SRH (see Fig. R3, to be compared with Fig. 15a).

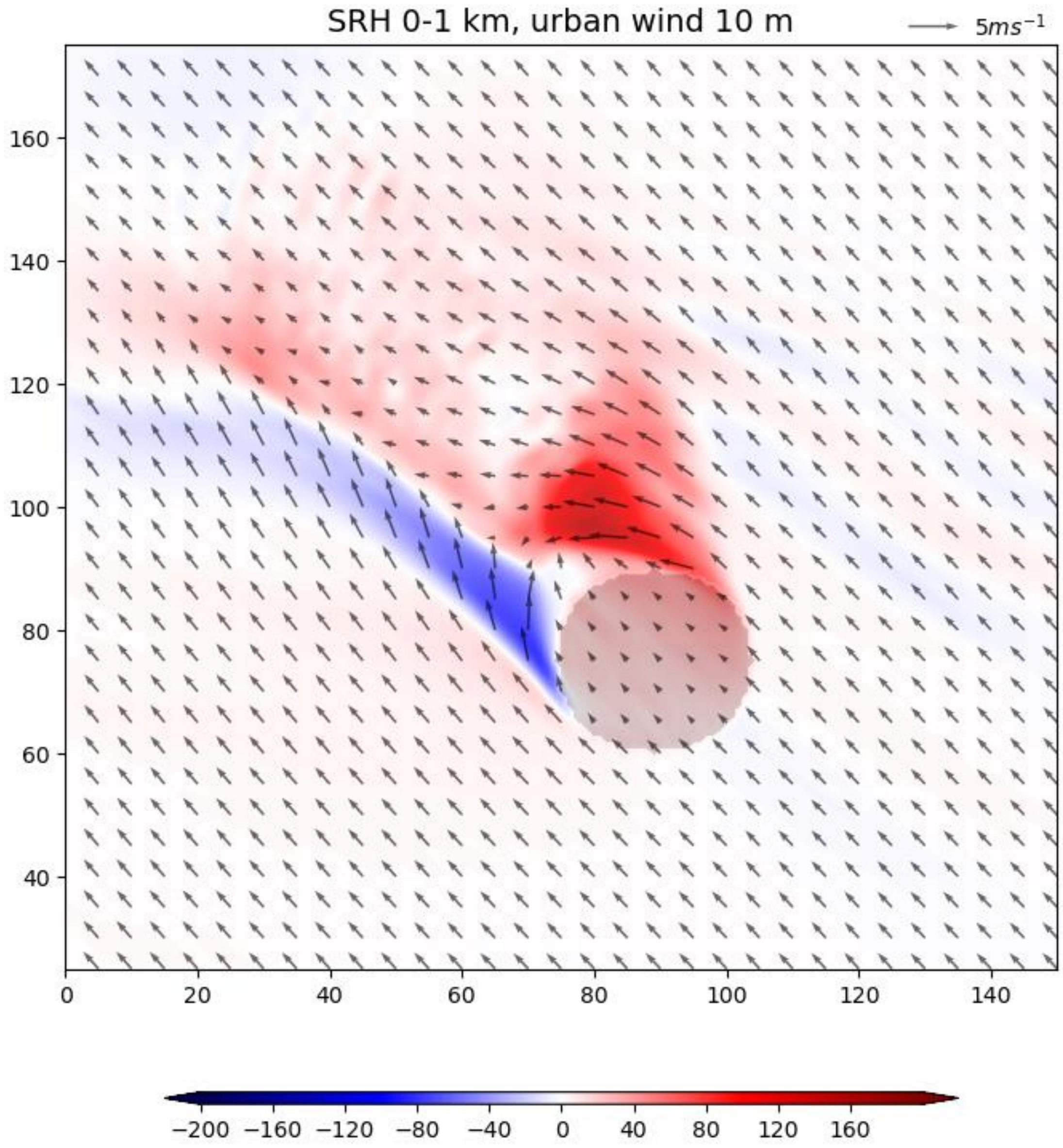

Fig. R3: ensemble mean difference of SRH 0-1 km (J/kg) between the 30km simulations and the no urban ones. The ensemble mean of the wind at 10 m in the 30 km simulations is also plotted.

9. Periodic boundary conditions are an interesting choice. Why not use open boundaries like other studies? The initial warm bubble creates a fast moving wave that propagates across the domain

   That is a good point. In WRF, the issue of mass loss associated with open boundary conditions is not resolved. Initially, we tested open boundary conditions, but we had concerns regarding mass loss. Therefore, we opted for periodic boundary conditions with a sufficiently large domain to avoid spurious wave interactions.

   We carefully examined the results and did not identify any concerning numerical issues within the chosen domain configuration.

References

Bryan, George H., John C. Wyngaard, and J. Michael Fritsch. “Resolution Requirements for the Simulation of Deep Moist Convection.” *Monthly Weather Review* 131, no. 10 (2003): 2394–416. https://doi.org/10.1175/1520-0493(2003)131%253C2394:RRFTSO%253E2.0.CO;2.

Huang, Xinjie, and Jiyun Song. “Urban Moisture and Dry Islands: Spatiotemporal Variation Patterns and Mechanisms of Urban Air Humidity Changes across the Globe.” *Environmental Research Letters* 18, no. 10 (2023): 103003. https://doi.org/10.1088/1748-9326/acf7d7.

Huff, F. A., and S. A. Changnon. “Precipitation Modification By Major Urban Areas.” *Bulletin of the American Meteorological Society* 54 (1973): 1220–32. https://doi.org/10.1175/1520-0477(1973)054%253C1220:PMBMUA%253E2.0.CO;2.