1 Response to Reviewer 1

We thank the reviewer for the insightful comments and particularly for bringing the literature we were not aware of to our attention. To acknowledge these earlier studies, we reformulate and enhance the beginning of our introduction to specify the research gap we are addressing in view of the existing studies.

Below are the reviewer's comments in black and our answers to the comments including the changes we propose for the manuscript in red.

1.1 General comments

The authors explores the response of top of atmosphere energy balance to varied rotation rates in an aquaplanet with prescribed SST. They find that radiative forcing behaves non-monotonically with rotation rate. The sensitivity of radiative forcing to rotation rate is dominated by the shortwave cloud radiative effect.

The paper sheds new insight into the top of atmosphere energy balance response to varied rotation rates as previous papers have not performed a detailed decomposition of radiative effects into clear vs cloudy sky and shortwave vs longwave components. However, the current version of the manuscript does not adequately acknowledge relevant literature on this topic. For example, Williams et al. (2024) found a similar non-monotonic dependence on cloudiness (as measured by albedo in their paper, see their Fig. 4). Top of atmosphere energy flux is considered to a limited degree (see their Fig. 5). Therefore, I believe it is not accurate to state that "the role of the Coriolis force for a planet's radiant energy budget has not been explicitly addressed, so far" as the authors have in line 29-30.

Additionally, the authors' claim that "the magnitude of such [the effect of humidity and consequently the clear-sky outgoing longwave radiation caused by changes in the Coriolis force] on the TOA radiant energy budget has not been studied before" seems to be inaccurate given discussions of similar concepts in Guzewich et al. (2020) and Haqq-Misra et al. (2018).

I recommend the authors review these papers and the cited literature therein to more clearly identify the unique contribution that their work adds to the existing literature on this topic. I believe their results are useful and interesting; my remark is simply on the matter of accurately acknowledging similar work in the literature and synthesizing how the authors' findings fit with existing results.

Williams, D. A., X. Ji , P. Corlies, and J. M. Lora, 2024: Clouds and seasonality on terrestrial planets with varying rotation rates. Astrophys. J., 963, 36, https://doi.org/10.3847/1538-4357/ad192f.

Guzewich, S. D., J. Lustig-Yaeger, C. E. Davis, R. K. Kopparapu, M. J. Way, and V. S. Meadows, 2020: The Impact of Planetary Rotation Rate on the Reflectance and Thermal Emission Spectrum of Terrestrial Exoplanets around Sunlike Stars. ApJ, 893, 140, https://doi.org/10.3847/1538-4357/ab83ec.

Haqq-Misra, J., E. T. Wolf, M. Joshi, X. Zhang, and R. K. Kopparapu, 2018: Demarcating Circulation Regimes of Synchronously Rotating Terrestrial Planets within the Habitable Zone. ApJ, 852, 67, https://doi.org/10.3847/1538-4357/aa9f1f.

We propose to reformulate the introduction as follows:

Line 31: ”Previous studies have discussed changes in radiative properties and top-of-atmosphere (TOA) radiant energy fluxes caused by rotation rate changes in different contexts. Williams et al. (2024) analyses TOA energy flux at 23°N to discuss cloud seasonality, and Haqq-Misra et al. (2018) discuss thermal emission at the equator. Both studies highlight the importance of high clouds for TOA radiant energy fluxes in comparison to clear sky fluxes, but only at single tropical latitudes for slower than Earth-like rotation cases. Also for slowly rotating planets Guzewich et al. (2020) and Way et al. (2018) analyse global averages of cloud radiative effects and water vapour, but emphasise the importance of water vapour changes for the surface temperature response to rotation rate changes. So both the clear-sky and cloud radiative effects have been found to be important in previous studies of slowly rotating planets. However, their contribution to the global TOA radiant energy budget remains unclear. This is even more the case for faster than Earth-like rotation rates. For this reason, unlike previous studies, we focus mainly on the rotation durations < 32 Earth days, because changes in the Coriolis force in this range can alter both the clear-sky and cloud contributions to the total TOA radiant energy budget of the planet. This is important because changes in the TOA radiant energy budget can potentially affect the habitability of Earth-like rotating planets.”

Line 59: ”As mentioned before, such effects have been shown to affect climate feedbacks at slow rotation rates (e.g., Way et al., 2018; Guzewich et al., 2020) but their effect on the global mean radiant energy budget wasn’t discussed.”

We also acknowledge Williams et al. (2024) on the non-monotonic dependence of cloud changes, and the suggested changes to the manuscript are included in the response to the second reviewer’s general comment.

1.2 Specific comments

Line 85: “the model becomes unstable for even faster rotations” Can the authors briefly explain the source of the instability? Does it arise from a specific parameterization scheme that breaks down at parameter spaces far away from where it was designed to be used? Any insight into this would be helpful for readers who are interested in pushing models to extreme limits.

It is generally difficult to unambiguously identify the source of the instability. The model often crashes when variables exceed the range of lookup tables from parameterisations. However this is in general only the symptom, not the source of the problem. When moving to faster rotation rates, small-scale circulation features with high wind speeds > 250 m/s can occur very close to the surface, leading to instabilities and and lookup table violations in the convection routine. Increasing the model resolution to better resolve the small-scale atmospheric features and decreasing the integration time step to avoid the CFL violation may help to handle such instabilities. For the purpose of our analysis, we choose to limit the range to $\Omega/\Omega_e = 8$, and accordingly we are able to explain the changes in the eddies and their importance for the radiant energy budget using this range. Therefore, we did not find it necessary to simulate even faster rotation cases.

In our manuscript, we will address it as "the model becomes unstable for even faster rotations. Increasing the model resolution and decreasing the integration time step may help to handle such instabilities. Nevertheless, we limit our analysis to $\Omega/\Omega_e = 8$ and we are also able to explain the importance of the eddies for the radiant energy budget already using this range."

Line 132, 161, 198, 204: "(not shown)" What are the reasons for not showing these results? I understand if the authors prefer to not show the result if they plan on publishing these results in a separate paper. Otherwise, these should be provided in a supplement.

We do not plan to have a separate paper but rather wanted to keep our manuscript concise and focus on figures that we thought are most important to explain the main story of the paper. However, as specified below, we will now include some of those figures in the Supplement:

Line 132: We do not show the poleward shift of the midlatitude jets, as this has been discussed in detail in previous studies (Del Genio and Suozzo, 1987; Navarra and Boccaletti, 2002; Kaspi and Showman, 2015). We think it is just important to indicate that our simulations are consistent with these previous studies.

Line 161: The eddy momentum flux (Figure 1) will be added to the supplement. It is consistent with the poleward eddy heat flux.

Line 198: The increased convective activity due to decreasing stability can be seen from the increase in convective precipitation in the high latitudes (Figure 2a). The large scale precipitation however reduces over that region (Figure 2b). This will also be added to the Supplement.

Line 204: The SW cloud radiative effect and total cloud fraction of the intermediate experiments (between $\Omega/\Omega_e = 1/4$ and $\Omega/\Omega_e = 1/16$) are shown in Figure 3. This will also be added to the supplement.

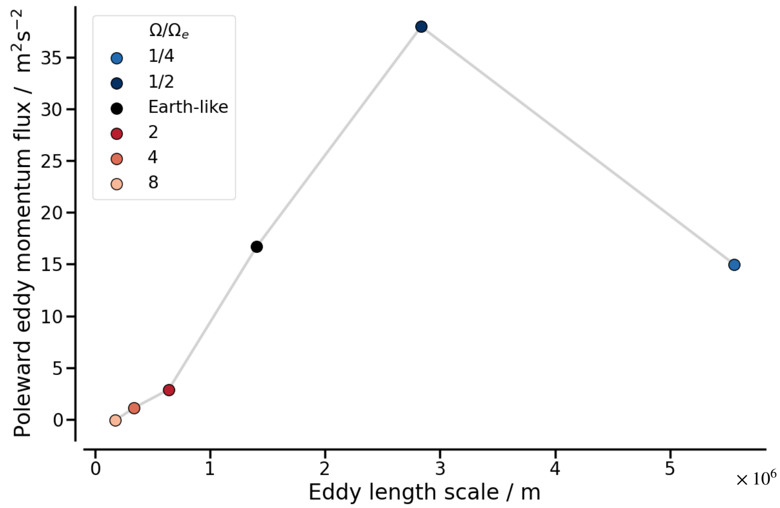

Figure 1: Poleward eddy momentum flux vs eddy length scale for different rotation rates.

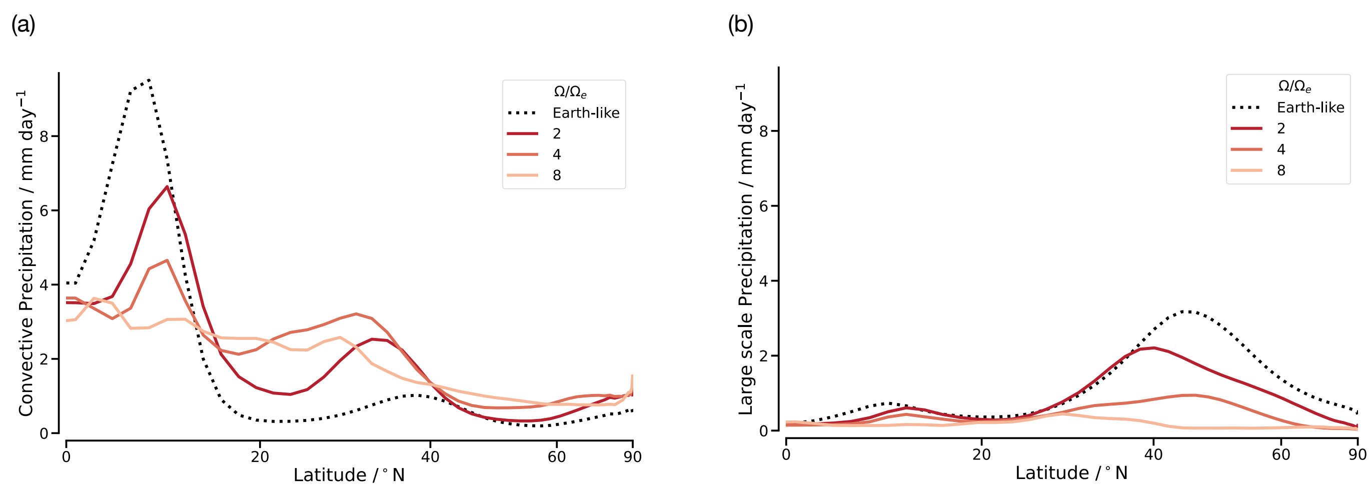

Figure 2: Zonal mean of (a) convective and (b) large-scale precipitation for rotation rates from Earth-like to $\Omega/\Omega_e = 8$

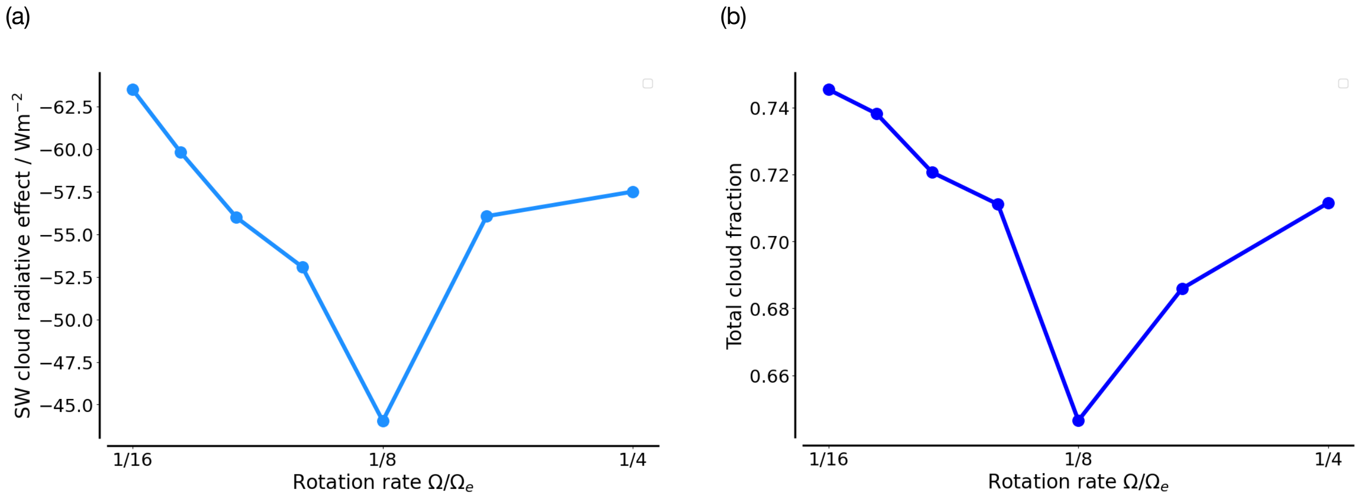

Figure 3: Global mean of (a) SW cloud radiative effect and (b) total cloud fraction for rotation rates between $\Omega/\Omega_e = 1/4$ and $\Omega/\Omega_e = 1/16$.

Line 145: "The cell becomes more slanted at $\Omega/\Omega_e = 8$" Can the authors be more precise about how slantedness is measured? It is not clear to me from looking at the stream function contours in Fig. 1 that the Hadley cell is more slanted for $\Omega/\Omega_e = 8$ compared to 4.

We agree with the reviewer that the slantedness of the Hadley cell is not easy to identify from the stream function contours for $\Omega/\Omega_e = 8$ shown in Figure. 1 of the manuscript, while it is obvious for the slower rotation case $\Omega/\Omega_e = 1/4$. We measure the extent of the Hadley cell as the latitude where the stream function reverses sign. The reversal of the stream function occurs at the same latitude for the upper and lower troposphere for $\Omega/\Omega_e = 4$ but not for $\Omega/\Omega_e = 8$. This is what we call slantedness. We minimise this effect when determining the extent of the Hadley cell by averaging the stream function between 400 and 700 hPa. In an averaged stream function between 400 and 700 hPa, $\Omega/\Omega_e = 4$ and $\Omega/\Omega_e = 8$ appear to have the same latitudinal extent of the Hadley cell, but not at single pressure levels of 400 hPa or 700 hPa. However, based on the comment, we understand that using the word 'slantedness' for the Hadley cell can be misleading. Further, it is not important for the energy budget. So, we propose to remove the word slantedness and make changes in the manuscript as below.

Line 137: "For slower ($\Omega/\Omega_e < 1/2$) and faster ($\Omega/\Omega_e = 8$) rotations, the latitudinal extent of the Hadley cell differs in the upper and lower troposphere, motivating the use of the mean mass stream function between 400 and 700 hPa as a metric."

Line 145: "Figure 2a (in the manuscript) also shows that the faster rotations, $\Omega/\Omega_e = 4$ and $\Omega/\Omega_e = 8$, seem to have similar extents in the averaged stream function between 400 and 700 hPa. There is no hard limiting factor (also not from the theories) that the Hadley cell no longer shrinks for rotations faster than $\Omega/\Omega_e > 4$. Therefore, similar extents may be an artefact of averaging between two pressure levels. The Hadley cell, however, becomes weaker at $\Omega/\Omega_e = 8$ than at $\Omega/\Omega_e = 4$."

Line 159-160: "This is similar to Wang et al. (2018) except that we use 500 hPa instead of 800 hPa." What is the motivation behind evaluating eddy heat flux at 500 hPa vs 800 hPa? Are the results consistent when evaluated at 800 hPa?

We would like to be consistent with other free-tropospheric parameters that we show for 500 hPa such as temperature. The results are consistent even when the eddy heat flux is computed at 850 hPa which is shown in Figure 4 (800 hPa model output is not available and so we used 850 hPa). In the manuscript, we will change it as below.

Line 159-160:"This is similar to Wang et al. (2018) except that we use 500 hPa instead of 800 hPa. The results are, however, consistent at lower tropospheric pressure levels."

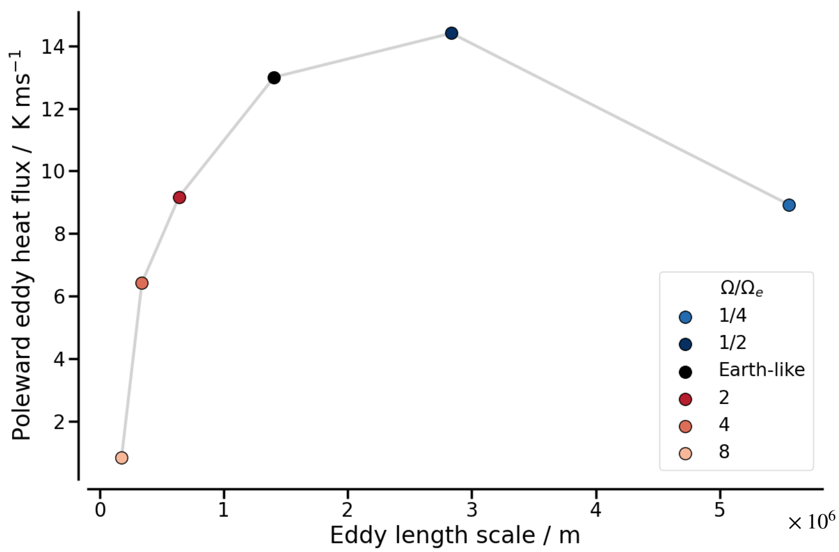

Figure 4: Eddy heat flux at 850 hPa vs eddy length scale for different rotation rates.

Line 182: "the meridional temperature gradient associated to the baroclinic instability is reduced" Is this because as the Rossby deformation radius decreases with increasing rotation rates, the latitudinal extent of the baroclinic zone shrinks? Whether this is the correct interpretation or not it would be helpful to explain this explicitly. It would also be useful if the explanation is illustrated in Fig. 3a (e.g., the latitudinal extent of the baroclinic zones could be indicated as points on the line).

We agree that Line 182 is not very clear. Therefore, we reformulate the whole section 3.2.1 (see our reply to the comment concerning Line 183-184).

The Rossby radius of deformation decreases at all latitudes for increasing rotation rate. But we choose the baroclinic zone as the latitudinal extent where the poleward eddy heat flux is greater than 30 % of the maximum value. We mark the edge of this zone by dots in the temperature at 500 hPa (Figure 5; will become the new Figure 3a of the manuscript).

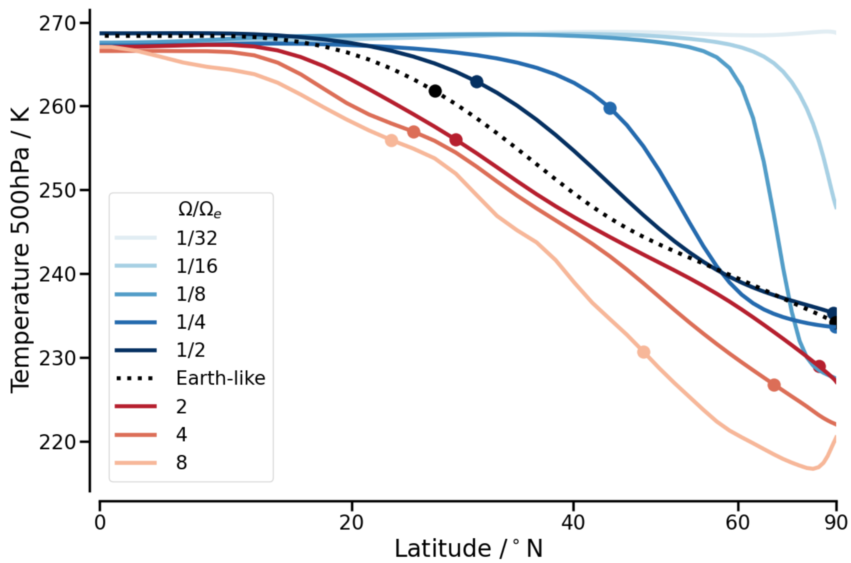

Figure 5: Zonal mean temperature at 500 hPa for different rotation rates and the latitudes between the two dots for $\Omega/\Omega < 1/8$ are the baroclinic zones which are the regions >30 % of the maximum eddy heat flux.

Line 183-184: "This leads to a weakening of the vertical wind shear with increasing rotation rate." Is this supported by any results or figures in the manuscript? If not I recommend including a plot supporting this in a supplement.

The difference in the zonal wind between 250 hPa and 850 hPa is calculated as the vertical shear of the zonal wind and is shown in Figure 6. This will be added to the supplement. We show only the zonal wind component as to discuss the meridional temperature gradient. Figure 6 shows the decrease in vertical shear from $\Omega/\Omega_e = 1/4$ to Earth-like and even faster rotations. From the thermal wind balance (equation 1) we can deduce that the weakening of the zonal wind shear is mainly due to the increase in the Coriolis parameter, which is much stronger than the changes in the meridional temperature gradient (Figure 3a in the manuscript).

$$\frac{\partial u}{\partial p} = \frac{-R}{pf}\left(\frac{\partial T}{\partial y}\right)_p \tag{1}$$

where $\frac{\partial u}{\partial p}$ is the vertical shear of the zonal wind, f is the Coriolis parameter, $\left(\frac{\partial T}{\partial y}\right)_p$ is the meridional temperature gradient.

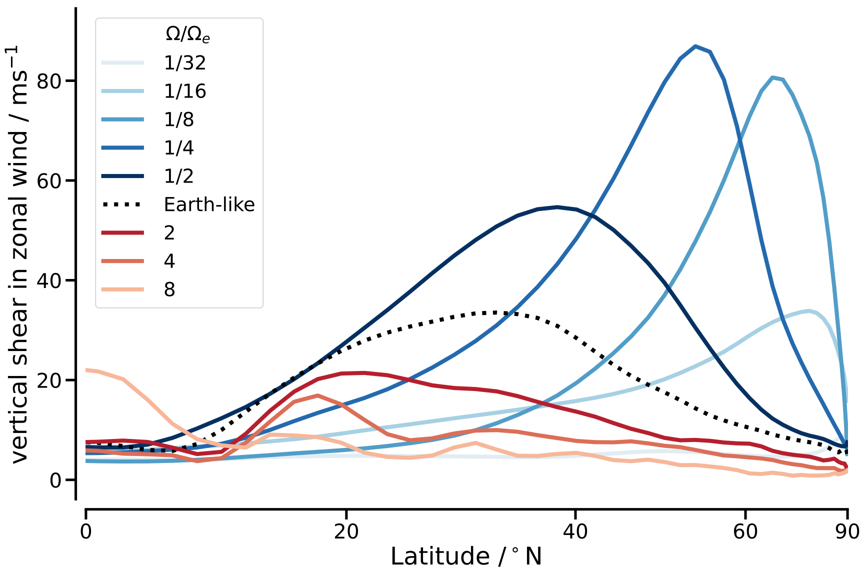

Figure 6: Zonal mean of the vertical shear of the zonal wind between 250 hPa and 850 hPa at different rotation rates.

In the manuscript, we will reformulate Section 3.2.1 including the lines 182-184: "When the Coriolis force is weaker than Earth-like, gravity waves are efficient in smoothing out the temperature differences over a wider range of latitudes. Therefore, the free tropospheric temperature profile is similar to the equatorial temperature profile over a wider range of latitudes for slower rotation cases. This expansion of the WTG region shifts the strong meridional temperature gradient poleward, and with it the baroclinic eddy activity. This is indicated by the meridional temperature profile at 500 hPa shown in Fig. 3a. The transition from the subglobal to the global Hadley cell occurs at $\Omega/\Omega_e = 1/8$, and the meridional temperature gradient disappears completely at $\Omega/\Omega_e = 1/32$, i.e., the baroclinic eddy activity becomes negligible at $\Omega/\Omega_e = 1/8$ and disappears completely at $\Omega/\Omega_e = 1/32$. In the other direction for faster rotation than Earth-like, the WTG region becomes narrower. However changes in the strength of the meridional temperature gradient are small (Figure 3a). With minimal or no change in the meridional temperature gradient, the thermal wind balance suggests that the increase in the Coriolis parameter from $\Omega/\Omega_e = 2$ to 8 decreases the vertical wind shear (see Supplement). Weaker vertical wind shear leads to weaker eddies, weaker storms, and reduced meridional energy transport (e.g., Schneider and Walker, 2006; Kaspi and Showman, 2015). The increasing temperature difference from $\Omega/\Omega_e = 1/4$ to 8 between the equator and the poles is a consequence of this."

Fig. 3a: Why does temperature increase with latitude very close to the pole in the $\Omega/\Omega_e =$ 8experiment?

We speculate that this is related to the appearance of a narrow fourth circulation cell poleward of the original polar cell. However, we think that this feature is not very important for the energy budget and would therefore prefer not to speculate in the manuscript.

Line 194-195: “the lapse rate is close to a moist adiabat over a wider range of latitudes” Is this supported by any results or figures in the manuscript? If not I recommend including a plot supporting this in a supplement.

We think that our current formulation is insufficient and can be misunderstood. The free tropospheric temperature profile at the equator closely follows the moist adiabatic lapse rate. For slower rotations, gravity waves are efficient in smoothing out temperature differences over a wider range of latitudes than for Earth-like rotation. Therefore, the free tropospheric temperature profile is similar to the equatorial temperature profile over a wider range of latitudes for slower rotation cases. Figure 3a in the manuscript supports our argument of a more uniform free tropospheric temperature over a wider range of latitudes for slower rotation cases.

In the manuscript, we will rephrase it accordingly in two places. Line 178: ”When the Coriolis force is weaker than Earth-like, gravity waves are efficient in smoothing out the temperature differences over a wider range of latitudes. Therefore, the free tropospheric temperature profile is similar to the equatorial temperature profile over a wider range of latitudes for slower rotation cases.” Line 194-195: ”Due to the extension of the WTG region, the free troposphere is warmer over a wider range of latitudes at slower rotations (Figure 3a).”

Line 194-195: Line 258-259: “the synoptic-scale storm systems in our simulations become smaller, but more numerous and frequent” Can the authors show results from their simulation that support the claim that synoptic-scale storms become more numerous and frequent with increasing rotation rate?

Figure 7 shows snapshots of the surface pressure for Earth-like and faster rotation rates. The alternating high and low pressure regions visible for latitudes > 30 degrees in the Earth-like rotation case become smaller with increasing rotation rate. This suggests that the storms related to those pressure systems will also be smaller and more numerous with increasing rotation rates. This figure will also be added to the Supplement.

Line 261-262: “the reduction in storm-track cloudiness corresponds well with the reduction in vertical wind variance” Vertical velocity variance changes do not seem to fully explain cloudiness changes to my eyes. For example there is a monotonic decrease in vertical velocity variance from $\Omega/\Omega_e = 2$ to 8 but cloudiness changes are complex. There is a decrease in low clouds but an increase in clouds at 400 hPa around 45 deg N for $\Omega/\Omega_e = 8$. Is vertical velocity variance changes expected to be more consistent with extratropical low clouds more than high clouds? A more nuanced discussion seems warranted here.

We agree with the reviewer that vertical velocity variance does not fully explain the changes in cloudiness associated with storm systems. There are two modes of cloudiness associated with the storm systems: a) convective and b) stratiform. The convective mode is usually deep and more localised, while the stratiform mode is widespread in the middle and lower

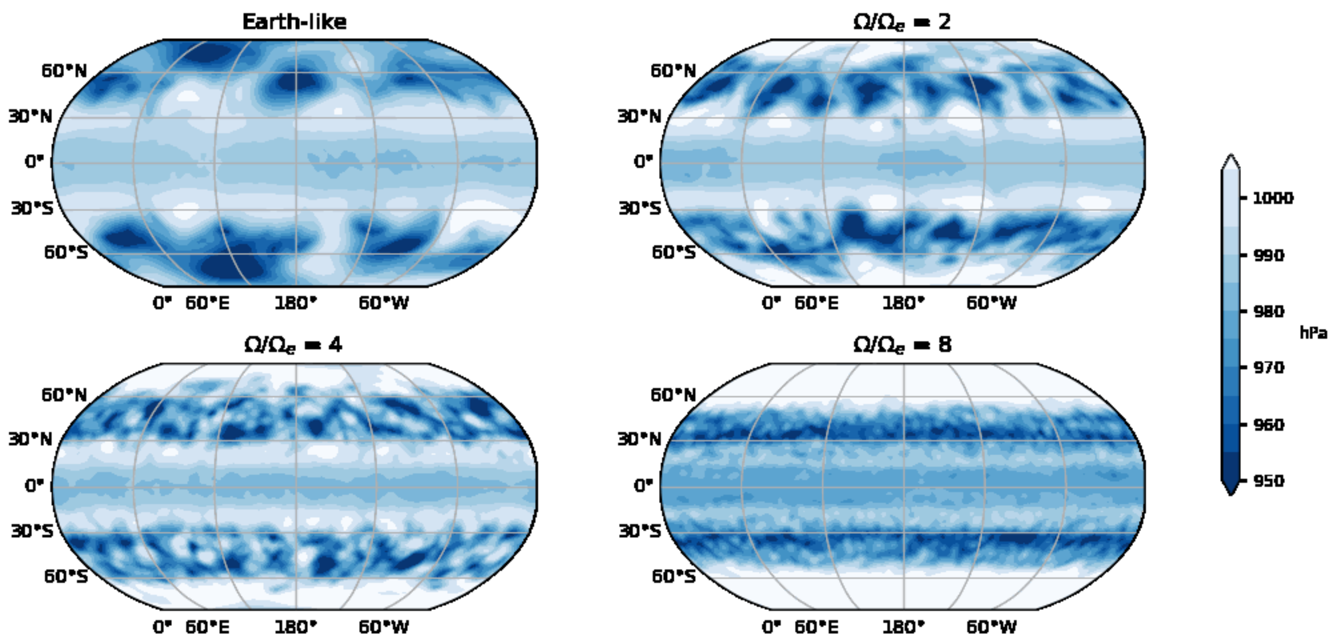

Figure 7: Snapshot of surface pressure for Earth-like and faster cases

levels of the troposphere (Houze Jr, 2004). It is the latter that decreases with decreasing Rossby radius of deformation, and hence the low and middle level clouds and also the large scale precipitation (Figure 2b). Our original comment intended to refer to the low and middle level clouds of the storm systems, but we failed to specify this in the text. We will add this specification. The high clouds < 400 hPa expand meridionally at mid and high latitudes for increasingly faster rotations, but become thinner. This expansion is rather due to the decreasing stability of the troposphere (Figure 3b in the manuscript), which allows convection to occur frequently over a wider range of latitudes at faster rotations. This view is supported by the increase in convective precipitation (Figure 2a) over mid and high latitudes at faster rotations.

In the manuscript, we add the following explanation for the high clouds after explaining the mid and low level clouds from Line 259: "There are two types of cloudiness associated with mid-latitude storm systems: a) convective and b) stratiform. The convective mode tends to be deep and more localised, while the stratiform mode is widespread in the middle and lower levels of the troposphere (Houze Jr, 2004). The cloudiness of the storm systems, in general, on the poleward flank of the Ferrel cell is positively correlated with the variance of the vertical velocity (Datseris et al., 2022). The comparison of Figures 7 and 8 shows that for faster than Earth-like rotations, the reduction in storm-track cloudiness, in particular the low and middle level clouds, corresponds well with the reduction in vertical wind variance. This is because for increasingly faster rotations, the geostrophic balance leads to increasingly weaker winds due to the increasing Coriolis parameter. Together with the smaller eddy length scale, the weaker winds result in smaller and weaker synoptic-scale storm systems. Consequently, the stratiform i.e., the mid and low-level cloudiness associated with storm systems is reduced at faster rotations. However, the reduction in vertical wind variance does not explain the expansion of high clouds at faster rotations. At increasingly faster rotations, the tropospheric stability decreases (Figure 3b) and the convective precipitation increases over mid and high latitudes (see Figure. 2a). Both together suggest that the expansion of high clouds is more likely due to convective activity extending to high latitudes. As the latitudinal extent of the unstable atmosphere increases, the convective activity becomes more localised, resulting in a greater number of local high clouds spread over a larger area."

Fig. 9 and 10: Would it not make more sense to move Fig. 10d as a new panel 9c? These panels are discussed together in line 276.

We agree that Fig. 10d fits nicely to Figs. 9a and 9b, and we will move it there. Originally, we had added it to Fig. 10, because it is first discussed in the context of Fig. 10c

Line 293-294: "At faster than Earth-like rotations, the extent of high clouds increases, but the cloud fraction decreases." As I indicated in my comment above, there are some regions where high clouds increases with increasing rotation rate. It would be helpful to show a height and spatially averaged metric of high cloud fraction similar to Fig. 10c to support this statement.

We agree: There are some regions where high clouds increase with increasing rotation rate, and the description and explanation for the high-cloud effects were misleading. Therefore, we would like to rephrase Lines 293-294 as below.

"At faster than Earth-like rotations, the extent of the high clouds expands meridionally due to convective activities over mid and high latitudes (discussed above). However, near the equator, the high cloud fraction decreases due to the weakening of the Hadley cell. Both these effects result in a more uniform distribution of the high clouds in the zonal mean and hence more zonally uniform LWCRE. This is similar to zonally uniform column relative humidity for rotation rates around $\Omega/\Omega_e = 8$."

Line 294-295: "This is due to weakening circulation and weakening convective updrafts at all latitudes" This contradicts the statement made in line 198: "The larger instability for faster rotation leads to increased convective activity and increased convective precipitation at higher latitudes". Can you reconcile this contradiction?

Indeed, this sounds contradictory. The evidence of increased convective precipitation for faster rotation supports the statement made in Line 198. We suggest to remove the sentence from Lines 294-295 "This is due to weakening circulation and weakening convective updrafts at all latitudes". We think that the paragraph without this sentence and with the changes made in response to previous comments is more convincingly describing and explaining the high-cloud changes.

1.3 Technical comments

Line 107: "SSTs, enables" → "SSTs enables"

Line 130: "mid-latitute" → "mid-latitude"

Line 157: "wind, θ" → "wind and θ"

All figures: Can the authors make the scientific notation labels on colorbars and axes larger? They are currently very small and difficult to catch.

Line 209: "LW cloud radiative effect varies (LWCRE)" → "LW cloud radiative effect (LWCRE) varies"

Line 221: "regions, where" → "regions where"

We corrected the all technical comments accordingly in the manuscript.

2 Response to Reviewer 2

We thank the reviewer for the insightful comments. Below are the reviewer's comments marked in black and our answers to the comments including the changes we propose for the manuscript in red.

2.1 General comments

The authors discuss the relevance of the Coriolis force for the habitable zone by varying Earth's rotation rate and analyzing its impact on the radiation energy budget. This study expands on previous research that focused solely on the Hadley cell by also considering the effects of storminess. However, I agree with the other reviewer that the paper's major weakness is the lack of a comprehensive review of previous studies and a discussion of how this research aligns or contrasts with those findings. I recommend that the authors conduct a thorough review of the relevant literature and include a dedicated section discussing how their findings compare to previous studies.

We agree that we have missed some relevant previous publications. We suggest to include them in the introduction as indicated in the response to the first reviewer's comments. In addition, we suggest to put our results into the perspective of these earlier publications in the respective sections where they are discussed and as specified below.

Line 243: "The changes in humidity with rotation rate are supported by previous studies using different experimental setups. Way et al. (2018) and Guzewich et al. (2020) identified the decrease in column water vapour as an important reason for the surface cooling for reduced rotation rates, both in setups with a dynamical ocean and the presence of land. However, in simulations where SSTs are allowed to adjust, the clear-sky OLR response would not only be determined by changes of atmospheric water vapour and temperature but also by SST feedbacks. This is likely why in the case of Liu et al. (2017), e.g., who analyse rotation rates similar to ours, but in a slab ocean setup, the zonal mean clear-sky OLR generally increases with the rotation rate (their Figs. 8c and 9c), except for latitudes poleward of about 50 °. In our case, this is true only near the equator (Figure 6b)."

Line 298: "The changes in the zonal mean cloud radiative effects, both LW and SW, are similar to those of Liu et al. (2017). Both effects decrease from slower to faster rotation near the equator, providing a robust response to Hadley cell changes with rotation rate. However, the changes in storm systems and associated cloud effects are not apparent in Liu et al. (2017). In the case of storm systems, Williams et al. (2024) reported a similar non-monotony in the SWCRE using a slab ocean setup, with the hypothesis that neither the mean circulation nor the baroclinic eddies dominate cloud formation at $\Omega/\Omega_e = 1/8$. Our reasoning is in part similar to that, but we attribute the drop at $\Omega/\Omega_e = 1/8$ mainly due to the disappearance of the storm systems, which in our simulations is related to the fact that the Rossby radius of deformation reaches the size of the planet at $\Omega/\Omega_e = 1/8$ (Figure 2b)."

Line 331: "Indeed, the decrease in humidity at slower rotations and the drop in SWCRE at $\Omega/\Omega_e = 1/8$ have been identified in simulations by Way et al. (2018) and Williams et al. (2024), respectively".

2.2 Specific comments

Line 200: How is "effective forcing" defined? Are the values in Figures 4 and 5 global averages?

The "effective forcing" has already been defined in the Experimental setup as "the change in the net radiative flux at the TOA after adjustments of atmospheric temperatures, humidity and clouds to the change of the rotation rate". Also the values in Figures 4 and 5 (in the manuscript) are global averages which is mentioned in the Figure captions. To avoid confusion we add the specification 'global mean' in the paragraphs where these figures are discussed.

Lines 217-220: The change in clear-sky OLR differs from Liu et al. (2017) (see Figs. 8c and 9c). Can you provide an explanation? Additionally, the temperature response differs between the two studies, although one shows surface temperature (Fig. 3 of Liu et al., 2017) and the other shows temperature at 500K (Fig. 3a). Can the authors explain why the temperature in this study behaves this way with increasing rotation rate? The authors said the decrease in temperature is due to reduced baroclinicity, but it is actually energy transport, not baroclinicity itself, that determines extratropical temperature. In fact, Fig. 3a shows that the temperature gradient increases with increasing rotation rate, which should enhance energy transport, compensating for the decrease associated with reduced baroclinicity.

Yes, we can provide an explanation for the difference in the clear-sky OLR response and why the temperature behaves as it does between Liu et al. (2017) and our work. We didn't claim in the original manuscript that 'the decrease in temperature is due to reduced baroclinicity'. Instead, we wrote 'The meridional temperature gradient associated with baroclinic instability is reduced' in line 182, 'The increasing temperature difference between the equator and the poles and the colder higher latitudes is due to the weaker circulation, in particular the weaker eddies, which are less efficient in meridional energy transport' in line 184, and 'Temperatures at mid and high latitudes are also reduced due to inefficient heat transport by weaker and smaller baroclinic eddies' in line 240. Line 182 is misleading and we suggest to change it according to the first reviewer's comment. To answer the reviewer's question, we provide an explanation for the energy transport below.

It is difficult to directly compare the clear-sky OLR from our simulations with those from Liu et al. (2017). This is because we are analysing the LW forcing resulting from a change of the rotation rate using simulations with fixed SST while Liu et al. (2017) analyse the response of the OLR to the climate feedback resulting from a change in rotation rates, which, in particular, includes the effect of strong SST changes on the OLR. For instance, our original Fig. 6b shows a reduction of clear-sky OLR for faster rotation almost globally (except for close to the equator), while Figs. 8c and 9c from Liu et al. (2017) show a much less clear rotation dependence in the extratropics. Likely, the increase in SST for faster rotation that is shown in their Fig. 1a compensates for parts of the forcing shown by our simulations. This reason is also likely to explain the difference in the slower rotation cases. The decrease in humidity in the subsidence region explains the increase in clear-sky OLR in our case, but such an increase is not seen in Fig. 9c of Liu et al. (2017). It is rather reduced globally, likely following the decrease in SST for slower rotation also shown in their Fig. 1a.

In the manuscript, we mainly highlight the difference in the clear-sky OLR between Liu et al. (2017) and our study, which is included in the response to the general comment above.

This difference in the goals of the studies and the setup of the simulations, with fixed SSTs only in our case, explains also why we analyse the temperature changes at 500 hPa.

We rephrase line 182 "The meridional temperature gradient associated with baroclinic instability is reduced" in response to reviewer 1. The other two lines 184-186 and line 240 discuss the reduced energy transport at faster rotations and are based on what we inferred from previous studies (e.g., Kaspi and Showman, 2015; Liu et al., 2017; Navarra and Boccaletti, 2002).The decrease in poleward energy transport at faster rotations is mainly due to a decrease in the eddy length scale (Kaspi and Showman, 2015) and not to a decrease in baroclinicity. The decrease in the eddy length scale results in smaller eddies and narrower baroclinic zones, and hence reduced poleward energy transport, whereas the baroclinicity actually increases with the increase in the Coriolis parameter at faster rotations. Figure 2b in the original manuscript, which shows poleward eddy heat flux vs eddy length scale for different rotation rates, supports our argument for reduced poleward energy transport at faster rotations. Associated with it are the colder middle and high latitudes and the increased equator-pole temperature contrast. We agree with the reviewer that an increase in the equator-pole temperature contrast alone would increase poleward energy transport if the eddies remained the same. However, as our Fig. 2b shows, the eddy heat flux is actually reduced for faster than Earth-like rotations which lets us conclude that the increased temperature difference between high and low latitudes is the consequence of the reduced heat flux and potential feedbacks on it are not dominating.

Line 270: I cannot derive the number 2/3 from Fig. 4.

It's true, the number 2/3 isn't easy to see from the plots. It stemmed from a comparison of regression line slopes which were not shown. To make the quantification traceable, instead of the sentence on the original Line 270, we propose to include the following text: "The effective forcing differs by about 63 Wm^{-2} between the slowest and fastest rotation cases. The difference in the SW cloud radiative effect is 47 Wm^{-2}, i.e., it contributes almost 3/4 to the effective forcing. The remaining contribution is mainly provided by the clear-sky OLR.

Figure 9: This figure could be compared with Figs. 8c and 9c of Liu et al. (2017). Can the authors give a comparison?

Figure 9 in our manuscript shows the cloud radiative effects, and Figures 8c and 9c in Liu et al. (2017) show the clear-sky OLR for faster and slower rotations, respectively. So we are not sure if the reviewer asked to compare the cloud radiative effects or the clear-sky OLR. We have compared the clear-sky OLR with Liu et al. (2017) in response to a comment above. Here is a comparison of the cloud radiative effects.

Similar to the clear-sky OLR, the cloud radiative effects of Liu et al. (2017), both SW and LW (Figs. 8b and 8d, and 9b and 9d), are not directly comparable to the effects shown in our original Figs. 9a and b, but there are similarities. Both studies show that both the SW and LW effects decrease near the equator from slower to faster rotation. However, changes in the SW and LW cloud effects associated with the mid-latitude storm systems can not be clearly identified in Liu et al. (2017).

In the manuscript, we discuss the similarity as the robust response to Hadley cell changes with the rotation rate. Our changes are included in the response to the general comment above.

Line 280: The explanation does not clarify why, at the same latitude where insolation remains constant, the SWCRE decreases with increasing rotation rate.

The decrease of the SWCRE with increasing rotation rate is discussed in lines 270 to 277. However, the line starting at line 276 was misleading and we changed it to: "This [the dominance of the storm tracks for the reduction] can be seen from Figures 9c and a which show that the zonal mean low cloud fraction and SWCRE are reduced for faster rotation mainly between 35° and 60°. Although the storm systems shift towards the equator with increasing rotation (Fig. 9c), the reduction in the low-level cloud fraction dominates the higher insolation at lower latitudes such that the SWCRE is strongly reduced with increasing rotation."

References

Datseris, G., Blanco, J., Hadas, O., Bony, S., Caballero, R., Kaspi, Y., and Stevens, B. (2022). Minimal Recipes for Global Cloudiness. *Geophysical Research Letters*, 49(20):e2022GL099678.

Del Genio, A. D. and Suozzo, R. J. (1987). A Comparative Study of Rapidly and Slowly Rotating Dynamical Regimes in a Terrestrial General Circulation Model. *Journal of Atmospheric Sciences*, 44(6):973–986.

Guzewich, S. D., Lustig-Yaeger, J., Davis, C. E., Kopparapu, R. K., Way, M. J., and Meadows, V. S. (2020). The impact of planetary rotation rate on the reflectance and thermal emission spectrum of terrestrial exoplanets around sunlike stars. *The Astrophysical Journal*, 893(2):140.

Haqq-Misra, J., Wolf, E. T., Joshi, M., Zhang, X., and Kopparapu, R. K. (2018). Demarcating circulation regimes of synchronously rotating terrestrial planets within the habitable zone. *The Astrophysical Journal*, 852(2):67.

Houze Jr, R. A. (2004). Mesoscale convective systems. *Reviews of Geophysics*, 42(4).

Kaspi, Y. and Showman, A. P. (2015). Atmospheric dynamics of terrestrial exoplanets over a wide range of orbital and atmospheric parameters. *The Astrophysical Journal*, 804(1):60.

Liu, X., Battisti, D. S., and Roe, G. H. (2017). The Effect of Cloud Cover on the Meridional Heat Transport: Lessons from Variable Rotation Experiments. *Journal of Climate*, 30(18):7465–7479.

Navarra, A. and Boccaletti, G. (2002). Numerical general circulation experiments of sensitivity to Earth rotation rate. *Climate Dynamics*, 19:467–483.

Wang, Y., Read, P. L., Tabataba-Vakili, F., and Young, R. M. (2018). Comparative terrestrial atmospheric circulation regimes in simplified global circulation models. Part I: From cyclostrophic super-rotation to geostrophic turbulence. *Quarterly Journal of the Royal Meteorological Society*, 144(717):2537–2557.

Way, M. J., Del Genio, A. D., Aleinov, I., Clune, T. L., Kelley, M., and Kiang, N. Y. (2018). Climates of warm Earth-like planets. i. 3d model simulations. *The Astrophysical Journal Supplement Series*, 239(2):24.

Williams, D. A., Ji, X., Corlies, P., and Lora, J. M. (2024). Clouds and Seasonality on Terrestrial Planets with Varying Rotation Rates. *The Astrophysical Journal*, 963(1):36.