

## 1 Reviewer 2 (RC2)

The authors would like to thank the reviewer for their kind words and their comments on this manuscript. We detail our response and how we will address each comment in the following section. We have included our responses in red.

### 1.1 General Comments

5 The authors investigate the age spectra of water vapor and precipitation, with regional resolution, and how these spectra change per degree of imposed climate warming. They developed a new method that works in an Eulerian framework. Their method produces similar global mean ages as existing Lagrangian methods. The article explains the method and presents simulation results for a reference period and for warmer periods, where a uniform SST warming is imposed. The authors describe the results and give some interpretation to the spatial structure in the reference climate and in the changes, based on changes in  
10 tropical upwelling, in the strength of the Hadley circulation, Brewer Dobson circulation, eddy mixing etc. Overall it is an interesting work that deserves publication after improvements. There are two important areas of concern:

- The description of the method is insufficient. The reader most likely cannot understand the evolution equation in the way it is presented. The authors must be able to present this better. (See my comments further below.)
- The simulations made for these studies are only mentioned on the side, but not explained. For example which years  
15 are covered by the 30 year AMIP simulation that is used as control simulation? How many simulations with increased temperatures were made and with which temperature increments? (One may guess from Figure 8.) Have greenhouse gas concentrations been increased to values reflecting the surface temperature increase prescribed by the SST boundary conditions so that also a stratospheric cooling exists and the radiative forcing on land surfaces can be captured? Is sea ice concentration adjusted in any way to the temperature increase? If not what does that mean for high latitude water  
20 vapor ages? Preventing loss of sea ice in the Arctic in warmer climates also means that new sources of water vapor are excluded. Etc. ...

The authors would like to thank the reviewer for their comments on this manuscript. We generally agree with the comments, have applied the recommended changes to make the derivation of the evolution equation and added details on the simulation parameters. We think that you have pointed out an interesting flaw in the presentation of our study.

25 First, we need to present the details of our tracer development and to more clearly present the configuration of the climate model we used.

30 Second, we need to emphasize, and rephrase certain sections to make it clear that our work is a proof of concept that Eulerian tracers of water vapor age, and although we can get very interesting results on the WV age distribution, and its evolution with climate change, these main conclusions are more qualitative than purely quantitative.

### 1.2 Specific Comments

#### 1.2.1 L81: ... an atmospheric parcel ...

How should we imagine this air parcel? It this assumed to be a macroscopic Lagrangian air parcel that follows a sort of larger  
35 scale flow, and hence is mixed by turbulent mixing of original microscopic air parcels moistened at the surface? Or is this an Eulerian air parcel fixed in space at a specific time, which naturally contains air with moisture from different sources?

It is true that in the current formulation, we do not specify that we are considering an Eulerian air parcel. Now, we present it as "... Now, if we consider Eulerian atmospheric parcel, fixed in space, it will have an age distribution associated with the  
40 different amounts of water vapor evaporated at different times. ". We applied these changes on line 82.

**1.2.2 L91: ... so that  $q_a da$  is the amount of WV with age between  $(a, a + da)$ , ...**

Why not you add for clarity also that  $\int q_a da = q$  and that  $q_a$  has units of kg/kg/s ?

That is a good point, and will help make this section clearer for the reader. We made the changes on line 97.

**45 1.2.3 L92: ... evaporation (E) ...**

for clarity it would be good to add : ... evaporation (E) from the surface or condensed water in the atmosphere (cloud water, rain, etc.) ... (assuming that such a process exists in the model)

**50 We agree that we could clarify that we mean evaporation and re-evaporation, although re-evaporation is turned off in our model. Changes have been applied on line 93.**

**1.3 L93: ... For WV injected into the system at time  $t_e$ , ... then at any given moment in time, the WV content associated with that age category is  $q(t_e = t - a)$  ...**

**55** But it seems like this depends on the assumption that no water vapor existed in the system earlier than  $t_e$ , and thus  $q(t < t_e) = 0$  and that  $q$  is the total specific humidity of a closed system. This should be clarified. (I imagined first that here  $q = q(\lambda, \phi, p, t)$  from a 3d atmosphere with advection, diffusion, evaporation, clouds etc., but then the statement on L.95 is not correct).

In this sentence, we refer to the WV that gets created at time  $t_e$ , so before  $t_e$ , no amount of this WV existed. Nonetheless, we agree that we can make this sentence clearer. Changes were applied around line 95.

**1.4 Eq.3:  $q_a(t) da = q(t, t_e = t - a) dt$ .**

**60** Writing  $q(\cdot)$  on the right side with two time arguments is a kind of confusing. It is better to add the definition  $t_e = t - a$  separately, like:  $q_a(t) da = q(t_e) dt$ , with  $t_e = t - a$ . Or simply to use the time difference directly:  $q_a(t) da = q(t - a) dt$ . Additionally, placing the age parameter  $a$  as a subscript in  $q_a$  is not a good choice, because a subscript does not make it clear that this is not an index or other sort of descriptor or discrete subset indicator, but rather a variable parameter that is as important for the value of  $q_a(t)$  as the time parameter  $t$ . Thus I think it would be clearer to write:  $q_{age}(t, a) da = q(t - a) dt$ . Then it becomes

**65** also clear that  $q_{age}$  can be differentiated wrt. to two parameters,  $t$  and  $a$ . Can you please consider a better notation? One more question concerning units: If  $q(t)$  has units kg/kg, then  $q_{age}(t, a)$  should have units kg/kg/s. Further age  $a$ , time  $t$  and the differentials  $da$  and  $dt$  have units s. Then Eq.(3) cannot be correct. I think the  $dt$  needs to be dropped, and then Eq.(3) would read:  $q_{age}(t, a) da = q(t - a)$ . As you see Eq.(3) needs to be re-written and explained better.

**70 Thank you for this comment, we have rewritten this section to improve its clarity.**

**1.5 Eq.4: What is  $c$  in  $q_a/c \cdot S^\sim$  ?**

Should this  $c$  scale the total loss of  $q$  given by  $S^\sim$  to that related to a given age? Further I struggle a bit with Eq.4 because of the emission  $E$ , which is the only term in Eq.(4) that seems unrelated to a given age.  $E$  should certainly be a source for  $q_a$  of age 0 at that time. But it also should also not modify  $q_a$  of age  $a > 0$ . This is not visible in Eq.(4). Maybe I completely misunderstand

**75** your Eq.(4), or it should be re-written.

$c$  is a typo, and was replaced by  $q$ , matching equation A5. Thanks for catching this typo.

**1.6 L132: ... The negative moisture tendencies from the convection and condensation schemes ...**

**80** What about the positive moisture tendencies from convection and condensation schemes, for example from evaporation of rain? Are such processes, which convert water condensates in the atmosphere to water vapor, ignored in this model? Please

clarify this aspect.

85 We only consider the negative moisture tendencies because we chose to implement equation 8 into our model, as opposed to equation 7, which is hard to implement since aggregating the positive moisture tendencies is more complicated. We should make it clearer that we are implementing Equation 8 into our model. The changes we implemented on line 134.

### 1.7 L146: ... and constant SST change experiments ...

Does this mean an AMIP experiment with a constant increase in SST, like AMIP + 2K?

90 This comment (and the other ones on the subject) have made us realize that we were not clear on what we mean by "AMIP experiment". We forgot to mention it, but we have the same configuration as Thomson and Vallis (2018), where the SSTs are kept to an annually repeating but seasonally varying SST climatology from AMIP. Our climate experiment is similar to the amip and amip4k experiment from Taylor et al. (2012), which they describe as the AMIP run, but with a uniform 4-K increase in SST (2K in our case). We implemented these changes around line 151.

### 1.8 L147: ... The model ran for 30 years with the configuration described above, ...

95 Please specify what this configuration is. Please add the details that would be necessary for reproducing such simulations, e.g. the time period covered (1979 to 2008, or any other 30 year period?), source of greenhouse gases, ozone, land boundary conditions etc. Please also explain the details of the "constant SST change experiments". How were these started, what is the constant increase, which constant SST changes are used? Please add a table for all of your experiments evaluated in this article, with the necessary details so that it would be possible to repeat your experiments in fair approximation, maybe with a different model.

100

We agree that this section could use more details. We added details around line 150 and a table detailing the model configuration in Appendix C.

### 1.9 L147: ... The model ran for 30 years with the configuration described above, and the results were averaged over the final 5 years to avoid any spinup. ...

105

Please explain why you need 25 years of spin-up in an AMIP simulation. The time scales in the troposphere (days) and in the land (months) are certainly much shorter. Age of air in the stratosphere can reach ca. 4 years. Why are you forced to skip 25 years, which is 83% of your simulation. If you can make use of 20 or even 25 years, you could have better statistics.

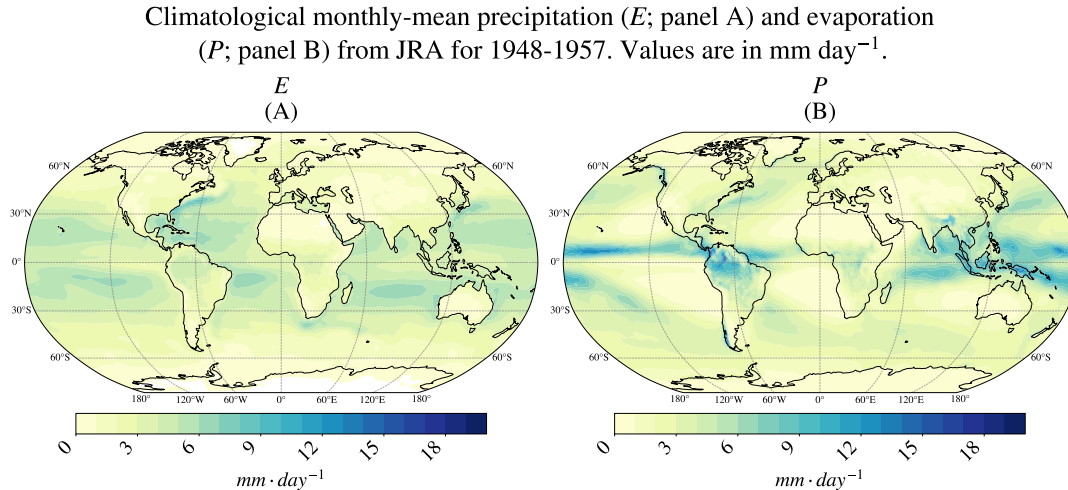
110 We have changed to a 20-year spin-up and 10 year average. We chose to do a 20-year spin-up to make sure that the stratosphere properly spun up, as it takes multiple years for it to be fully ventilated. The time and zonal average of the mean WV age can reach multiple months in the stratosphere, but values of the mean WV age can reach up to multiple years for some timesteps. So, our 20-year spin-up assures us that the atmosphere is fully spun up. Furthermore, a 10-year average captures well the mean state of the atmosphere.

### 1.10 L200: ... The regional patterns and mechanisms of vertical water vapor transport, evaporation, and precipitation generally match between the model and observations (figure 3) ...

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120 Here the authors claim that the model and the observation match. But neither observations nor differences between model and observations are shown. Certainly the precipitation does not match observations (essentially no precip in tropica Africa, very little precip in Amazonia, etc.). Statements about the model quality must be toned down to a more realistic level that accounts for the level at which this model can reproduce the main features of the observed general circulation, moisture distribution, precipitation etc. It is certainly not necessary to have the perfect model for this study (though better is better), but one also should not claim that the model matches observations without any real discussion.

125 Figure 1 shows the 10-year average of evaporation (A) and precipitation (B) from JRA. When we look at evaporation, the general pattern seems to be similar, but we note some overestimation on our part around Central America and Southeast Asia. For precipitation, the general pattern is respected, but values are lower in general. Similarly to evaporation, there is a strong overestimation around Central America and Southeast Asia. But the regions of high and low precipitation align well. Nonetheless, we added some consideration of the differences around line 205 to add some nuance.



**Figure 1.** 10-year averages of evaporation (A) and precipitation (B) from JRA re-analysis data KOSAKA et al. (2024)

### 1.11 L204: ... The eddy transport (figure 3B) dominates in two regions, ...

130 Maybe it is worth to mention that the southern hemisphere mid latitudes are another important region of eddy moisture transport. Though there do not exist single hotspots as in the north, the eddy moisture transport in the SH is substantial.

That is a good point, we added this detail on line 210.

### 1.12 Section 3 in general

135 Section 3 in general: It is worth to mention that all the following discussion is based on the final 5 years of the control AMIP simulation and – so far not unknown – maybe also 5 final years of all (how many?) constant SST warming experiments

We agree that we did not make it clear. Essentially, we use a similar configuration to Thomson and Vallis (2018) that we then perturb uniformly like the amip4k experiment from Taylor et al. (2012). More details are now presented in section 1.7.

### 140 1.13 Figure 4: Please add some more pressure labels

Figure 4: Please add some more pressure labels on the vertical axis in the panels. That would help to understand the vertical structure, e.g.: ..., 100, 70, 50, 30, 20, 10, assuming that 10hPa is the highest level shown.

We agree with this comment, adding pressure labels would make it easier to interpret this figure. This has been modified.

145

#### 1.14 L258: ... Brewer-Dosbon circulation (BDC)

... Brewer-Dosbon circulation (BDC) the air mass rises ,... → ... the air mass rises in the upward branch of the Brewer-Dobson circulation (BDC), ...

150 Thank you for pointing this out. This mistake was corrected to improve readability.

#### 1.15 L266: ... There is a clear difference between the exponential-like distribution

There is a clear difference between the exponential-like distribution near the surface and the top of the updraft (solid and dotted curves), and the Gaussian-like distribution within the strongest region of the updraft (dashed curve). ...

155 In Figure 5 A and B only the dashed curves are Gaussian, meaning that the distributions drop towards  $P = 0$  the age going towards 0. The other profiles are exponential although no air can have age 0 away from the surface. In principle all age spectra in the atmosphere should take off from  $P = 0$  at age = 0, right? Is this the price to pay for a Gamma distribution parameterized age spectrum? Maybe it is worth to discuss these “artifacts” in the parameterized age spectrum, either where the parameterization is introduced or here where the first such results are shown.

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Thank you for this comment. The Gamma distribution is defined on  $a < 0$ . Therefore,  $P$  at age= 0 is not defined in our current application. Appendix B, we compare our results with Tuinburg and van der Ent (2016), whose WV age distribution (figure 4 of their paper) also shows that very low age is resolved. However, we agree that we had not mentioned that the age is defined for  $a < 0$ , so we added this detail near equation 9.

165 1.16 L278: ... Generally, the age is greater in the poles and increases towards the mid to low latitudes. ...

Generally, the age is greater in the poles and increases towards the mid to low latitudes. ... Sentence unclear. Yes, the age is greater at the poles, but “the age ... increases towards the mid to low latitudes.” is unclear. Please rewrite.

Looking back, we agree that this sentence deserves to be rewritten. We rephrased it to improve clarity.

170 1.17 L291: ... converges, rise, does ... → ... converges, rises, does ...

... converges, rise, does ... → ... converges, rises, does ...

Thank you.

#### 1.18 L359: ... and investigate how they scale with temperature ...

175 This introduces new experiments which have not been introduced so far, except for the brief mentioning in the beginning. It is really necessary to provide the details of such new experiments. Otherwise it is not possible to reproduce such a study. Fig. 8 shows 9 data points on each line. Does this mean that there exist 8 simulations with a globally constant sea surface temperature increase with respect to the observed SST used in the control AMIP simulation, which probably contributes the coldest point on the graphs? It is necessary to explain these simulations before entering the discussion on the results. Please  
180 add a documentation of all simulations, for example including a table of all simulations. Further, the x axis is described equally as “Global Surface Temperature [K]” in both panels, but the axes have different ranges: ca. 284 – 294 in (A) and ca. 295 – 304 in (B), although the same simulations are shown in both panels, and therefore the Global Mean Temperature must be the same. But maybe the axis for (B) should be “Tropical Surface Temperature”?

185 We agree that we could provide more context on the experiment here. We have added a table in Appendix C with the specific parameters of the model. Each simulation constitutes a constant rise by some degree of the AMIP fixed SSTs. We have

added information on the climate change experiment on lines 356-361. Also, you are right that the x-axis of the panels should read “Average Surface Temperature” to avoid confusion.

**1.19 L362: ... This shows how we can expect global TUT to be equal to a global average ...**

190 This shows how we can expect global TUT to be equal to a global average of the mean WV age, and, about equal to the global mean of precipitation age. But, the latter would evolve differently with climate change (although they match the wide range of anticipated scaling rates of 3-6. What is missing here is a quantification of the statistical uncertainty of the model results. While there is no doubt that the TUT and the mean age in (A) are equivalent (the lines overlap), it would be interesting to know if the 5th point of the precipitation age, between 288 and 289 K, which creates a deviation from a smooth line, is simply a statistical outlier, or if it is not. Anyway, as I assume that you can derive statistics for the uncertainty of the results in Figure 8, I ask you to provide this information. In the Legend of Fig. 8 the dashed line is described as  $\langle \Lambda \rangle / \langle W \rangle$ , but the caption uses  $\langle q \tau \rangle / \langle W \rangle$ .

Figure 8 shows the global turnover time (TUT) as a function of globally averaged temperature (T) over 2 domains. For each definition of TUT, we calculated the fractional increase rate as

200 
$$\phi_{TUT} = \frac{1}{\Delta T} \cdot \frac{\Delta(TUT)}{TUT} \tag{1}$$

discretely, for each value of  $TUT$ . Then, those values were averaged over the temperature range. Now, we perform a linear regression,

$$TUT = m \cdot T + b. \tag{2}$$

to be able to calculate the fractional increase rate from it, and in turn get error bounds based on the fitting parameters. The fractional change in TUT per degree of global warming is

205 
$$\phi_{TUT} = \frac{m}{m \cdot T + b}. \tag{3}$$

Then, the fractional increase rate is a function of  $T$ . We calculate the mean  $\phi_{TUT}$  as the associated fractional increase rate for our climate change experiment. Now, let  $\sigma_m$  and  $\sigma_b$  be the uncertainty on parameters  $m$  and  $b$  from the linear regression, and  $Cov(m, b)$  the covariance. Then, the uncertainty on  $\phi_{TUT}$  is

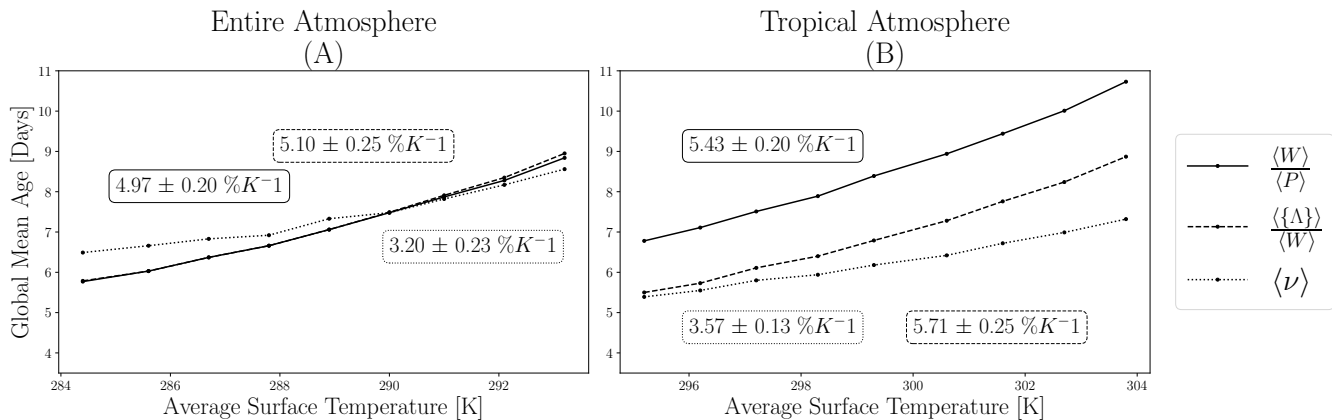
210 
$$\sigma_{\phi_{TUT}} = \frac{\sqrt{b^2 \cdot \sigma_m^2 + m^2 \cdot \sigma_b^2 - 2 \cdot b \cdot m \cdot Cov(m, b)}}{(b + m \cdot x)^2}. \tag{4}$$

Figure 2 is the new version of Figure 8 from the paper, with a recalculation of the fractional increase rate, and the associated uncertainties. As we can see, error bounds are fairly small.

We will fix the notation in the caption to be the same throughout the paper.

215 **1.20 L370: ... Broadly, figure 8B shows how our previous assumption about the equivalence of  $W/P$ ,  $\langle \Lambda \rangle / W$  and  $\nu$  breaks down over the tropics. ...**

Broadly, figure 8B shows how our previous assumption about the equivalence of  $W/P$ ,  $\langle \Lambda \rangle / W$  and  $\nu$  breaks down over the tropics. ... And by extension the same would hold for the non-tropics. Would it be wrong to assume that the equivalence found on the full globe actually cannot be expected for any subdomain due to the lateral exchange of water vapor between the subdomain and the rest of the world? Here the tropical domain exports moisture to the mid and high latitudes, which qualitatively must have the discussed effects. Here of course the authors are also able to quantify the effect. But overall it is not a surprise. I wonder if there is something more to conclude from Fig. 8B.



**Figure 2.** Turnover Time (TUT),  $\langle \frac{W}{P} \rangle$ , the globally averaged moisture weighted mean WV age tracer  $\langle \Lambda \rangle / \langle W \rangle$ , and the globally average precipitation age  $\langle \nu \rangle$  as a function of globally averaged 2m air temperature. The averages were performed on (A) the entire atmosphere and (B) restricted to the tropics. The mean fractional increase rate between temperatures is marked on each panel for each quantities.

Thank you for this comment. We believe that we had not made our main conclusion from Figure 8 clear in the text.

225

In the current literature, precipitation changes in the tropics has been assumed to be the main driver of changes in TUT (Held and Soden, 2006; Gimeno et al., 2021), hence why in Held and Soden (2006), they used the water flux from the tropics in their estimation of WV age. If that were true, we would expect panels A and B to be very similar. However, we see the opposite. This is the main conclusion we have from Figure 8.

230

Although we tend to agree with the reviewer that this point seems relatively straightforward, it is an idea that is frequently encountered in the literature, so doing a quantifiable test of it here seems appropriate.

**1.21 L374: ... They are calculated using the +2K climate experiment mentioned in section 2.2. ...**

They are calculated using the +2K climate experiment mentioned in section 2.2. ... Mentioning is not enough. Please explain the simulations properly in section 2.2.

235

Thank you for this comment. We added information on this at the start of section 3.2.

**1.22 L375: ... Figure 10D and 9A show how increased evaporation over the oceans, leads to a rise in  $q$  throughout the atmosphere. ...**

Figure 10D and 9A show how increased evaporation over the oceans, leads to a rise in  $q$  throughout the atmosphere. ... Certainly  $q$  in the atmosphere depends not on the evaporation alone, but on other factors as well, like the lapse rate change in the tropics, the precipitation change etc. I think the authors know this, and this sentence should simply be improved.

240

We failed to mention that other processes lead to a rise in  $q$ . We rewrote the sentence to remove the implication that it only depends on evaporation. Also, on line 385 we have added mentions of our land process and how it can impact the physics of our model. Mainly, it overestimates evaporation.

245

**1.23 L379: ... precipitation over the tropics (figure 10D) ... → ... precipitation over the tropics (figure 10E) ...**

... precipitation over the tropics (figure 10D) ... → ... precipitation over the tropics (figure 10E) ...

250 **Thank you. We applied the necessary change.**

**1.24 L383: ... Additionally, the eddy kinetic energy reduces in the mid to low troposphere due to the added moisture (Lutsko et al., 2024) (figure 9F). ...**

255 Additionally, the eddy kinetic energy reduces in the mid to low troposphere due to the added moisture (Lutsko et al., 2024) (figure 9F). ... Fig. 9F indicates a general reduction of EKE also in the upper troposphere with exception of the upper equatorial troposphere. (And such exceptions exist also in the lower troposphere poleward of 60° latitude.) As the figure shows this EKE reduction in the upper troposphere, it should be worth to mention this.

**Thank you for this comment. We now mention it on line 395.**

**1.25 L385: ... Over the stratosphere, ...**

260 Over the stratosphere, ... What is meant here? Can you rephrase?

**Thank you for this comment. We rephrased the sentence.**

**1.26 L387: ... figure 9 and 10 shows ... → ... figure 9 and 10 show ...**

265 ... figure 9 and 10 shows ... → ... figure 9 and 10 show ... Fig. 11 C and D: please add additional pressure labels on the vertical axes, as proposed earlier.

**We will add labels and correct the extra "s".**

**1.27 L465: ... our model**

270 ... our model has a very simplified land surface scheme, which creates anomalously high evaporation. Additionally, due to the low resolution of the model, additional moisture from nearby warm oceans can diffuse over the Sahara. Our model does not include snow or ice processes, which may also impact the simulation of water vapor age over the Tibetan plateau. The simplified surface processes may also explain the relatively small value of globally averaged precipitation in our model ...

275 No model is perfect. But generally it is better to point out such limitations in the description of the model (e.g. land surface scheme) and the simulation setup (e.g. resolution). Then the reader knows about this from the beginning.

**Thank you for this comment. We added this detail in the model description.**

**1.28 L467: ... procesess ... → ... processes ...**

**Thank you for this comment, we rephrased the sentence.**

280 **1.29 References**

References: The last reference in the list, by Éric J.M Delhez and Éric Deleersnijder, needs to be put in alphabetic order.

**Thank you for pointing out this mistake.**

## References

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