Point by point reply to Reviewer 2

Review for "Scaling Artificial Heat Islands to Enhance Precipitation in Arid Regions"

Opening Reviewer comment.
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.

We would like to thank the reviewer for taking the time and effort to review this submission, and for the positive appraisal which is much appreciated. We would like to address your comments below, point by point:

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.**

The authors agree that perhaps a bit more background could be useful here regarding albedo change. These papers (Doughty et al., 2011; Kala et al., 2022; Ridgwell et al., 2009) are thought to be relevant here because they relate to *deliberate* albedo change. However, these all relate to an increase in albedo, not a local decrease as we are proposing here. Of course, increasing albedo on a wider scale is also a contribution to a reduction in overall global forcing (however small..) as well as a potential means of modifying regional climate. Papers which advocate deliberate albedo *reduction* are scarce, aside from four of our own studies which all relate to this topic and together provided the basis for this work (see also the next point).

To address your important point, we have modified some text to emphasize that the three studies (Doughty et al., 2011; Kala et al., 2022; Ridgwell et al., 2009) relate to deliberate albedo increase, or 'brightening', and to clarify the methods used (L31-35).

From:

"Examples are marine cloud seeding to reduce coral bleaching (Latham et al., 2014; Latham et al., 2013; Tollefson, 2021) and albedo management through landscape planning, and breeding of higher-albedo crops (Doughty et al., 2011; Kala et al., 2022; Ridgwell et al., 2009). Albedo change can trigger regional scale impacts (Quaas et al., 2016; Seneviratne et al., 2018) such as reduction of temperatures (Kala & Hirsch, 2020), but at the same time could also contribute toward reduced global forcing (Carrer et al., 2018; Sieber et al., 2022)."

To:

"Examples are marine cloud seeding to reduce coral bleaching (Latham et al., 2014; Latham et al., 2013; Tollefson, 2021) and ***deliberate*** *albedo management through agricultural landscape planning, and breeding of higher-albedo crops (Doughty et al., 2011; Kala et al., 2022; Ridgwell et al., 2009). Their general aim is to investigate the potential for regional cooling of temperatures.* ***The deliberate increase of albedo falls under the***

geoengineering category of terrestrial solar radiation management (SRM). And although geoengineering is considered to be global in scale, regional actions may trigger regional impacts *(Quaas et al., 2016; Seneviratne et al., 2018) such as reduction of temperatures (Kala & Hirsch, 2020), whilst at the same time contributing toward reduced global forcing (Carrer et al., 2018; Sieber et al., 2022).*

● 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…

Absolutely. Our previous works on deliberate albedo change are directly related to this work (Branch and Wulfmeyer, 2019; Wulfmeyer et al., 2014; Branch et al., 2014; Becker et al., 2013). These works all have a different emphasis though and relate to deliberate local reduction in albedo through cultivation of desert *vegetation*. Our work differs in that here we focus on artificial non-vegetation surfaces, and also that we examine the effects of scale and associated lower boundaries for impacts, which the other works did not.

To address your comments and clarify these links between our previous and present studies, we have re-emphasized that these works were carried out by our group, which is important to set our latest work in context and build on the findings and new process understanding. Therefore, we have modified the text at L39-40:

From:

"Several studies show that desert xerophyte plantations can enhance rainfall via canopy heating (Becker et al., 2013; Branch et al., 2014; Branch & Wulfmeyer, 2019; Wulfmeyer et al., 2014)."

To:

"Several of our previous studies show that desert xerophyte plantations can enhance rainfall via canopy heating (Becker et al., 2013; Branch et al., 2014; Branch & Wulfmeyer, 2019; Wulfmeyer et al., 2014)."

To link our most relevant publication (Branch & Wulfmeyer, 2019, PNAS) to this one we have also modified the text at the end of the introduction (L82-84).

From:

"In the Results and Discussion section, we present impacts on precipitation, convective processes, and feedbacks. Finally, in the Summary and Outlook section we put results in context and discuss wider implications."

To:

"In the Results and Discussion section, we present impacts on precipitation, convective processes, and how we applied a thermodynamic feedback index to investigate predictive potential (similarly to Branch & Wulfmeyer, 2019). Finally, in the Summary and Outlook section we put results in context and discuss wider implications."

The authors hope this addresses your comments without increasing the length of the paper too much.

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!

This is a very good idea. We did provide some background already on the climate and precipitation patterns in the region, and have cited our previous work on convection initiation (CI) patterns in the area (Branch et al, 2020) on identification of CI events from Meteosat radiances. Perhaps we did not reference this work strongly enough. This paper shows that deep convection only occurs with any regularity in the eastern Al Hajar Mountains, and areas west of these mountains are in general extremely arid, dominated by regional subsidence. Given that our ABS patches were simulated on these plains we have emphasized this more within the text and suggested to the reader to investigate our work on convection patterns in the region (after L131-132). We have also added a line to state the main climatic 'constraint' to be overcome by the land surface modification method:

Lines modified from:

"Warm sea breezes reach the coast around midday (local time (LT), Eager et al., 2008), and reach the ABS areas typically around 14:00 LT."

To:

"Warm sea breezes reach the coast around midday (local time (LT), Eager et al., 2008), and reach the ABS areas typically around 14:00 LT. Nevertheless, deep convection only occurs regularly in the eastern Al Hajar Mountains. The more westerly desert plains are in general extremely arid, dominated by regional subsidence and capping inversions (See Branch et al. (2020) for regional convection initiation statistics). These inversions must be overcome by any deliberate albedo modification"

We trust that this addition provides enough detail and has improved the context.

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.

The authors agree that verification is very important. Our previous validation of the WRF model (Branch et al. 2021) was based on a computationally-ambitious one-year model simulation, evaluated with the Model Evaluation Tools (MET) package against data from 48 surface stations in the UAE. We are convinced that this provided good context as to the skill of WRF regarding simulation of regional surface conditions. We will include some details on the evaluation results here as you suggest, particularly as daytime temperatures and dewpoint were satisfactorily reproduced during daytime, for the most part. It is for this reason that we used the same resolution, configuration, and forcing/sea surface temperature data for this study (although an updated WRF version). The only persistent deficiency in the model was an overestimation of daytime surface winds, which was observed also in some other papers (Fonseca et al., 2020; Temimi et al. 2020) [In an oversight, this last Temimi 2020 paper was not cited and so we have added this to the manuscript (L88)].

The successful simulation of convective precipitation is always a challenge for numerical models. The expectation that case-study downscaling simulations can reproduce actual cloud and precipitation patterns at any point in time is likely to be unreasonable *without* a significant data assimilation effort, which is beyond the scope of this impact study. For these reasons, we do not evaluate these phenomena for our cases, but instead rely on findings from other regional studies. However, we have now expanded the text in the Summary and Outlook to further emphasize the need for more testing (L458):

"Ensemble UAE simulations with varying model physics including microphysics (e.g., Schwitalla et al., 2020; Fonseca et al., 2020, Taraphdar et al., 2021) would be particularly useful. Here, data assimilation, quantitative precipitation estimation, and rain radar and satellite analyses may be employed (Bauer et al., 2015; Kawabata et al., 2018; Branch et al., 2020; Schwitalla & Wulfmeyer, 2014). ***The latter analyses, probably based on seasonal simulations, will be especially important to test further the plausibility of convective rainfall amounts, from a statistical point of view.*** *For now, we have at least some confidence that WRF V4 has reproduced convective cells in a satisfactory way in the region (Fonseca et al., 2022."*

As well as the studies that we have already cited around L88-89, we have now added (in Modelling and Methods and in the Summary and Outlook) a more recent study by our UAE colleagues who recently assessed the usefulness of WRF V4 in predicting cloud and convective patterns, with found satisfactory results (Fonseca et al. 2022). We trust that this adds weight to confidence in the model performance in simulating convective process (especially with WRF V4 which we used here).

To address your comments, we added more detail to the verification results and also added/modified text (L90-92) from:

"Here, we use the same domain, resolution and configuration to Branch et al., 2021, who evaluated the skill of WRF (V3.8.1) in reproducing 2-m temperature and humidity when compared to 48 surface weather stations."

To:

"Here, we use the same domain, resolution and configuration to Branch et al., 2021, who evaluated the reproduction of 2-m temperature and humidity by WRF (V3.8.1) in when compared to 48 surface weather stations. The results over four seasons were satisfactory, providing a good basis for using this model at the same scale. Here we used an updated version of WRF (V4.2.1), and make the assumption that the model performance has not deteriorated with the model updates. Indeed, Fonseca et al. (2022) assessed the ability of WRF V4 to reproduce cloud and convective cell spatial distributions, for the purpose of cloud seeding operations, and with satisfactory results. This adds some confidence in the reproduction of convective process in the UAE by WRF V4, as used within this study."

We have also exchanged the word 'improvements' at L93 to 'updates' to avoid the impression of unwarranted claims about performance.

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.

This is a good suggestion. We are very conscious about keeping the manuscript length relatively concise, and not including too much general background on land atmosphere interactions over multiple scales. We elected to maintain the focus only on this particular effect, i.e., a relatively localized isolated heat perturbation, especially in arid environments. Hence, we elected to include only the most relevant papers, relating to this effect and at similar meso-gamma to meso-beta scales e.g. urban heat islands. To address your comments though, and to introduce a link to moisture requirements in arid regions (with low latent heat fluxes), we have modified the following sentence (L39-40) from:

"Several studies show that desert xerophyte plantations can enhance rainfall via canopy heating (Becker et al., 2013; Branch et al., 2014; Branch & Wulfmeyer, 2019; Wulfmeyer et al., 2014). Branch et al., 2014, measured albedos of 0.17 and 0.12 for jatropha and jojoba plants, and the surrounding desert ~0.3, leading to temperatures

up to 4°C higher than the surrounding desert (see also Saaroni et al., 2004). This heating led to greater simulated cloud development and ls (CI) (Branch & Wulfmeyer, 2019)."

To:

"Several of our previous studies show that desert xerophyte plantations can enhance rainfall via canopy heating (Becker et al., 2013; Branch et al., 2014; Branch & Wulfmeyer, 2019; Wulfmeyer et al., 2014), facilitated by the advection of coastal marine moisture. Branch et al., 2014, measured albedos of 0.17 and 0.12 for jatropha and jojoba plants, and the surrounding desert ~0.3, leading to temperatures up to 4°C higher than the surrounding desert (see also Saaroni et al., 2004). A subsequent model simulation with a similar magnitude of heating showed increased cloud development and convection initiation (CI) (Branch & Wulfmeyer, 2019). "

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.

Both Reviewers raised this very important point, and from the beginning we did consider the potential impacts on regional temperatures. We suggest adding a new figure, if space allows, showing the mean impact on simulated daily maximum and mean 2-m temperatures:

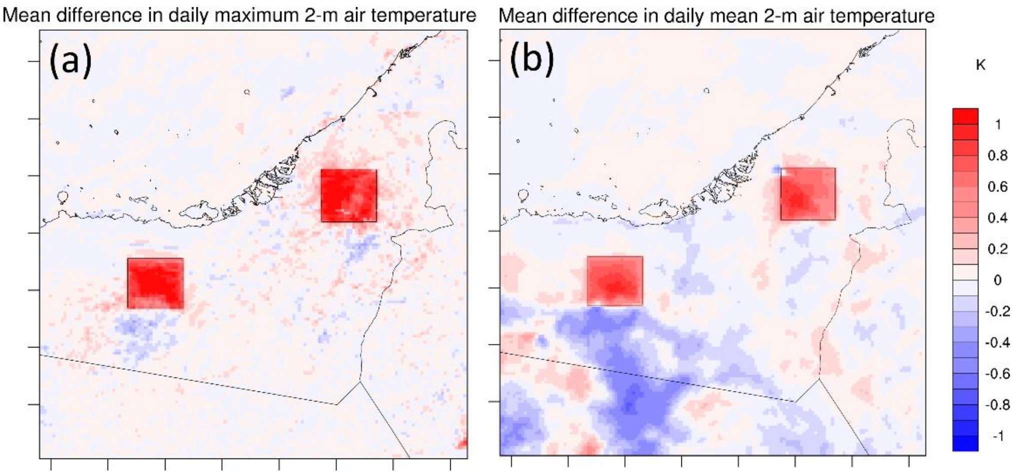

Figure X: The case-average impact on daily mean and daily maximum 2-m air temperatures from the 50km ABS. Computed respectively, as $\frac{1}{n}\sum_1^n(\overline{T2m}\ ABS_{50km} - \overline{T2m}\ Control)$ *and* $\frac{1}{n}\sum_1^n(T2m_{max}\ ABS_{50km} - T2m_{max}\ Control)$*, where n is the number of cases. Panel (a) is the daily maximum 2-m temperature impact, and Panel (b) 24-hour daily mean impact.*

To supplement the figure, we would add more text (at L241) to include information on how surface heating is distributed both inside and around the ABS zones. We have added the following text:

"It is also important to consider the impacts on temperatures outside as well as inside the ABS zones, because large temperature changes could affect local citizens and vegetation. Figure X shows the difference (case-average) in daily maximum and mean temperatures between the 50 km ABS scenario and the Control. For maximum daytime temperatures (panel a), there is a maximum temperature increase of up to ~1 °K, inside the 50km ABS zones when compared to Control. In the surrounding areas, there is a temperature increase particularly around the eastern zone, but the differences are relatively limited, both in spatial extent and the temperature increase (~0.2-0.3 °K). Curiously, there are also some minor cooling effects to the south of the ABS. For daily mean temperatures (panel b), there is an increase of up to ~0.8 °K inside the ABS zones. Outside the ABS, the largest increases are ~0.2-0.3 °K, but these areas are quite close to the ABS zones. These simulated values indicate that there is a slight temperature impact on the near-surroundings, but even at the largest ABS 50 km scale, this is simulated as low to moderate."

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.

The authors agree. We have added an additional line in the Summary and Outlook section to state that there are wider uncertainties on climate impacts which may need to be addressed in future works (L432):

"It is still uncertain though if impacts on circulation or the sea breeze are modified, or even tele-connected to other regions. This is subject requiring further investigation, most likely with longer simulations to provide statistics."

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.

Studies on deliberate use of solar photovoltaic for weather modification are scarce. We have cited one study (Mostamandi et al., 2022) who state that simulated solar panels over the Saudi Arabian coast increases surface air temperature by 1–2 K, strengthens land-sea temperature contrasts, intensifies breezes, increases water vapor mixing ratio in the boundary layer, and increases rainfall. However, partly based on other Reviewer comments, the authors have elected to reduce emphasis on the use and description of solar panels somewhat, and we instead focus on black panels for the ABS systems. The use of solar is still given as a potential surface given the proliferation of solar farms in the region but we have reduced the references to solar PV in the Background section. One good reason for this is that, in our opinion, a good representation/simulation of solar surfaces (with varying cell materials), requires detailed solar farm measurements – which may form part of a future study.

Also, I don't believe you ever spelled out PV as photovoltaic, which would be good for the first use.

Thank you. This has now been amended.

● Line 57: somewhat tangential, but are those PV panel efficiencies valid for an environment like the UAE with such high temperatures?

This is a good question, but we had anyway considered possible efficiencies as low as 10% in the text, which we think is reasonable. Nevertheless, as we mentioned above, we have reduced our emphasis on solar PV.

● 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?

This is a very good idea. We have added here some text on the interaction between sea breeze and urban heat islands (L72):

"Some studies include the interactions between UHIs and sea breezes (e.g., Cenedese & Monti, 2003; Freitas et al., 2007; Zhang & Wang, 2021), and found interactions between them, particularly for convection. For instance, Freitas et al. (2007) found that an urban heat island (UHI) formed convergence zones in a city, accelerating the sea-breeze front toward the city centre."

● 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.

An excellent idea. We have now modified the sentence at L72 to:

"The relevance of this interaction will become apparent later in this study on 'artificial heat islands'."

● 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?

This was a question also raised by another reviewer and we have clarified this at L107-108:

"The ECMWF forcing data is only used to provide the lateral boundary and initial conditions for WRF-Noah-MP which then itself computes the evolving conditions within the model domain. The only exception to this, is the ingestion of OSTIA sea surface temperature (SST) data (Δx 1/20°, Donlon et al., 2012), which are re-updated within the domain every 12 hours (00:00 and 12:00 UTC). This is likely to be beneficial for simulating the sea breeze."

● 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)?

In fact, we are looking for a representative season, and year, but we are not looking for 'representative' days for our cases, but days on which convection is likely with some heating perturbation, e.g., moderate to high CAPE and moderate CIN. We outlined our criteria here (L133-134):

"Our aim was to observe strong impacts, so we selected a season where impacts are most likely (summer) and days with unstable convective conditions during a suitable season.We elected to do the simulations in JJA 2015, to coincide with the evaluation simulations of Branch et al. (2021), who found this season was representative in terms of the long-term climate. Summertime was also selected because strong impacts were observed during this season (e.g. Branch and Wulfmeyer, 2019)."

Also, in L167-168, we have also stated:

"In summary, all cases are moderately unstable and exhibit a wave of reduced CIN passing over the ABS. CIN may be a defining constraint, for even when CAPE is only moderate (e.g., 27 July), a low CIN may still permit CI."

The authors trust that this provides enough justification on our year/season/case selection.

● Line 154: When you say conditions in the east vary more with the Gulf of Oman, can you be more specific?

Agreed. To some extent we did specify this in the second sentence here:

"In the east, conditions vary more with influence from the Gulf of Oman. Sometimes wind confluence occurs (e.g., 27 July, Figure 3, panel d), or one side dominates the other, or more southerly winds prevail."

But perhaps the link between the two sentences is not clear. Hence, we have modifed this now to:

"In the east, conditions vary more with influence from the Gulf of Oman, with wind confluence from these two Gulfs occurring (e.g., 27 July, Figure 3, panel d), or sometimes the winds from one direction dominate the other, or at other times more southerly winds prevail."

● Line 177: Is the model overly drizzly?

The model is not particularly drizzly in this simulation. Nevertheless, we have maintained the lower (Frey) threshold of 1mm to remain consistent with current rainfall modelling studies.

● Caption for Figure 4 references a 150 km diameter, but in the text it is 90 km.

Thank you. This was a caption error and has now been changed to 180 km diameter.

● 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.

This is a very interesting point. The practical implementation of water collection is not part of this study, and we simply assume that all extra precipitation is a 'benefit' whether it is collected or put in the groundwater. A more crosscutting his would certainly be an interesting investigation for later publications.

● Line 245: I'm curious why you didn't pick the case where the rainfall impact was strongest?

We elected to choose not the most extreme case, but instead a moderate one from our four cases. We feel it illustrates the convective processes very well.

● Line 258: "heat flu" is missing the x for "flux"

Thank you. Corrected.

● 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

We agree that this could be expressed more clearly. Hence, we have re-written the caption as:

"Figure 1: Mean sensible heat flux and standard deviation on 27 July. The left panel shows the spatial mean within a common footprint (the 50 km ABS area) for all scenarios The right panel shows the spatial mean of each individual ABS footprint. Here, two spatial means are shown for Control - the 10km and 50 km footprints, so they can be compared."

We trust that this provides more clarity for the reader.

● Line 283: Why were these other factors disregarded?

This was clarified in the following sentence but could be clearer. Hence, to connect this reasoning, we have modified this sentence from (L283-285):

"Differences in static land surface characteristics, such as soil texture or moisture, were considered as possible reasons for these patterns but these were disregarded. The soil moisture is virtually zero and the soil texture is very homogenous over the whole area."

to:

"Differences in static land surface characteristics, such as soil texture or moisture, were considered as possible reasons for these patterns but these were disregarded, because the soil moisture is virtually zero and the soil texture is very homogenous over the whole area."

● Line 324: "diurnal timing" of what?

Agreed this needs clarification. We have modified the sentence from:

"Interestingly in the 50 km scenario, low-level convergence and PBL development are impacted earlier in the day, indicating that diurnal timing may also be important for CI."

To:

"Interestingly in the 50 km scenario, low-level convergence and PBL development are impacted earlier in the day, indicating that the varying diurnal onset of strong convergence between the scales may also be important for triggering CI."

● Line 352: CI referring to convective initiation or impacts?

This refers to convection initiation here. We have added the words "convection initiation is influenced" here (L353-354):

Our goal is to gain insights into how scale influences convective initiation, and it is known that convection initiation is influenced not only by large-scale conditions, but also by land-atmosphere (LA) interactions, or feedbacks (e.g. Jach et al., 2020, Gerken et al., 2019).

● Line 426: Just to clarify, is the HCF index what you are using as your LA feedback metric?

Yes this is correct. To make that clear we have modified the sentence (L427):

"and to explore the predictability of impacts by applying the LA feedback metric, the HCF index."

To:

"and to explore the predictability of impacts by applying the LA feedback metric (the HCF index)."

● 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 reg

This is of course an excellent question, and we have written in the text that we are assuming that precipitation amounts are *reasonable* at L454:

"In terms of rainfall enhancement, **if we assume** that the water quantities produced by the model microphysics are plausible, then the implications are considerable."

We will make this caveat a bit stronger.

"In terms of rainfall enhancement, **we are assuming** that the water quantities produced by the model microphysics are plausible. If they are representative, then the implications for these amounts are considerable."

We trust this makes the case more clearly. We also discussed the need to evaluate rainfall quantities in the future in the Summary and Outlook:

"Confidence in rainfall enhancement should be further tested in further studies which assess simulation sensitivity, regional climate variability, and statistical analyses. Ensemble UAE simulations with varying model

physics including microphysics (e.g., Schwitalla et al., 2020; Fonseca et al., 2020, Taraphdar et al., 2021) would be particularly useful. Here, data assimilation, quantitative precipitation estimation, and rain radar analyses may also be employed (Bauer et al., 2015; Kawabata et al., 2018; Branch et al., 2020; Schwitalla & Wulfmeyer, 2014).”

However, we will modify this a little to make it clear that work still needs to be done, including precipitation quantification evaluation in subsequent research:

“Confidence in rainfall enhancement ***and rainfall quantification*** *should be tested in further studies which assess simulation sensitivity, regional climate variability, and statistical analyses. Ensemble UAE simulations with varying model physics including microphysics (e.g., Schwitalla et al., 2020; Fonseca et al., 2020, Taraphdar et al., 2021) would be particularly useful. Here, data assimilation, quantitative precipitation estimation, and rain radar and satellite analyses may be employed (Bauer et al., 2015; Kawabata et al., 2018; Branch et al., 2020; Schwitalla & Wulfmeyer, 2014). The latter analyses, probably based on seasonal simulations, will be especially important to test further the plausibility of convective rainfall amounts, from a statistical point of view. For now, we have at least some confidence that WRF V4 has reproduced convective cells in a satisfactory way in the region (Fonseca et al. (2022).”*

Aside from the absolute representativeness of precipitation amounts though, the authors also consider that the relative rainfall enhancements between the different scenarios, and the clear intensification of convective processes, even from 20km scales upward, already represents a positive indicator for enhancement potential. In respect to convective processes we consider that the likely increases in updrafts, cloud development and convection initiation have been demonstrated convincingly here. In that respect, the convection evaluation paper of Fonseca et al. (2022) provides confidence in WRF’s ability to produce convective cells, and in the right locations.

Concluding remarks from the authors:

Many thanks again for taking so much time to review our work and for your well-thought out and constructive comments. We feel that your comments have greatly improved our manuscript.

New references

Fonseca, R., Francis, D., Nelli, N., Farrah, S., Wehbe, Y., Al Hosari, T., & Al Mazroui, A. (2022). Assessment of the WRF Model as a Guidance Tool Into Cloud Seeding Operations in the United Arab Emirates. *Earth and Space Science*, *9*(5). https://doi.org/10.1029/2022EA002269

Temimi, M., Fonseca, R., Nelli, N., Weston, M., Thota, M., Valappil, V., Branch, O., Wizemann, H. D., Kondapalli, N. K., Wehbe, Y., Hosary, T. Al, Shalaby, A., Shamsi, N. Al, & Naqbi, H. Al. (2020). Assessing the impact of changes in land surface conditions on WRF predictions in arid regions. *Journal of Hydrometeorology*, *21*(12), 2829–2853. https://doi.org/10.1175/JHM-D-20-0083.1