

My recommendation is again Reject. I do find this study interesting, but I have the same concerns that I've had from the start: The authors continue to misrepresent the findings of others, they seem to misinterpret or misrepresent my comments to them, and they have not adequately addressed the shortcomings of their study.

I still question the representativeness of the urban dry island (UDI) in the current simulations—an issue I have raised since my initial review. The authors have added additional citations they claim support their findings, but as I show below, this is not the case. In addition, the authors have not adequately addressed my comments/concerns about the hodograph.

I find it very concerning the authors continue to cite (in both the manuscript and response to me) previous studies they claim support their current findings when a relatively quick investigation reveals this is often not the case. I'm not sure if this is laziness, dishonesty, or outright fraud. Whatever the reason, these repeated instances have caused me to question the credibility of the entire study.

1. Misrepresentation of my comments:

In the authors' response to the Editor, they note that:

"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"

This is a misrepresentation. Please go back and review my actual comment. It is not that a reduction in CAPE was found. My concern is the **magnitude** of the CAPE reduction—the authors are claiming a decrease of over 1000 J/kg, which is about a 37% reduction from the input sounding.

From the Discussion section of the current manuscript, the authors have added several new citations they claim support their findings:

"Some previous studies found higher CAPE values in urban areas (Moraglia et al., 2024), while others noticed that urban land use can reduce CAPE (e.g., Yang et al. 2021b; Reames and Stensrud 2018; Yang et al. 2014), due to the reduction of moisture (Wang et al., 2012; Holley et al., 2014)."

While it is true that these studies do document a reduction in CAPE, none have noted a reduction that is comparable to the current study:

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

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

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

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.

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.

My concern is that the authors are claiming what is **by far** the largest reduction in CAPE ever documented by an urban area—both by overall magnitude and percent reduction—without offering any evidence that such a reduction is possible in the real atmosphere.

2. Another example of misrepresenting previous work.

The reduction in CAPE is clearly driven by the strong UDI in the simulations. So, if the reduction in CAPE is excessive, it is most likely due to excessive urban dryness. In section 4.1, the authors state:

“A pronounced UDI is also reproduced (peak of -2 gkg^{-1}), corresponding to a relative humidity difference of about 13% between the city and the surrounding rural areas, consistent with several other studies (Meili et al., 2022; Luan et al., 2022; Luo et al., 2021a; Lokoshchenko, 2017).”

I have reviewed each of the studies referenced in this statement and none of them support this claim:

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.

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

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

Luo et al. 2021a found higher urban specific humidity in their simulations, but observational analysis did show a UDI. However, from their Figure 4, the largest UDI occurred in the early morning hours. The difference in urban-rural specific humidity in the afternoon and evening hours (which is a comparable time to this current study) looks to be quite small.

Once again, I do not believe previous studies agree with the authors' current results as they claim. The urban dry island in this current study seems to be much stronger than has typically been documented in the literature. At best, the UDI magnitude they are seeing in their simulations is an extreme edge case and it should be acknowledged as such.

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.