

Review of:

Quantifying Water Vapor Age: A Dynamical Constraint on the Response of the Global Water Cycle to Climate Change

by:

Philippe Boulanger and Robert Fajber

---

### **General comments**

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 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 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 vapor ages? Preventing loss of sea ice in the Arctic in warmer climates also means that new sources of water vapor are excluded. Etc. ...
- 

### **Specific comments**

L81: ... an atmospheric parcel ...

How should we imagine this air parcel? Is this assumed to be a macroscopic Lagrangian air parcel that follows a sort of larger 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?

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  $\int_0^\infty q_a da = q$  and that  $q_a$  has units of kg/kg/s ?

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)

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  $q(t_e = t-a)$  ...

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  $q = q(\lambda, \varphi, p, t)$  from a 3d atmosphere with advection, diffusion, evaporation, clouds etc., but then the statement on L.95 is not correct).

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

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:

$$q_a(t) da = q(t_e) dt, \text{ with } t_e = t-a.$$

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 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.

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

Should this  $c$  scale the total loss of  $q$  given by  $S^-$  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 your Eq.(4), or it should be re-written.

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

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.

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

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

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

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.

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. ...

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.

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

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.

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

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.

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

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.

L258: ... Brewer-Dobson circulation (BDC) the air mass rises ,... → ... the air mass rises in the upward branch of the Brewer-Dobson circulation (BDC), ...

L266: ... 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). ...

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.

L278: ... 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.

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

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

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 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”?

L362: ... 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% K<sup>-1</sup>, Gimeno et al., 2021). ...

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 5<sup>th</sup> 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  $\langle q\tau \rangle / \langle W \rangle$ .

L370: ... Broadly, figure 8B shows how our previous assumption about the equivalence of  $\langle W \rangle / \langle P \rangle$ ,  $\langle \Lambda \rangle / \langle W \rangle$  and  $\langle v \rangle$  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.

L374: ... 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.

L375: ... 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.

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

L383: ... 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.

L385: ... Over the stratosphere, ...

What is meant here? Can you rephrase?

L387: ... 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.

L465: ... 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 ...

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.

L467: ... procesess ... → ... processes ...

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