Review of Branch et al (2023)
Scaling Artificial Heat Islands to Enhance Precipitation in Arid Regions

The authors present a study of the impact of artificial reductions in surface albedo as a means to enhance convective precipitation over the hyper-arid UAE in a convection-permitting modeling framework. These reductions in surface albedo are imposed as uniform changes to the land surface albedo over a prescribed area in the model, but in reality would be achieved through some combination of solar panels, vegetation plantations, and other artificial surfaces that are much darker than the surrounding desert. Fundamentally, these changes in albedo impact precipitation in the model through their enhancements of surface sensible heat flux, which in turn has a variety of meteorological consequences. The sensitivity of precipitation impacts to an increasing area of albedo modification is also a key consideration.

I found the study to be reasonably well constructed and executed. The modeling tools and analysis seem appropriate for the key questions of the work. It's an interesting and relevant topic of study, and I am curious how this work will continue or even be applied in the future.

Comments

I believe the paper could be improved through an expansion of the context provided first in the Background section, which the authors then return to in the Summary and Outlook. For example,

- Line 33: The authors reference a few papers presumably showing that albedo "can trigger regional scale impacts," but they don't provide any detail on what those previous papers were studying and whether they are relevant to this paper in particular. If you are just trying to generally say that albedo can alter regional climate and weather, I'd also look for a bit more detail here highlighting some of the ways that people have shown albedo changes altering regional climate and weather. But it would be especially good if they were directly relevant to this work and connected into the introduction more smoothly.
- Lines 40 - 45: Branch and Wulfmeyer (2019) seems very relevant to your work here, and I would like to see a bit more drawn from that paper into your introduction in terms of what they found, how your approaches are similar/different, how it might have motivated this study etc…

I also believe the paper could benefit from more detail on prior work looking at convection & precipitation and its diurnal cycle in the region more broadly, at least during the summertime months considered. How often does it occur? Does it go right to deep precipitating convection or is there shallow convection first? What's the variability? Are there any consistent patterns, and when might those patterns break down? I think this would feed well into your proposed future work directions at the end of the paper, expanding into other times of year or if there's climate variability. It would just be helpful to know more about what the background setting of convective precip is in the region you're looking at, in case the reader is less familiar. In other words, what exactly are we modifying through the albedo perturbation in the first place!

Similarly, I think the authors need to provide a bit more detail on how the model they are using does with convection and precipitation in this region. They note a validation study in Line 90 from a few years ago, but they don't indicate whether the results of that evaluation were favorable, especially for the variables they're pulling out of WRF in this analysis. Does the model reproduce CAPE over the UAE well compared to soundings, for example? Could you show satellite imagery, if radar isn't available, showing that your control simulation produced reasonable patterns convection on your four case days?

This is also important to discuss in your Model Configuration section if the only prior validation was done against surface observations. And along those lines – I would be careful in saying that this version of WRF is the same as the "validated" version when the "only change" was to use an updated version of the PBL, surface layer, and land schemes (Line 92) – the components of the model that are among the most important for this study. Just because a model component has been updated doesn't guarantee that it will improve the skill, especially over a particular region and when looking at something as sensitive as convective precip. Overall, I would just be more clear about what has been and what hasn't been validated for this particular region using this configuration of WRF. And it may be the case that some aspects (like my CAPE example above) haven't been tested exactly, but I would just mention that as a caveat and/or an area for future work.

Additionally, I think you have an opportunity to better connect your work to other areas of surface-atmosphere interactions in the Background (Lines 60 - 72) and the Outlook to round out the manuscript. Enhancements in sensible heating, usually associated with changes in vegetation cover or properties, have been shown to alter cloudiness/convection/precip in different ways depending on where you look. Your work highlights the importance of background humidity (here brought by the daily sea breeze) that can then be lofted to saturation by deeper, more vigorous boundary layers, which is something that also comes up over vegetated surfaces. I think highlighting that similarity and connection, especially in an environment with little-to-no latent heat flux would ground the study in prior/ongoing work more completely.

My final "general comment" deals with the temperature effect of the albedo perturbation introduced by the ABS. This study is mainly looking at precipitation effects, which makes sense given the region, but I would also look for some discussion of other implications of this strategy. If we darken the surface, we will also increase near-surface air temperatures. Will this be a problem locally for people, even if it is helping with some of their water scarcity issues? Maybe the temperature change isn't impactful relative to the background hot climate. If the area of the albedo perturbation gets large enough or there are too many of them, could it alter sea breeze or other regional dynamics in unhelpful (or maybe helpful) ways? If this is being considered in the context of a regional climate strategy those other effects need to be mentioned, at least. And again, that might be an area of future work for this manuscript, but I do think it needs to be mentioned.

Minor/Detailed Comments:

- Line 47: what kinds of weather modification have the previous studies shown for the PV panels? This ties back into my general comment above about more detail in the background, but just wanted to make a specific note here. Also, I don't believe you ever spelled out PV as photovoltaic, which would be good for the first use.
- Line 57: somewhat tangential, but are those PV panel efficiencies valid for an environment like the UAE with such high temperatures?
- Line 70: found "interactions" between urban heat islands and sea breezes, particularly for convection – what are those interactions? Are they relevant for your results here?
- Line 83: The term Artificial Heat Island is not used in the introduction. Given how it's used to frame the study in the paper title, I would look for it to show up here somewhere. Perhaps in the connection from Urban Heat Island.
- Line 105: Can you clarify that this data assimilation of soil moisture and temperature is happening in the ECMWF system you use for your boundary conditions? Or is this going into WRF directly?
- Line 136: Could you provide more detail on how the case days were selected within the year and if the days themselves are representative of diurnal patterns in the study area (I see that you noted a validation that summer 2015 was climatologically representative but not the days themselves)?
- Line 154: When you say conditions in the east vary more with the Gulf of Oman, can you be more specific?
- Line 177: Is the model overly drizzly?
- Caption for Figure 4 references a 150 km diameter, but in the text it is 90 km.
- Lines 230 - 240: How confident are you in the ability to operationally predict where precip enhancements needed to be captured for human use? Is the enhancement based on the ABS here falling in the right spot that it *could* be collected or directed to groundwater recharge? Would be important to note if this is being used to justify any sort of deployment/construction.
- Line 245: I'm curious why you didn't pick the case where the rainfall impact was strongest?
- Line 258: "heat flu" is missing the x for "flux"
- The caption on Figure 7 was a bit confusing in terms of the left panel having "both 50 km ABSs", when you're just noting that they have a common footprint
- Line 283: Why were these other factors disregarded?
- Line 324: "diurnal timing" of what?
- Line 352: CI referring to convective initiation or impacts?
- Line 426: Just to clarify, is the HCF index what you are using as your LA feedback metric?
- Line 453: *Are* the water quantities produced by the model microphysics plausible? This could tie back into my question about more detail about model validation/future work and the context of convective precip over the region.