Point by point reply to Reviewer 1

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

Opening Reviewer comment.

In their manuscript "Scaling Artificial Heat Islands to Enhance Precipitation in Arid Regions", the authors present analysis of several numerical simulations exploring the impact of decreased land surface albedo in squares of various sizes over two desert regions of the United Arab Emirates. They find that, when the imposed squares of low albedo land are larger than 20 km per side, there is a statistically significant increase in precipitation in the surrounding region. The simulations are conducted using the Weather Research and Forecasting model, with boundary conditions forced for four distinct weather events in the region. The methodology is appropriate for the aims of the study, and the topic is appropriate for GMD. I have a few addressable (but still important) concerns, outlined below, as well as several minor comments. Following revision, this manuscript would be a valuable addition to the literature.

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

Major comments:

Line 225: "*It is not linear, but more exponential in appearance – which is reasonable, given the areal size increments.*" --- I wouldn't make this claim unless you back it up with analysis, and you'd want to do the analysis with area of imposed ABS, not width of imposed ABS square (e.g. the 20km box is 4x larger than the 10km box); IS the relationship non-linear with area of forcing? That isn't at all obvious to me.

This is a good point and the authors agree that one has to be careful. The intention here was to make a qualitative observation to describe the non-linear bar spacing only (Fig 6c), not necessarily to quantify a definitive area/precipitation relationship in terms of processes. We understand that and it is difficult to assess whether the non-linear rainfall amounts relates directly with the areal increase, or could be due to other factors too e.g. a non-linear response to the downwind path length (fetch) of the ABS. To clarify our intent, add some new ideas, and to avoid making unwarranted claims we have added a caveat, modifying the text (L226-227) from:

"The mean values indicate a clearer relationship between scale and surpluses. It is not linear, but more exponential in appearance – which is reasonable, given the areal size increments."

to:

"After averaging the amounts over the four cases, the precipitation amounts (and increases over Control) suggest a non-linear relationship between precipitation and ABS side length, i.e., differences between bars increase with successive increase in scale. Intuitively, this might be expected, given the exponential areal increase in our ABS scenarios, but other factors could also be influential in addition to the surface area e.g. the ABS path length in respect to prevailing wind direction."

Line 240: It seems necessary at this point in the analysis to discuss the *temperature* and *heat stress* impacts of the ABS - are these locally confined to the region of the ABS or do they extend regionally? (I wondered if perhaps this would come up later in the paper, but (a) it didn't and (b) this is where I felt like I wanted to see it addressed.)

This is an excellent suggestion, and we have considered this very important regional impact. We had elected to avoid making the manuscript unnecessarily long. However, in view of your comment we suggest to add a few lines of text to clarify how the simulated surface temperatures may change outside of the ABS zones – based on our four cases. We suggest to supplement this with a figure below if the editor agrees to include it. We suggest to add the following text (at L241):

"It is also important to consider the effect of surface heating on temperatures outside the ABS zones, as large increases in temperature could affect local citizens and vegetation. Figure X shows the difference (mean of the

four cases) in daily maximum and mean temperatures for the 50 km ABS scenario. Panel Xa indicates the mean maximum temperature difference during the daytime. Within the ABS zones the largest values are around one-degree Kelvin higher than in the Control. There is a temperature increase in the surrounding areas particularly around the eastern zone, but the differences are relatively limited, both in spatial extent and the increase (~0.2-0.3 K). Curiously, there are also some minor cooling effects to the south of the ABS. There is also a small mean daily temperature increase outside the ABS (panel b, ~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."

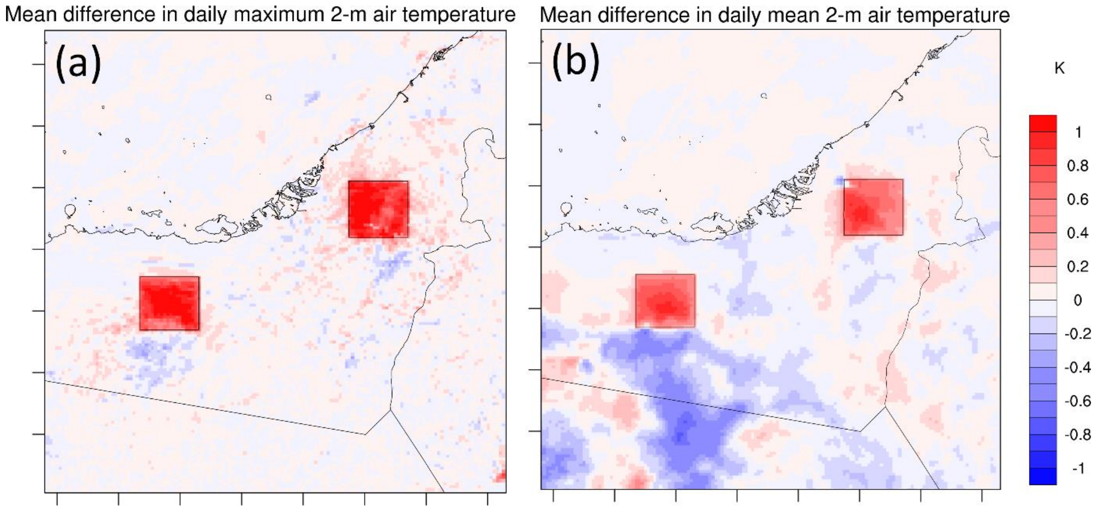


Figure X: The case-average impact on daily mean and daily maximum 2-m air temperatures from the 50km ABS. Computed respectively, as $\frac{1}{n}\sum_1^n(\overline{T2m}\ ABS_{50km} - \overline{T2m}\ Control)$ and $\frac{1}{n}\sum_1^n(T2m_{max}\ ABS_{50km} - T2m_{max}\ Control)$, where n is the number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

The authors hope that this figure and discussion of the temperature impacts will provide a more comprehensive picture for the reader, and perhaps provide some measure of reassurance about regional temperature impacts.

Line 250: "*As a result, net-shortwave radiation absorbed at the surface is increased by 25% of the incident solar radiation, and this extra energy is partitioned into turbulent and ground heat fluxes.*" ... presumably also into heating the surface which leads to increased LW up. Maybe this is what you mean by "ground heat flux", but typically we think of "ground heat flux" as energy stored by the ground at each timestep, ie that doesn't have to be removed as LW, SH, or LH. But heating up the surface leads to higher LW out , which isn't a turbulent flux.

The authors agree that this could be expressed more clearly and so we have modified the text (L250) from:

"As a result, net-shortwave radiation absorbed at the surface is increased by 25% of the incident solar radiation, and this extra energy is partitioned into turbulent and ground heat fluxes."

to:

"As a result, an increase in net-shortwave radiation occurs (~25%), and an increase in the net-radiation which is partitioned into sensible and ground heat and latent (negligible) fluxes."

The authors assume that by introducing the net-radiation here, that changes in all longwave radiation fluxes are inherently accounted for when discussing the total energy balance.

Minor comments:

Line 16: what do you mean by "one-day cases over a 24 hour period"?

The authors agree this could be expressed more clearly, so we have modified this line from:

"Simulations of five square ABS of 10, 20, 30, 40, and 50 km sizes were made on four one-day cases over a 24-hour period."

To:

"Simulations of five square ABS of 10, 20, 30, 40, and 50 km sizes were made on four one-day cases, each for a period of 24-hours."

The authors trust that this should be clearer to the reader now.

Line 26: how much direct warming comes from the albedo change?

This may be answered only partially within the results of this study, because we have modified not only the albedo, but also the surface height/roughness properties (shown in Table 2). Even if we had not modified the albedo, the latter change would also influence the feedbacks which shape sensible heating rates, 2-m and skin temperatures, and so on. As the prescribed ABS height (50cm) and roughness length parameter (5cm), do not deviate drastically from the bare desert soil parameters though, the effect of roughness is likely to be relatively small compared to that arising from albedo change. In that case, then new figure proposed above (on 2m temperature impacts) provides a 'reasonable' estimate of the albedo impact on 2m temperature over the ABS zones. Around ~1 °K mean increase in maximum daily temperature, and up to ~0.8 °K mean daily temperature (averaged over the four cases).

These temperature impacts are now described in L241-245 thus:

"It is also important to consider the effect of surface heating on temperatures outside as well as inside the ABS zones, because large increases in temperature 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."

Line 34: specify with albedo *brightening* - will be clear to some but not all, especially since the bulk of the paper is about albedo *darkening*

As part of this review we have already changed the text (L31-35) 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).*

We hope that this modification has also addressed this comment by specifying a deliberate albedo increase.

Line 40: "*. 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 convection initiation (CI) (Branch & Wulfmeyer, 2019)."* --- make clear in 2nd sentence you're no longer talking about measured or observed things, but rather a model simulation

Thank you for pointing this out. We have changed this text (L40) accordingly from:

"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 convection initiation (CI) (Branch & Wulfmeyer, 2019)."

to:

"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 of jojoba plantations reproduced similar differential heating, and an associated increase in cloud development and convection initiation (CI) (Branch & Wulfmeyer, 2019)."

The authors hope this clarifies that these results come from two related, but separate publications.

Lines 46/47: be consistent - earlier references to albedo would make this "0.05"

The authors agree. To make it consistent, we have now modified the specification of albedo from % to a 0-1 parameter (L46-47)*:*

"Panels could be coated with black paint with an albedo ~0.05 or even with specialist coatings with < 0.01 albedo (Theocharous et al., 2014)."

Line 47: PV - define (photovoltaic I assume)

Agreed. This line (L47) is now changed to:

"Other surfaces which may modify weather are ***solar photovoltaic (PV) panels*** *(Li et al., 2018; Lu et al., 2021; Mostamandi et al., 2022)."*

Line 51: not just sensible heating, but also high surface temperatures (increasing LW out of the surface through increased surface T)

The authors agree this could be clearer: In a similar manner to the changes at L250 we have modified the lines

"With PV, one must also account for radiation converted to electrical power, which for a given radiation flux would leave less energy for sensible heating."

to:

"With PV, one must also account for the amount of shortwave radiation converted to electrical power, which may lead to modified skin temperatures, longwave fluxes, and net radiation."

Line 51: "net total" - net total what? (I assume "net total SW absorbed that goes directly into the local surface energy budget" or something along those lines)

Agreed. The authors have modified this line (L51) from:

"The net total can be expressed as an effective albedo, here in Eq. (1):"

To:

"The net total 'loss' of shortwave radiation can be expressed as an effective albedo, here in Eq. (1):"

Line 52: Is this equation necessary / especially as an equation on its own line? It doesn't get used in the rest of the paper, and you don't actually simulate these combined albedo/energy uptake surfaces, so it is kind of distracting… You could put it in-line and emphasize that it is for background info only, and NOT what you're going to try to model here.

The authors are happy to change this to an inline equation, if acceptable to the editor. We suggest to remove the equation number and change the line (L52) to:

"The net total 'loss' of shortwave radiation can be expressed as an effective albedo, as $A_{eff} = \delta + \varepsilon$.

Line 54: "delta" - This is the "regular" surface albedo, correct?

This is correct. The authors decided to use the definitions provided by the referenced author (Taha, 2013). However, to clarify that reflectivity is the surface albedo, we have changed the line (L54) to:

"where δ and ε are reflectivity (albedo) and conversion efficiency, respectively (Taha, 2013)."

Line 55: This comment is kind of long and the actual paper doesn't simulate solar panels taking energy "out" of the surface energy budget, so perhaps consider altering this text to emphasize that it is background information for the reader, and that you *aren't* going to be trying to capture this in this study! This discussion definitely sent me down a rabbit hole when you were describing your methods, until I realized in the results that this discussion isn't actually applicable to the actual simulations you ran.

The authors agree that this section could be shortened a little since detailed simulations of PV panels are not the focus of the paper. Therefore, we have reduced the following lines (L54-56) from:

"PV efficiency can theoretically reach ~46% in laboratory conditions (Allouhi et al., 2022), but in reality, is usually closer to 10-20%, with most radiation transformed into heating (Taha, 2013). This may be useful for rainfall enhancement, but can reduce cell efficiency and longevity (Dwivedi et al., 2020)."

to:

"PV efficiency is typically only ~10-20%, with much of the radiation transformed into heat (Taha, 2013), thereby offering potential for rainfall enhancement if implemented on a large scale."

Secondly, based on your comment, we clarify that we are not explicitly aiming to investigate PV panels in detail, but introduce them only as a possibility for use as an ABS surface. We propose to clarify that we are not focussing on PV or vegetation by modifying the following lines (L57-60) from:

"Assuming the lowest albedo of 0.04, and panel efficiency between 0.1 and 0.15, this would yield effective albedos of 0.14 and 0.19, i.e., similar to jojoba and jatropha. For simplicity in this study we will use an umbrella term 'Artificial Black Surfaces (ABS)' for these systems, whether they are made of black panels, PV or any composite of such surfaces."

to:

"Assuming the lowest albedo of 0.04, and a panel efficiency of 0.1-0.15, this would yield effective albedo (A_{eff}) values of 0.14 and 0.19, i.e., similar to jojoba and jatropha. Although solar PV panels may be a suitable subject for future research into rainfall enhancement, this study focuses on simulation of generic black-painted panels with a set of prescribed parameters to describe the likely land surface properties of such a surface. In this study we use the term 'Artificial Black Surfaces (ABS)' for these panels."

To simulate this, would have to modify the surface energy budget to have a special "energy out" term that is energy production; this would be the most physically consistent way. Instead, it sounds like the authors have imposed surfaces brighter than actaul PV panels, such that the energy that typically would go to energy production is instead reflected away from the surface as SW radiation. For the most part, this probably isn't going to qualtiatively change their answers given the magnitude of changes imposed here. But the atmosphere *does* absorb SW radiation, including SW reflected from the surface, and also altering SW albedo in these low water vapor, relatively cloud-fre desert regions will alter the top of atmosphere energy balance in a way that could influence circulation.

Thank you for these well-considered ideas. Given that we have now narrowed the focus toward black-painted panels we consider it is therefore no longer necessary to delve too deeply into PV properties. However, the radiation exchanges you discussed are very interesting and we are considering future publications on solar PV including both measurements and simulation components.

If you instead had the surfaces as dark as they actually are, with an extra term in the surface energy budget that removes energy used for power generation, you'd have to release that energy *somewhere* in a coupled model to conserve energy. In a regional model, though, you could just assume that the energy is moved out of the region you're simulating. I'm not suggesting you re-do your simulations this way - just that you explain what is and is not representative of the actual physical system (real world) in the way youv'e chosen to simulate this.

Please refer to comment above.

And of course, if the surface is made artificially dark without any power production - e.g. just painting surfaces dark, which the authors do discuss as one option of ABS - the power / energy conservation thing isn't a concern!

Please refer to comment above.

Line 56: What is reducing the cell efficiency with precip? Being low efficiency makes it lower efficiency with time? Or getting rained on makes it lower efficiency with time?

Please refer to comment above. We have removed this reference to the efficiency reduction now, along with the citation (L56-57):

"This may be useful for rainfall enhancement, but can reduce cell efficiency and longevity (Dwivedi et al., 2020)."

The authors hope that the manuscript is more clearly defined now.

Line 84: Is "material" really appropriate here? Maybe "Model and Methods"?

Agreed. We have changed the section title as you suggest to "Modelling and Methods".

Line 87: specify the region (title just says "arid regions", and intro discussed the Middle Eastern Gulf region, but here specify that that is indeed where you're going to simulate, and maybe modify the title to reflect the actual region of study – it would be a leap to extrapolate from this analysis to all arid regions, as the background flow and moisture sources are pretty critical to the results).

Thank you for this important point. We suggest modifying the title to "Scaling Artificial Heat Islands to Enhance Precipitation in the United Arab Emirates".

To specify the region, we have also amended the text (L87) from:

"Simulations were carried out with the Weather Research and Forecasting (WRF) model (V4.2.1, Powers et al., 2017). WRF has been used in the region for numerous model evaluation and process (Branch et al., 2021; Fonseca et al., 2020; Valappil et al., 2020; Wehbe et al., 2019; Nelli et al., 2020; Schwitalla et al., 2020), and rainfall modification studies (Mostamandi et al., 2022; Wulfmeyer et al., 2014; Branch & Wulfmeyer, 2019)."

to:

"Simulations were carried out with the Weather Research and Forecasting (WRF) model (V4.2.1, Powers et al., 2017). WRF has been used ***in the middle-east region*** *for numerous studies on model evaluation and processes (Branch et al., 2021; Fonseca et al., 2020; Valappil et al., 2020; Wehbe et al., 2019; Nelli et al., 2020; Schwitalla et al., 2020), and rainfall modification (Mostamandi et al., 2022; Wulfmeyer et al., 2014; Branch & Wulfmeyer, 2019)."*

Line 90: in the same domain? Please specify.

Thank you. To clarify that this is a model domain, we have added the word "model" in the text (L90) to:

*"Here, we use the same **model** domain, resolution and configuration"*

Line 92: MYNN - what is this?

Thank you for alerting us to this oversight. This refers to the Mellor-Yamada-Nakanishi-Niino boundary layer scheme, selectable in WRF (Nakanishi & Niino, 2006). We will clarify this by modifying the text (L93):

*"…to take advantage of improvements to the **Mellor-Yamada-Nakanishi-Niino (MYNN)** boundary layer,…"*

Line 98: I know the choice of convection scheme needs to be specific to the model resolution, but am not up-to-speed enough with the WRF convective scheme options to know what the appropriate choice here is. Also, please specify which scheme you used.

This is a very good point and may be clarified. As the model is being run at the so-called 'convection-permitting' scale (usually ~<4 km) convection is not parameterized but is simulated explicitly. We clarify this by adding the following line (L94):

"The model is being run at a 2.7 km grid increment, which lies in the so-called 'convection-permitting' (CP) scale (generally <4 km), which allows that convection can be simulated explicitly, and not parameterized."

We hope this is now clear.

Line 105: are you still talking about the outside of domain forcing? Clarify. Sounds like you're talking about the model, but the land surface you use in the model domain is NOAH-MP, right?

This does indeed relate only to the global IFS model proving the boundary and initial conditions, which has its own land use model, HTESSEL. Within the WRF domain, the land surface processes are carried out by the NOAH-MP scheme. The only exception to this are OSTIA sea surface temperatures which is the only data re-initialized periodically *within* the WRF domain itself.

We clarify this by modifying the text (L107-108) from:

"Additionally, OSTIA sea surface temperature (SST) data (Δx 1/20°, Donlon et al., 2012) were also ingested every 12 hours (00:00 and 12:00 UTC), which is particularly important for simulating sea breezes."

to:

"The ECMWF forcing data is only used to provide the lateral boundary and initial conditions for WRF-Noah-MP which then itself develops 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."

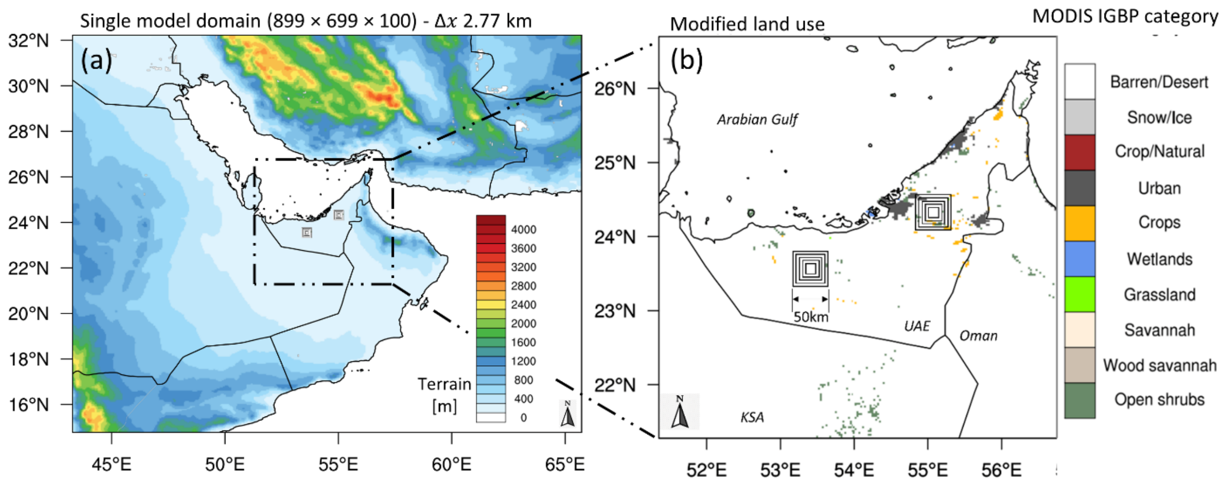
Figure 1: what is the 899x699x100? number of x, y, and z grid cells?

We agree this could be expressed more clearly. We have amended the line (L101) to:

"The model grid has horizontal dimensions of 899 (east-west) × 699 (north-south) cells, and 100 vertical levels."

Figure 1: Could you please make ocean a different colour than low (0-400m) land?

Thank you. The figure has now been modified accordingly.



Line 128-131: this discussion is a great orientation to the base-state of the region! nice!

Thank you for this positive comment.

Line 136: what four selected cases? To help the reader not get confused, it would be helpful if before this point, you say that you're going to do, for each ABS box, 4 different runs of YYYY days each, each forced with weather conditions from <insert time periods here>

This is a good suggestion. To better summarize the modelling method, we have accordingly modified the line (L137) from:

"We illustrate weather conditions for the four selected cases in Figures 2 and 3 (from Control) to highlight the likelihood of impacts."

to:

"In this study we selected four one-day case studies. For each of these days, we simulated the five ABS scenarios (10, 20, 30, 40, 50 km squares) and a Control simulation for a total period of 24 hours (00:00-00:00 UTC). We illustrated the respective weather conditions for our four selected cases in Figures 2 and 3 (from Control) to highlight the likelihood of impacts."

Line 137: "Typical CI…" --- Define (I assume "convective initiation")

This was already introduced. However, to avoid overuse of acronyms we have changed 'CI' to 'convection initiation (CI)' at this location.

Figure 2: Adding a map, either as a separate figure or in one of figure 1 or 2, that shows the prevailing near-surface and aloft wind directions (on the map) for these two time slices would be really helpful. (Figure 3 has this for 10am, I assume near the surface? But maybe not near the surface – not specified!)

The wind field shown in Figure 3 is 10-m winds at 10:00 UTC. This is marked both in the caption and the text. Prevailing winds aloft are shown as wind barb profiles in the Skew-T plots of Figure 2 at both 06:00 and 10:00 UTC. We are confident that together, these provide a sufficient picture of the wind field at the relevant locations and times (pre-sea-breeze and at the time of the seabreeze).

Figure 3: what are 18.07, 24.07, 25.07, and 27.07? Can you write "July 18", "July 24", etc instead? otherwise this reads like a number and the reader gets confused about what the number means (when in fact it is a date)

Agreed. The date format throughout text and figures has now been modified to e.g. 'July 18'.

Figure 3: figure 3 b, c, d - make the reference vector larger, like in panel a --- otherwise it is too tiny for my poor eyes...

Agreed. The larger reference vector has been added to the other panels.

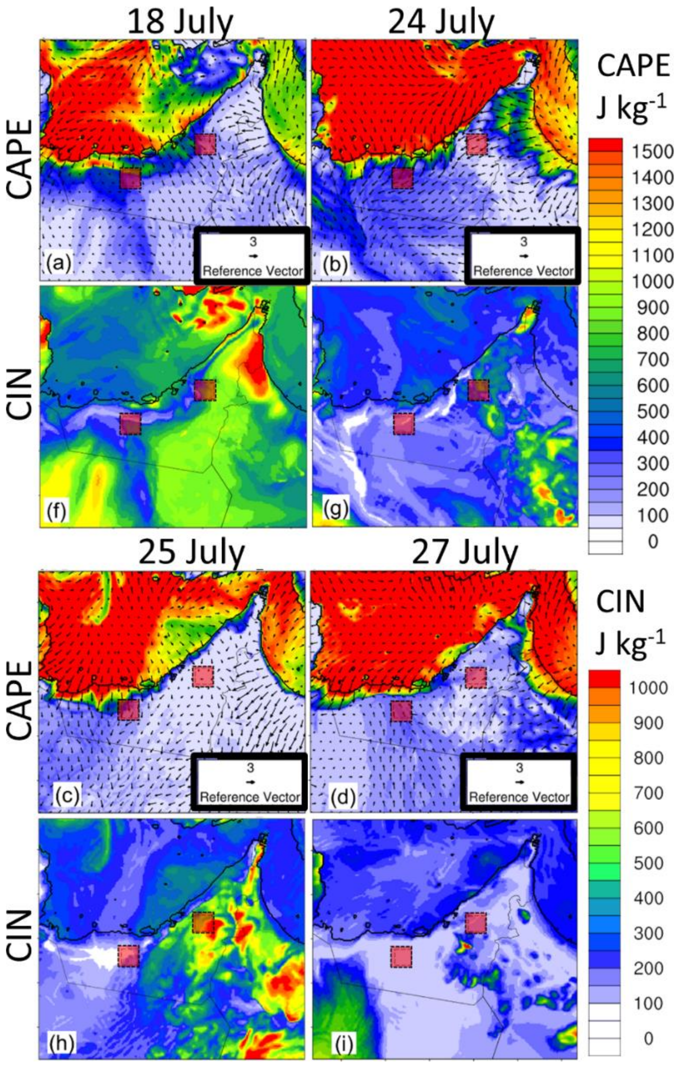


Figure 3: what level are these winds from? surface?

This is stated in the Figure 3 caption:

Figure 1: Control thermodynamic conditions at 10:00 am (UTC) during the four case studies, with red boxes to show the relative position of the ABS. The top row shows convective available potential energy (CAPE, J kg^{-1}), ***10-m wind vectors****, and a box showing the vector reference length for 3 m s^{-1} (panel (a)). The bottom row shows convective inhibition (CIN, J kg^{-1}). These conditions were used to investigate the daily sea breeze timing and select our case studies.*

And also in the text at L142:

"Figure 3 shows a horizontal perspective of convective available potential energy (CAPE), convective inhibition (CIN), and ***10-m winds*** *(at 10:00 UTC)."*

Figure 4: Again, please label with "July 18, July 24" etc. What are the arrows for? Wind vectors on the control would be helpful, but I don't think that is what the arrows are for here?

We agree, and have modified the date format throughout. The arrows were just to indicate that the panels below are a modification of the Control. However, as the arrows are perhaps more confusing than useful, we have now removed them. Regarding the wind vectors, the authors have considered this comment. Firstly, we think that because the figure shows precipitation totals of a whole day, it is probably not so useful to see wind vectors from a certain timestep, or even a daily vector average (which given the complex changes in daily wind flow, could be misleading anyway). Finally, we think that in such small panels, wind vectors would make the plot far too busy in any case.

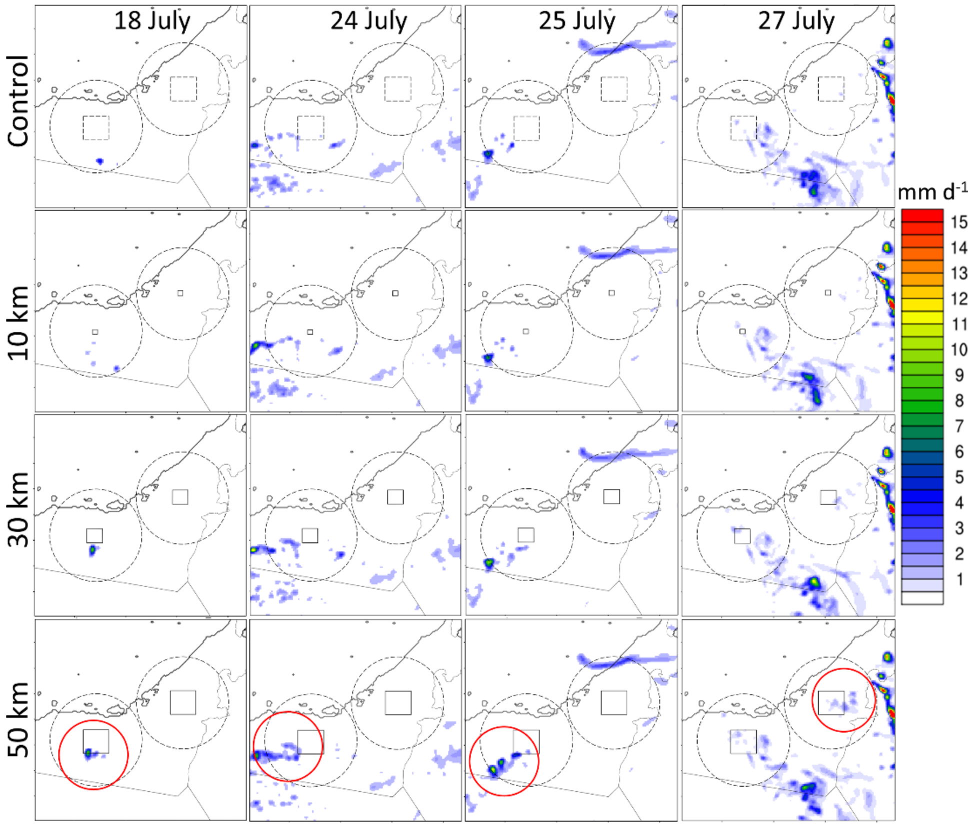


Figure 4: Inconsistency between text (line 180) and figure caption. Do you calculate the precipitation response in a circle of radius 90 km (ie diameter 180 km), as on line 180, or a circle of diameter 150 km (radius 75 km) as in the caption of figure 4?

Thank you for noticing this error! The caption is incorrect and should read 180 km diameter. This is now changed.

Line 200: panel e of what? Sounds like you're talking about figure 5 in this paragraph, but figure 5 only has panels a-d.

Thank you for pointing out this error. It is panel b. We have modified the text accordingly (L201) to *"(Figure 5, panel b)*

Figure 5: clarify "millions of m3" somewhere

Thank you. We have now added "*The amounts are in million m*^{3}" To the Figure 5 caption.

Figure 5: again, please write "July 18, July 24..."

Agreed. This now done.

Line 207: clarify this is combined accumulated precipitation volume for *both* regions

Thank you. This is already indicated in the text (L198) with "*(sum of east and west circles)*". We have now added this same text to the caption.

Line 207: "…*UAE per capita supply based on 500-liter capita-1 day-1 (right axis, people yr-1), amongst the highest in the world*" --- I read this as the UAE having the highest per capita supply of water in the world... is that right? That is not intuitive to me! Or is it the volume of supple that is high? Please clarify.

The UAE does indeed have one of the largest per capita water consumption rates in the world. To clarify this and emphasize the importance of protecting water resources in this region, we have modified the text (L207-209) from:

"Panel (b) shows the difference between the ABS and Control expressed in volume (left axis, mil. m^3) and UAE per capita supply based on 500-liter capita^{-1} day^{-1} (right axis, people yr^{-1}), amongst the highest in the world (Albannay et al., 2021; Yagoub et al., 2019)."

to:

"Panel (b) shows the difference between the ABS and Control expressed in volume (left axis, mil. m^3) and UAE per capita supply based on 500-liter capita^{-1} day^{-1} (right axis, people yr^{-1}), which is one of the highest national consumption rates in the world (Albannay et al., 2021; Yagoub et al., 2019)."

Line 212: "*There is a small surplus on 24 July though.*" --- Just for the 50 km case, right? No, I'm confused - what do you mean "surplus"? I don't see the demand listed anywhere on thesee plots, so what does a "surplus" mean?

The authors agree with your comment that 'surplus' may perhaps not be the best word to use here. We have therefore changed the word 'surplus' to 'increase over Control' at (L189 and L213).

Figure 6: So, Panel C is showing that the 50 km blobs of ABS lead to almost a 50% increase in precipitation, is that right? That seems worth highlighting!!!

The authors agree with your assessment of this significance. At your suggestion we decided to add some text to emphasize this. We have added a short sentence here (L227):

"The mean increase from both 50 km ABS represents almost a doubling of the rainfall in Control."

Line 258: influence of what from the surrounding what?

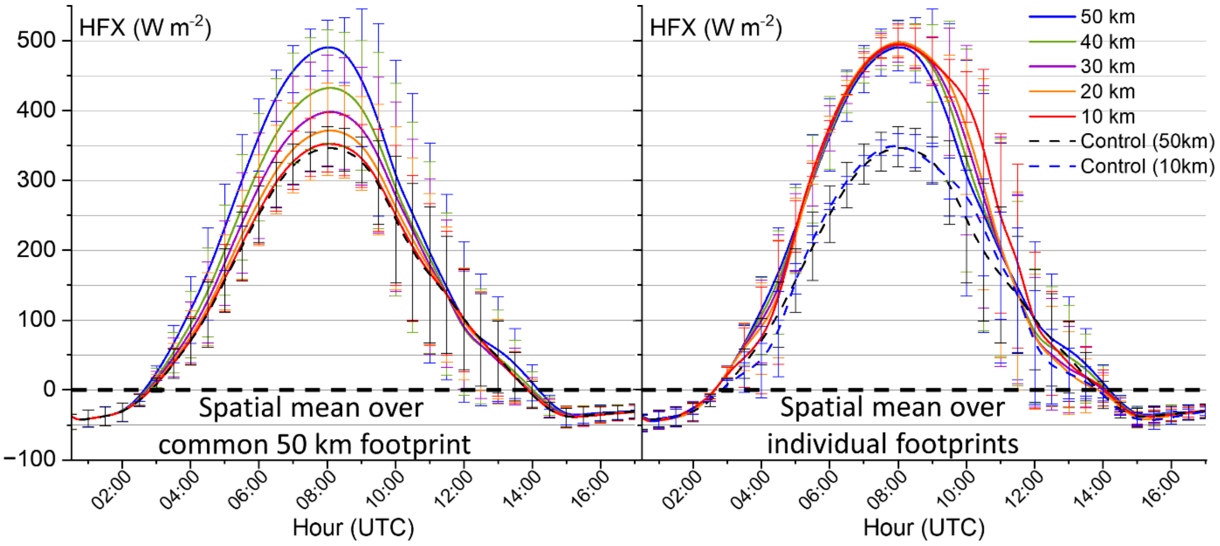
Here the authors are referring to the influence of the surrounding physical environment on the air mass inside the ABS zones. This could come from advection, entrainment or other processes. To clarify this, we have modified this sentence (L259) to:

"e.g., due to influence from the environment surrounding the ABS, such as from advection or entrainment of heat, moisture, or other quantities"

We hope this has improved clarity here.

Figure 7: it would be helpful to highlight the zero line here, e.g. with a darker horizonal line - at least in the pdf I have, there is no / almost no line visible at 0, 50, and 300. The one at 0 is particularly important to have though, and to make stand out more than the rest!

Agreed. The png format uploaded with the submission are lower resolution than the final tiffs that will be provided. Hence the faded lines. We have now added a thick reference line at 0, and show the final high resolution tiff image here.



Text/Grammatical:

Line 30: “are become” - "are becoming" or "have become"

Thank you. This line now reads: *“Regional crises like high temperatures, drought, wildfires, flooding and water scarcity are becoming more severe”*

Line 258: “flu” -> “flux”

Thank you. Corrected.

Concluding remarks from the authors:

Many thanks again to the Reviewer for taking the time to review our work and for providing well-thought out and constructive comments. We feel that addressing your comments has greatly improved the manuscript.