Reponses to reviewer 1

This study investigates the effect of the city of Houston on a convective storm by means of regional numerical simulations. The authors use the WRF model coupled to a bin microphysics scheme and a land surface scheme. The city effect is decomposed into the land effect (the urban heat island effect) and the urban aerosol effect. The aerosol effect is further decomposed into the direct effect (aerosol-radiation interactions, ARI) and the indirect effect (aerosol-cloud interactions, ACI). It is shown that both effects (land and aerosol) drive increase in the convective intensity and precipitation, with the aerosol effect dominate. It is also shown that the interaction between the two effects causes a further intensification of the storm. The aerosol effect is shown to be dominated by the ACI rather than by the ARI. The two effects considered here (land and aerosol) affect the clouds at different stages and different levels in the atmosphere.

We thank the reviewer for your time and constructive comments. We have provided detailed responses point-by-point as below.

The mechanisms behind the different effects are explained. The paper address interesting questions and uses appropriate tools for answering them. I do have a few minor comments and suggestions for the authors:

L26: 15 m/s increase compared to what base-line? Maybe better to present it in percentage change.

The sentence was clear that the increase is the joint effect of both urban land and anthropogenic aerosols therefore it is compared with both effects excluded. The ~15m/s increase is in the maximum updraft speed. Now we have changed to the percentage increase (~75%) (Line 26).

L32: is 40 min change significant? Form the abstract the reader can’t evaluate it.

40 min is quite significant since for thunderstorms since the total duration of the case is only a few hours. We have added text about it at Line 494, “…accelerates the development of convective storm into deep cloud stage by ~ 40 min, which is significant for thunderstorms since the storm duration is only a few hours”.

L67: It will probably be good to explain what you mean here by: “indirect effect”

Rephrased as “…while aerosols had the opposite effect through the serving as cloud condensation nuclei”.

L147: what is the sizes of the domains? From Fig. 1 it looks like domain 2 is of the order of 200-250 km. Is it enough for spin-up of the clouds entering the domain from the lateral boundaries? As the boundary conditions are from MERRA-2 (and not from WRF simulations) I assume that the spin-up requires quite a long time after entering the domain. For a typical windspeed of ~25-30 km/h (Fig. 12, for height below 1 km, and probably even higher windspeed above), the air will spend in the domain about 8- 10 h. Is this enough for the spin-up? In other words, how can you eliminate the domain boundaries effect?

Two nested domains with horizontal grid spacings of 2 and 0.5 km and horizontal grid points of 450 × 350 and 500 × 400 for Domain 1 and Domain 2. The initialization time of Domain 2 is 12-hr ahead of the analysis period for this case, so it is enough for spin-up. The information has been added to the revised manuscript (Line 151 and 169).

L220: are you only integrating liquid and no ice here? As you are simulating deep convective clouds, wouldn't it make more sense to include ice?

Since the tracking algorithm from Hu et al. (2019) used the quantity of vertically integrated liquid (VIL), which is an estimate a vertically-integrated column of radar-retrieved precipitation. VIL was shown to be an effective indicator of strong precipitation cells (Greene and Clark, 1972, Hu et al., 2019). Thus, for the tracking of model results, we used LWP, which an equivalent quantity to VIL) for a fair comparison. We explained this in the previous paragraph by the sentence "To apply the algorithm to both model simulation and NEXRAD observations consistently in this study, we calculated liquid water path (LWP), a variable of model output accounting for the column integrated liquid to replace VIL in MCIT for model simulation". We have also added a sentence to better describe VIL for strong precipitation cells, that is "VIL was shown to be an effective indicator of strong precipitation cells (Greene and Clark, 1972, Hu et al., 2019a)" (Line 225-226).

L233: you have twice "first".

Deleted now.

L381: missing ")"

Added.

L405: what about the role of the slower fall speed of hydrometeors under polluted conditions? As it was shown before, smaller droplets, with lower effective terminal velocity, would be pushed higher into the atmosphere (even for a given vertical velocity) and hence invigorate the ice processes (in addition to the increased latent heating by condensation). Does that play a role here? In addition, in this section it might be a good opportunity to comment on a very relevant recent paper: https://journals.ametsoc.org/doi/pdf/10.1175/JAS-D-20-0012.1

Here we are discussing the enhanced updraft speeds by the anthropogenic aerosol effects. In the updraft cores, updraft speeds are much stronger than the hydrometeor fall speeds so how much droplets are pushed to the high-levels should be determined by updraft speeds. Thus, slower fall speed would not have a large effect. The slower hydrometeor fall speeds do affect stratiform and anvil properties as shown from Fan et al. 2013 (PNAS), Grabowski and Morrison 2016 and 2020 in JAS.

In our case since the increase of latent heating from condensation is much larger than that from the freezing, deposition, and riming (Fig. 15c-d). Therefore, the enhanced vertical velocity is mainly contributed by enhanced condensation. Both Fan et al. (2018) and Lebo (2018) showed that the same amount of latent heating increase at the low-level plays a more significant role in invigorating convection than at the high-levels.

We showed that the higher droplet concentration in the case with anthropogenic aerosols enhances condensation therefore thermal buoyancy since condensation rates depend on droplet number and size based on accurate solution of supersaturation. Grabowski and Morrison (2020) interpreted the convective invigoration at low-levels in a different way. We have added a paragraph to discuss this on Page 20, that is,

"Grabowski and Morrison (2020) interpreted this warm-phase convective invigoration at low-levels by aerosols in a different way. They argued supersaturation (S) in updrafts rapidly, within a few seconds, approaches the quasi-equilibrium supersaturation (S_{eq}). With this quasi-steady assumption ($S \approx S_{eq}$), the condensation rate and buoyancy only depend on updraft velocity, not droplet number and size. Thus they concluded that the lower quasi-equilibrium supersaturation in the polluted case than the pristine case is the reason for enhanced buoyancy and updraft velocity, not the enhanced condensation. The problem is that the quasi-steady approximation is invalidated for updrafts where droplet concentrations are low or droplets are growing and their sizes are changing based on the explicit solution of supersaturation (Korolev and Mazin 2003). The explicit theoretical solution of supersaturation showed that condensation depends on droplet number and size besides updraft speeds (Pinsky et al. 2013). Here in this study the quasi-equilibrium supersaturation in the updrafts is generally 2-3 times higher than the true supersaturation, and the phase relaxation time is generally above 10 s above 3-km altitude in the case without anthropogenic aerosols and about 60 s when droplet number is of 10 cm^{-3} which occurs frequently in the convective cores where autoconversion and rain accretion are strong."