A modern-day Mars climate in the Met Office Unified Model: dry simulations

Response to reviewers:

Nov 2022

First, we thank the reviewers for their time and valuable comments. We have responded to each comment below with the original comment and the response in blue. We have provided an amended version which shows the old text with a strike-through and provides the additions/amendments in green.

Reviewer 1:

This manuscript provides an introduction to the modern Mars version of the Met Office United Model (UM). The paper describes the foundational adjustments that were made to the model to adapt it to modern Mars and then evaluates its performance relative to the LMD Mars Planetary Climate Model (PCM) and compares UM simulations with (RA) and without (RI) radiatively active dust. The model is still in development and currently lacks CO2 and H2O cycles. The model broadly reproduces the martian dust cycle and annual thermal and dynamical conditions. It will be useful to the scientific community to have another Mars GCM, but with terrestrial-derived physical parameterizations to help better understand the physics of the martian climate. I recommend the manuscript for publication after a moderate revision.

We thank you for a thorough review of our paper and for their reaffirmation of the usefulness of another Mars GCM.

Substantially more discussion is needed regarding several aspects of the modelled dust particles and dust cycle in general.

– First, only a single sentence is used to describe the dust optical properties used. Based on the reference, I assume this implies that a terrestrial dust composition and optical properties are used. But that reference also refers to other references with those details. The authors should summarize the relevant information in the text and also compare and contrast that with known properties of martian dust (chemistry, optical properties, albedo, etc.). Additionally, discuss how these differences could impact the simulated climate.

Done, we have added sections to the manuscript to correct this, most relevantly Section 2.4 and Appendix A3.

– Second, how is dust lifted in the model? It seems (based on a few sentences throughout the text) that the model only lifts dust through wind stress-driven saltation. Is that correct? Observations have shown that dust devils likely supply 50% of dust into the atmosphere, so it should be mentioned explicitly if there is no parameterization for dust devils. I have some additional comments below that relate to this.

We apologise for the lack of clarity. The reviewer is correct that the main uplifting process in this study is wind stress-driven saltation. Although dust devils have been shown to affect the total uplifted dust (shown by Neakrase et al., 2016), the UM does not currently include an explicit dust devil uplifting parameterization, but instead relies on the threshold wind friction velocity, which is dependent on the dust particle size and roughness length (Marticorena and Bergametti, 1995). Dust production rates change depending on the sizes of the dust particle, which uses the prescribed bin abundances as described in Sec. 2.4. The description of this in the manuscript has been amended to provide more explanation and description of the methods used for dust production and uplifting within the UM, namely in the second paragraph in section 2.4 (highlighted in green in the manuscript).

Given the paper is focused on the dust cycle and its impact on the climate, I think another figure or two on the modeled dust cycle would be quite helpful. Specifically, a figure showing the globally averaged aerosol optical depth as a function of the season for RA and RI simulations, perhaps with the PCM overplotted, and a second figure identifying model grid points favored for dust lifting and deposition. See Gebhardt et al. (2019) for similar figures: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019JE006253. I think such figures could replace Figures 1 and 2 if desired.

We thank the reviewer for the suggestion. We agree that this would add to the analysis within the paper and have added figure 11 depicting dust content and wind vectors at the near-surface level. We have added this in addition, rather than a replacement for Figures 1 and 2, as the information of the figure is different.

I don't think Figures 6-9 are structured in the most effective manner. The difference plot in the center is intuitively for the differences between the left and right columns, but that isn't the case here. And differences between the RA simulation and the PCM are discussed in the text, which leaves the reader having to mentally try and create such a difference plot. I don't think the authors need to double the number of plots necessarily (one set comparing RA and RI, another comparing RA and PCM), but I do think the visual comparison between RA and RI and RA and PCM should be separated.

Enabling comparison between the RA: RI/PCM outputs presented a significant challenge, as presenting standard plots for the RA-RI scenario (not difference plots) made the differentiation between the two difficult. While presenting the RA and PCM outputs as difference plots made it difficult to interpret what the actual values were for each output. Initially, we had separate plots for RA–RI and RA–PCM comparisons, where the plots comprised of RA—RI—RI-RA and RA—PCM—PCM-RA for each of the months. This initially addressed the comment the reviewer has now pointed out but severely inflated the figure count of the paper. Instead, as the current plots are difficult to improve without compromising readability for one of the comparison simulations (RI or PCM), we have added the aforementioned initial plot format as supplementary material (RA—RI—RI-RA and likewise for PCM), allowing for the reader, who wishes to view the RA–PCM or RA–RI comparisons more clearly, to see the relevant comparisons in more detail without diluting the comparison of the standard figures (6–9). Therefore, we would prefer to keep the current arrangement.

Line 98: Are spectral files for the ancient Sun (3.8gya) being used here?? Or is that just a typo?

You are correct, this is a typo. We have checked to ascertain this and the correct file names have been given in the manuscript. Thank you for spotting this typo.

Table 1: Maybe change the title from 'orbital parameters' to 'physical constants' or something else as the gas constant and scale height are mentioned? Additionally, is the scale height prescribed as a constant? How does that work? That value seems high relative to Mars (11 km).

We thank the reviewer for the suggestion, the table name has been amended. The scale height was incorrect for Mars, but as it does not actually affect the model integration, we have removed this value from the table.

Section 2.3: There's no mention of surface albedo or thermal inertia here or elsewhere. Are martian values used as boundary conditions? Something else?

We thank the reviewer for highlighting this, we have added a description on this at the end of section 2.3. We prescribe a constant surface albedo of 0.3 and constant thermal inertia of 368.646 J m^{-2} K^{-1} $s^{-0.5}$ (prescribed as a thermal capacity after timestep of 1.359e+6). This is close to the average thermal inertia of the Martian soil (Mellon et al., 2008).

Line 155: Is an effective dust radius of 3μm assumed as it was in Madeleine et al. (2011)? Some other value? Specify it here.

The mean radius is assumed to be 1.5μm, while the variance is assumed to be 0.3, as in the **second** scenario of Madeleine et al. (2011). This was chosen (as supposed to the third scenario) as the temperature bias tends to be larger than expected. As we do not currently include water ice clouds (as also not done by Madeleine et al. (2011) at the time of the study), we choose the second scenario dust size distribution to potentially help mitigate the omission of water ice clouds (by increasing suspended dust and increasing temperature) as that has been done in other idealised simulations (Ball et al., 2021). We have added the detail on which scenario from Madeleine et al. (2011) we use.

Line 167: Does it really take 40 years to reach steady state? Why so long, especially without H2O or CO2 cycles?

As the reviewer would assume, 40 years is much longer than is needed to achieve a steady state (normally only a few years would be sufficient for our idealised setup). As the dust reservoirs are not prescribed, however, we opted to run for an extensive number of years, to allow for the dust reservoirs to form dynamically from the simulation. This means that, while the reservoir abundance and location might not exactly match Mars observations, dust is not being uplifted in regions that typically would not feature any/much dust. This essentially bypasses the initial prescription of a uniform dust content across the planet. While the simulations could be analysed from after a few years, we opted to err on the side of caution and any potential spatial dust reservoir disparity to settle out. The text has been amended in the first paragraph of section 3 to highlight why this was done.

Line 221: "meaned" reads oddly to me. "Averaged"?

Meaned was used as averaged could imply a median average, but position adjusted to make it more readable

Line 237: should "to" be "in" here?

This section of text has received additional text, in addition to the amendment

Lines 239-240: it looks like 1-2 m/s and 1 K differences to my eyes?

The reviewer is correct, this is a typo and has been amended. In addition, some text as been added just after to provide more clarity on why these differences are so small.

Line 251: "in each month" should be "in this month"?

The reviewer is correct, this is a typo and has been amended to "this month".

Line 252: This difference perhaps could be somewhat ameliorated by tuning the dust cycle, however? Was any tuning done to the dust cycle? This goes back to general comment #1. Lifting efficiency factors, etc., are typical tuning parameters for Mars GCMs.

The dust scheme is tunable, with the horizontal flux rate (used to increase/decrease dust emissions) able to be set to a given value. This was tuned to match typical dust content during month 9 in the PCM. If this parameter is raised, it would lead to more dust across the year and vice-versa for decreasing it. The detailing of this has been added in section 2.4.4, in the paragraph before aerodynamic roughness length. Although increased dust abundance during month 3 (without an increase during month 9) could somewhat be achieved by increasing this tunable parameter **and** decreasing the total dust availability (i.e. limiting the total amount of available dust but promoting dust uplifting rates). This was not preferred as it would lead to a specific configuration of dust abundance to match other results, rather than a representation of the dust schemes default behaviour when under typical conditions. We believe this is justified, as tuning the dust conditions to match PCM results would never yield useful comparative results, due to the omission of wet deposition and varying atmospheric pressure affecting dust rates. Adding the processes described in the future work section would undoubtedly alter the dust distribution, so once these are added a more rigorous tuning process can and should take place, as the results would be more comparable.

Line 256: "reverse" should be "reverses"

Amended

Figure 7 caption: specify that negative values indicate southward winds.

This has been added to figures 6 and 7, as both benefit from the addition of information regarding the wind direction depicted by the colourshade.

Section 4.2. titles: include Ls range with each month title (e.g., "Month 9: Ls 270-300°").

Added

Line 314: "RI" here should be "RA"

Amended

Figure 9: I appreciate the reason to use different color bar scales for RA and PCM here, but it's still

quite confusing by eye when the RA plots have deeper red colors. Can the color bars be adjusted to help the reader visually intuit that RA often has less dust? Additionally, percent difference (as is used in the text) might be more intuitive for the difference plot in the center in this case.

We agree with the point made by the reviewer, but initial testing with occasional matching colour scales (i.e. colour scales match only when values are comparable between the UM and PCM, mainly during month 9) found that the plots were easier to misinterpret. We found that, if the months with more dust were along the same colour scale, then it was much easier to visually omit the values during month 3 and judge the differences solely based on colour differences. Figure 1 shows the UM RA and PCM with the same colour scales. As can be seen, there is seemingly no dust in the UM during months 3 and indistinguishable dust during months 6 and 12. With non-matching colour scales across all months, however, the reader is more inclined to observe the values for comparison, and use the colour scale to primarily assess the dust distribution. For these reasons, we prefer to keep the current colour scale arrangement. As per a previous response, additional plots for solely RA–PCM comparison are now provided in the supplementary material, these plots provide a difference plot for the two models, allowing for easier interpretation of areas with differing dust content.
Section 4.2.3 and Figure 9: One consistent difference (at least for months 6-12) between RA and RI seems to be that RA lifts dust farther south than RI? Is that correct? Is that a feedback effect between radiatively active dust and the Hadley circulation perhaps? This may be worth some discussion in the text.

This is incorrect, figure 9 shows that RA features comparable dust amounts near the SH pole during month 6 to the RI scenario, and less dust in the RA scenario during month 12. The RA–PCM differences are that the PCM consistently features more dust nearer the SH pole, even during month 9. The likely reason for this is that the majority of the dust uplifting in the RA scenario is subject to a positive feedback loop, with more dust warming the vertical column which causes more vertical uplifting of dust for that region. We believe this to be the cause of the large centralised plumes featured in the UM-RA, which, as it is absent in the UM-RI, further suggests this interaction. We agree that this should be highlighted further in the text and have added a section in 4.2.3 to represent this

Line 322: I'm confused by the sentence beginning with "The high vertical uplifting…". This sentence makes it sound like the UM has a "rocket dust storm" parameterization, which I don't think is true. So what is really meant by "high vertical uplifting"? What physical mechanism is bringing higher dust mixing ratios to high heights in UM-RA? Months 6 and 9 seem to natively produce a high-altitude dust layer (Heavens et al., 2011; Guzewich et al., 2013), which I think would be quite novel for a free-running dust simulation! More discussion is needed here.

You are correct that the UM does not feature a rocket dust storm parameterization. The primary source for high-altitude dust would be from localised areas of high vertical winds, rather than an explicit parameterization to increase vertical uplifting rates. We thank the reviewer, particularly for their consideration of the importance of being able to dynamically simulate a detached dust layer using free dust. We agree that this is an exciting feature that could potentially benefit the Mars modelling community. A paragraph has been added in section 2.4.4 to clarify the uplifting mechanisms used.

Section 4.2.3: calling back to my general comment #2, does dust opacity peak in month 9 in UM-RA and UM-RI? That is the typical pattern for free-running dust simulations in Mars GCMs,

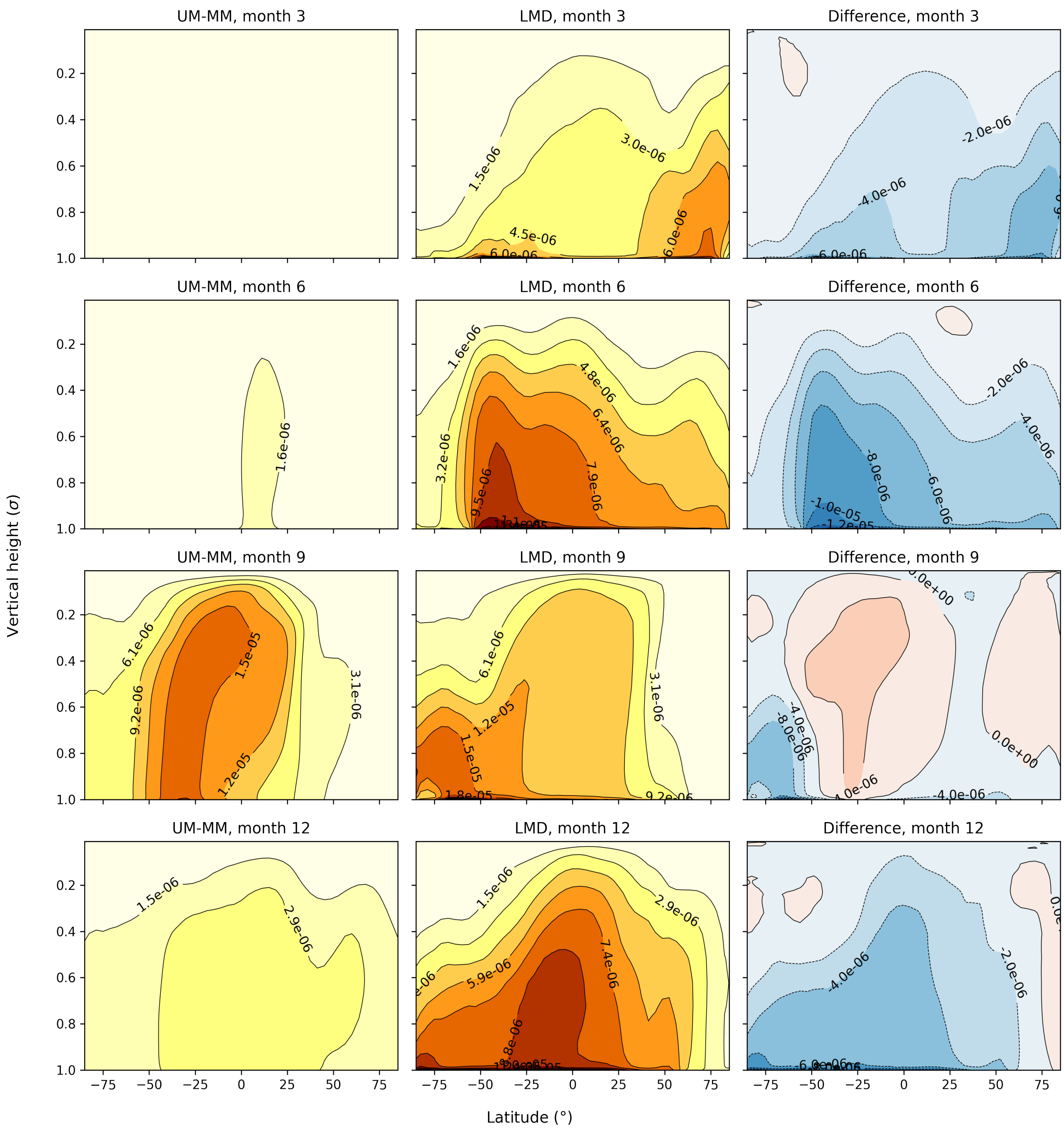

Figure 1: Dust content for the RA dust scenario and PCM for each month. Colourscales are matched between months and model outputs.

but the real Mars often has a "solstitical pause" in activity near the southern summer solstice outside of years with solstitical global dust storms. I imagine PCM reflects this since it uses the Montabone dust climatology.

Dust MMR peaks during month 10 in the RA scenario, but is only marginally higher than during month 9 (max of $1.80{\times}10^{-6}$ over a wider area compared to $1.75{\times}10^{-6}$). The RA (or base) and RI scenario outputs for each month can be seen in figures 2 and 3. The plots are given using the native vertical coordinate of the UM ("hybrid-height"), but restricted to 40 km to view dust better. Dust in the RI scenario also features more dust in month 10, with maximums up by $\sim 10^{-6}$. As can be seen, the general distribution of the dust remains similar between months 9 and 10, but with slightly more dust during month 10 for both scenarios. This is caused by the lag between increased temperatures and uplifted dust (likely as a result of the month-averaging process and the time it takes for the dust to be transported over large distances/heights within the model), as the higher temperatures uplift more dust, the consequence of this is not entirely reflected in the simulation until month 10.

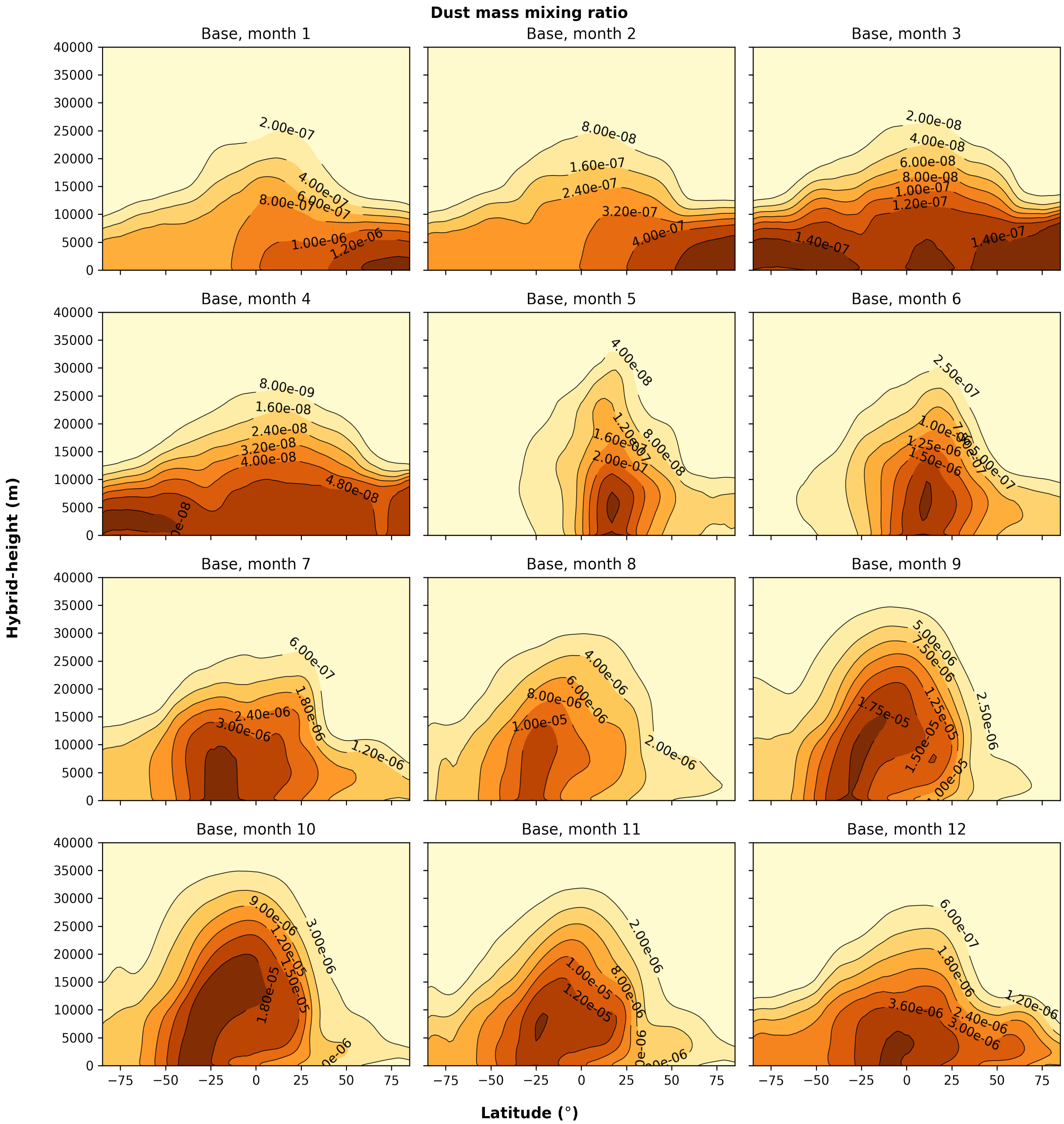

Figure 2: Dust content for the RA dust scenario for each month.

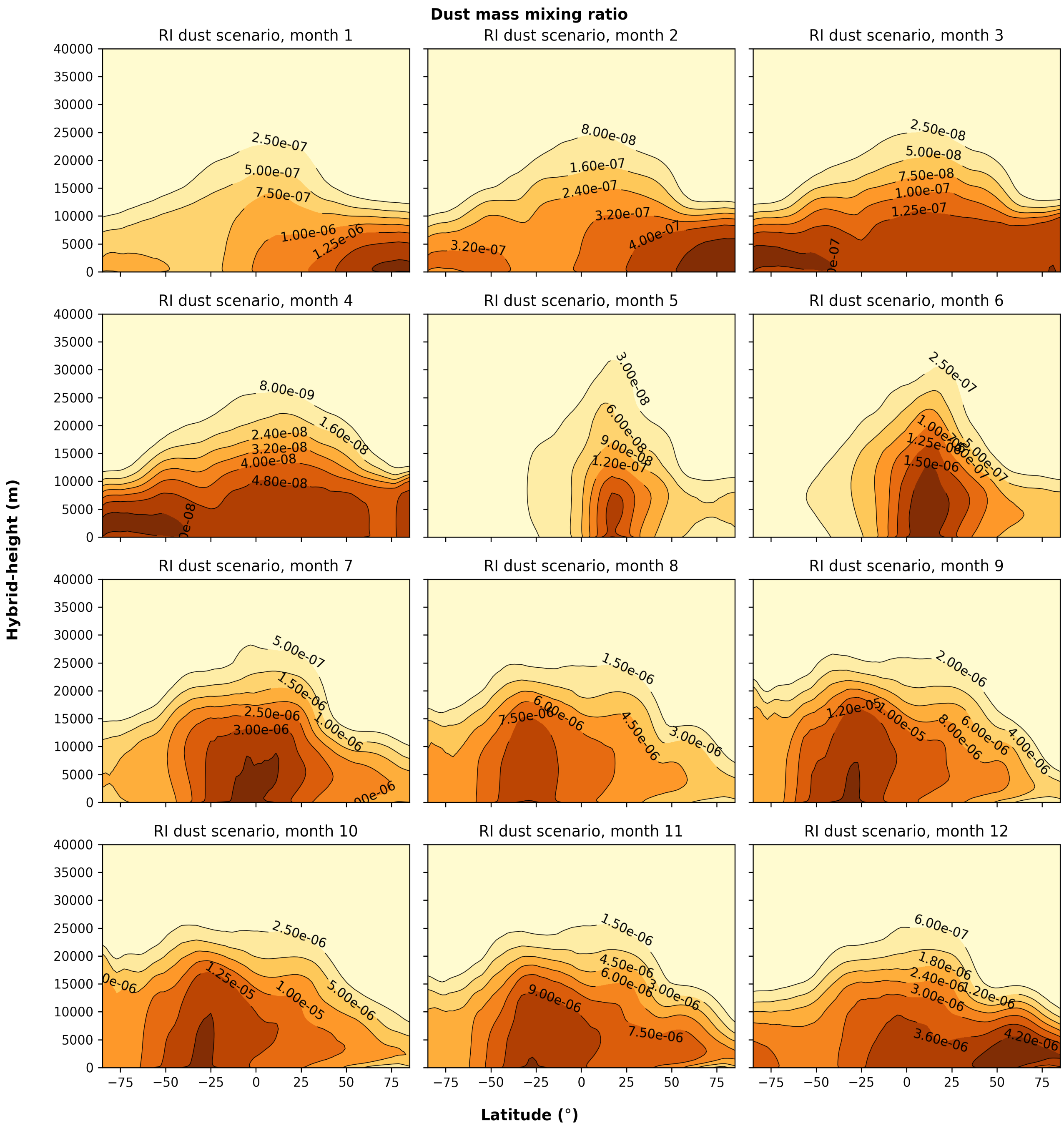

Figure 3: Dust content for the RI dust scenario for each month.

Line 347: The sentence beginning with "Dust abundance is higher…" is essentially repeated in the next paragraph. Similarly, saying a simulation is "higher" by a negative number is confusing.

Amended by removing the former version, and instead ending the paragraph with "more dust in the RA scenario)."

Line 378: "Once again there is a difference…" of zero?

This sentence has been reconstructed to avoid this confusion

Line 380: Missing "Fig."

Added

Line 381: specify this difference is only in RA and RI, not in PCM simulations

Added

Section 4.3.3: Dust devils aren't mentioned at all! The most likely reason for this disparity, in my opinion, is that UM doesn't have a dust devil parameterization. Wind stress lifting is very low during the early portion of the year, while real Mars (and hence the PCM) maintains a background dust haze through dust devils. Kahre et al. (2017) and references therein discuss this in detail.

The review makes an excellent point. We have added a critique on why dust amounts during month 3 are so low in section 4.3.3. This will go in tandem with the description of the lack of dust devil parameterization as mentioned in an earlier response.

Line 471: typo "whata"

Removed the "a"

Reviewer #2:

This manuscript documents two simulations from a new Mars global circulation model, ported from the UK Met Unified Model. The addition of new, independent global models is welcome in the community and should always be encouraged. Its development will increase the capabilities of the modeling community. In advocating for the development of a new Mars model, the manuscript also makes a strong case for the need for a Mars Atmosphere Inter-comparison Project. CMIP has been extremely successful for the assessment of future climates for Earth, and a MAIP might hold similar promise for Mars.
Generally, the model proves capable in simulating many of the large-scale features of the Martian climate. The model reproduces a dust cycle, which bears a reasonably good resemblance to that of Mars, without the need for forcing the simulation to observations. This is an impressive advancement. The manuscript is well-written and organized; however, a couple of discussion points are neglected, and most importantly, a key process is not included in the model. See main comments below and annotated PDF for minor comments.

We thank the reviewer for their thorough reading of the paper and their strong endorsement of new Mars GCMs. We also thank the reviewer for their affirmation of a key message intended for this paper, the calling for an inter-comparison project for Mars GCMs.

Major comments: I.) Most critical of the major comments: I understand that porting a terrestrial model to Mars is a substantial undertaking, but a Mars model lacking the CO2 cycle—and therefore having surfaces pressure being up to 20% too large for a given grid cell—seems like a massive (literally and figuratively) potential source of error. The authors recognize the need for a CO2 cycle on lines 521–531. A non-exhaustive list of the potential problems in simulating a realistic Martian climate without the CO2 cycle include: 1.) incorrect tidal amplitudes, since a given radiative forcing will, for a large part of the year according to Fig. 5, be working on more mass than actually exists. Because the tides are so important for closure of, for example, momentum budgets, getting this wrong makes the entirety of the presented results less robust. 2.) The reality of the radiative influence of dust may also suffer. If X amount of dust is lifted, the mixing ratio of X amount of dust would be less than reality if there was an incorrectly excessive amount of non-dust mass in the atmosphere. This comparatively thinner dust layer may not necessarily change the surface temperature because the total extinction would be nearly the same, but it might change the vertical temperature profile based on changed vertical distribution of dust. 3.) Similar arguments could be made on the impact spurious mass might have on various wave modes, but without running experiments, it is hard to know the non-linear changes on a model never adapted for Mars before, which is the point. 4.) Finally, the CO2 cycle is associated with a flow of the atmosphere from one pole to the other as the polar caps sublimate and deposit. This flow, while comparatively smaller the zonal winds of the Hadley cell itself, is still missing, and with it one of the important potential mechanisms for dust lifting in the high latitudes.
While overall, the simulated climate looks reasonable; as plotted in sigma coordinates, the effect of an incorrect surface pressure is obviated. I am left to wonder if the model would look even better if this neglected process were included; conversely, I am concerned that not including this process is hiding other issues that are not yet apparent. I would strongly encourage the authors to investigate the feasibility of incorporating the CO2 cycle in the present version rather than saving for a future manuscript. At least, a simplified parameterization as noted used by other models around line 525, could be attempted. The process by forcing the atmospheric mass to a prescribed surface pressure or enforcing mass sources/sinks as need in the poles at the appropriate times of year might be sufficient.

We thank the reviewer for their detailed comment, which we agree would provide an important improvement to the development of the Mars configuration of the UM. While we agree that a CO_2 ice scheme that affects the pressure of Mars would be eventually needed in the UM, we believe that the currently achieved developments warrant their own publication and discussion. This is so that the dust cycle can be scrutinised and assessed in isolation (in addition to Martian orography and orbital values). While the Martian climate is heavily influenced by pressure variations across the MY, the dust cycle has a predominant influence on the Martian climate during the SH summer/dust seasons. By isolating the dust cycle and the extent of its influence, the impact it has on the climate and the potential benefit of dynamically calculating dust reservoirs are allowed to be analysed in more detail than if the paper was also trying to present a dynamic CO_2 ice–pressure scheme. We believe this addition should warrant its own publication with accompanying analysis/discussion, as has been done similarly with Madeleine et al. (2011) for dust and Navarro et al. (2014) for water ice clouds. We intend to do this and are pursuing funding to develop our model further.
The reviewer highlights the overestimation of atmospheric pressure during month 3 of the simulation. As the primary focus of the study was capturing the behaviour during the dust seasons, we believe that capturing reasonable atmospheric pressure values during the dust season takes precedence (as shown in Fig. 5). While this does reduce the robustness of the results during the earlier months of the MY (as stated quite rightly by the reviewer), future work which would seek to improve representation during these months should seek to include this as a priority. The text has had two additions to address this point, 1) Section 4.1 has an extra plot showing surface pressure for the UM RA and PCM with a description of the UM–PCM differences. 2) Section 4.3.3 also features a note of this, highlighting how the pressure differences during months prior to the dust season might impact the total lifted dust content during month 9

II.) I missed a discussion on dust lifting. How is the process parameterized? How is the surface dust reservoir calculated? There is a brief mention in the results section on Line 320 that the UM calculates reservoirs and the horizontal motion, but this warrants a more complete description in the methods. Only on line 449, the fact that the dust reservoir is infinite is finally mentioned. This all needs to be organized into a specific section in the methods. It is impressive, as noted around Line 340, that a dust cycle is reproduced in the model without forcing, but it is difficult to assess how robust the cycle is without knowing how lifting is parameterized.

We apologise for the lack of clarity on this matter. A more detailed description of dust lifting has been added to the penultimate paragraph of section 2.4 (just before the description of the surface roughness used). We thank the reviewer for their commendation on the dust cycle reproduction without forcing, we believe that this intra-annual oscillation is an exciting result from the study that we would like to highlight. With the added detail on the dust uplifting parameterization the reviewer, and potential reader, can now more confidently assess the robustness of the presented dust scheme and results. Below a plot showing the surface pressure for UM RA and PCM is added to allow for scrutiny of pressure differences between the models.

One particular detail that appears contrary to the observed dust cycle is that month "9" of Ls 270 should have reduced dust MMR than months 6 or 12 (Montabone et al., 2015), but that is not the case.

As we mainly focus on large-scale features within the Martian climate, conditions do not vary significantly inter-annually (as mentioned in line 542). As such, the climate is fairly idealised in its current state. This is important to note, as the dust in these simulations seems to be primarily

driven by increasing temperatures during perihelion. Thus, when in month 9, temperatures are at their highest, dust abundance is also at its highest. In reality, however, the dust is affected by processes omitted currently in these simulations, which might be the cause of the 'dip' in dust content during month 9. We believe that the presented findings give an insight into how dust would behave in the absence of rapid vertical uplifting specifically from dust devils, or dust sequestering from freezing CO_2 and H_2O in the NH. There is also the influence of varying starting conditions at the start of the MY, where differences in initial conditions will affect the climate of the year. While in the UM in the context of this study, we solely focus on the average of initial conditions to simulate an average MY, based on those conditions.
This does emphasise an excellent point about the current state of the setup, however, as Montabone et al. (2015) highlights that there is substantial fluctuation in dust column optical depth alone (in addition to many other conditions in the Martian climate) across multiple MYs. Currently, the UM does not vary inter-annually, which means that, even in an idealised dry configuration, there is still potential work to be done regarding simulating subsequent MYs accurately. In future, after the proposed future implementation of schemes needed to capture the "wet" processes, the UM could potentially be set up to simulate specific years with prescribed initial values, as is done by Montabone et al. (2015). A paragraph detailing this has been added near the end of the discussion (line 617 in the revised manuscript).

III.) Is there any sensitivity to model resolution (predominantly horizontal but vertical as well)?

Zhou et al. (2022) suggests that lower resolutions predominantly affect the vertical distribution, but also (to a weaker extent) the spatial distribution in the horizontal. Although the 90x144 resolution was chosen for this study, the UM can easily be configured to higher resolutions. This was chosen to be similar to the Mars Climate Database resolution (49x64), but provide a higher resolution in fields such as orography. Increasing the resolution would change/improve the orography field, as it is currently regridded to a 90x144 resolution, increasing the resolution would increase elevation accuracy and provide better representation in the future. This would very much be an interesting topic to investigate, once the UM Mars configuration features more climate processes. A sentence has been added to the end of the discussion to highlight that the UM could be used in future work at a higher resolution.

IV.) The results and discussion focused on the zonal-mean structure. This is an excellent way to put the bulk climate into context but is not the full picture. As the goal is a comparison of the UM MCGM to the PCM, at least some investigation of the non-zonal mean structure is warranted. The addition of at least a few plots showing the column optical depth for each season on a lat/lon figure would show that dust is being transported within the circulation in a realistic way. Similarly, plots of the surface temperature and pressure would demonstrate how poorly or how well the model manages to capture the true climate without a CO2 cycle (Major comment I).

We thank the reviewer for their comment. A dust/wind plot has been provided for $\sigma = 0.99$ as in figure 11, we have also added surface temperature and pressure in figures 12 and 6, respectively. We have also added more discussion about the near-surface and surface conditions in the respective month sections and in sections 4.3.2 (at the end) & 4.3.3 (in the second paragraph).

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