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Prof. Dr. Valerio Lucarini
Editor, Earth System Dynamics
University of Hamburg, Germany


Dear Prof. Dr. Lucarini,

**RE:** Response to Reviewers for Manuscript ESD-2013-35


Thank you for handling my paper "The dynamics of the Snowball Earth Hadley circulation for off-equatorial and seasonally-varying insolation." I also thank the reviewers for their positive evaluations and helpful comments.

Below I respond in detail (in black) to each of the reviewer's comments (in blue) and describe how the revised manuscript addresses their comments.

Thank you for your time and help improving this manuscript.


Yours Sincerely,


Aiko Voigt

**Reviewer #1**

I thank the reviewer for his/her supportive comments, based on which I have clarified several aspects of the paper.

**1.** The author (et al.) already obtained both results (i and ii from above) in the previous paper concerning the equinox case. Given this and the fact that the underlying physics/equations do not change, one may ask what reasons could cause dry theories not to hold for the off-equatorial case and the author may comment on this in the introduction.

**Response:**

Thank you for this comment. To better motivate why repeating the study of Voigt et al. (2012) for off-equatorial and seasonally-varying insolation is warranted, I added the following sentences to the introduction:

> However, Voigt et al. (2012) used perpetual equinox solar insolation, with the insolation maximum centered at the equator and zero insolation at the poles, which might be a potential limiting factor for their results because the strength and dynamics of the Hadley circulation are known to change with the location of the insolation maximum. For example, Lindzen and Hou (1988) showed that the strength of the circulation increases non-linearly in axisymmetric models when the insolation maximum is displaced away from the equator. Moreover, Walker and Schneider (2005), comparing the Hadley circulation of an idealized axisymmetric model with that of an idealized general circulation model, found that large-scale eddies strengthen the circulation for an equatorial insolation maximum, while they weaken the winter and strengthen the summer circulation for an off-equatorial insolation maximum. To address the question if the magnitude and effect of vertical diffusion on the Snowball Earth Hadley circulation shows a similar sensitivity to the chosen insolation, I here expand the work of Voigt et al. (2012) to the cases of a perpetual off-equatorial insolation maximum as well as seasonally-varying insolation.

**2.** The general setup of the experiment is design to study Snowball Earth and not to study a dry atmospheric circulation. Though the obtained climate is almost dry and I have no particular doubt that the main conclusions will also hold for the perfect dry case in general, figure 5 suggests that there is a non-zero effect of latent heat release (stabilizing the profile?). The author may provide some numbers to quantify the effect of the remaining moisture: How large is the global mean diabatic heating by latent heat release (or the surface latent hat flux)? How large is the radiative forcing due to the remaining water vapor? Of course it would be interesting to know how large the difference to a climate of a run with no moisture is (i.e. a run which is initialized without moisture and the surface evaporation is switched off).

**Response:** The reviewer is right that the Snowball Earth atmosphere is not completely dry. The global-mean atmosphere water vapor content is $0.01\,\mathrm{kg\,m^{-2}}$, and the global-mean surface latent heat flux is $0.1\,\mathrm{W\,m^{-2}}$; at the location of the solar insolation maximum, these values "increase" to $0.02\,\mathrm{kg\,m^{-2}}$ and $0.5\,\mathrm{W\,m^{-2}}$, respectively. However, these values are about three orders of magnitudes lower than in present-day simulations with the same model. Moreover, in the Snowball Earth climate the turbulent surface flux is dominated by the sensible heat flux (global-mean of $7.5\,\mathrm{W\,m^{-2}}$, maximum of $20\,\mathrm{W\,m^{-2}}$), in contrast to the present-day climate for which the latent heat flux dominates. Overall, these values justify to consider the Snowball Earth atmosphere a virtually dry atmosphere. To better illustrate this point, the revised manuscript gives the global-mean atmosphere water vapor content and surface latent heat flux in Section 2.

Unfortunately the diagnostic output from the model does not allow me to quantify the radiative effect of water vapor.

**3.** The vertical diffusion of momentum is related to a loss of kinetic energy. I wonder if and how this energy is given back into the atmosphere in the model (to assure energy conservation). Is this done by diabatic heating? If so, how large is the heating rate, and what is the effect on the (Hadley) circulation? If not, how large is the energy loss and what effect can be expected if energy would be conserved by heating?

**Response:** In the model, the loss of kinetic energy due to vertical diffusion is accounted for by dissipative heating. This ensures that the vertical diffusion conserves energy. As is illustrated in the figure below, the magnitude of this heating, however, is so small that it has no effect on the vertical stability and hence the magnitude of vertical diffusion. A corresponding sentence is added to the discussion of Figure 5 in Section 3.1.

**4.** It seems that transient eddy forcing is weak in the present runs compared to "normal" Earth conditions. Thus, it is not clear to me how transferable the results are to cases with significant eddy contribution. Would vertical diffusion
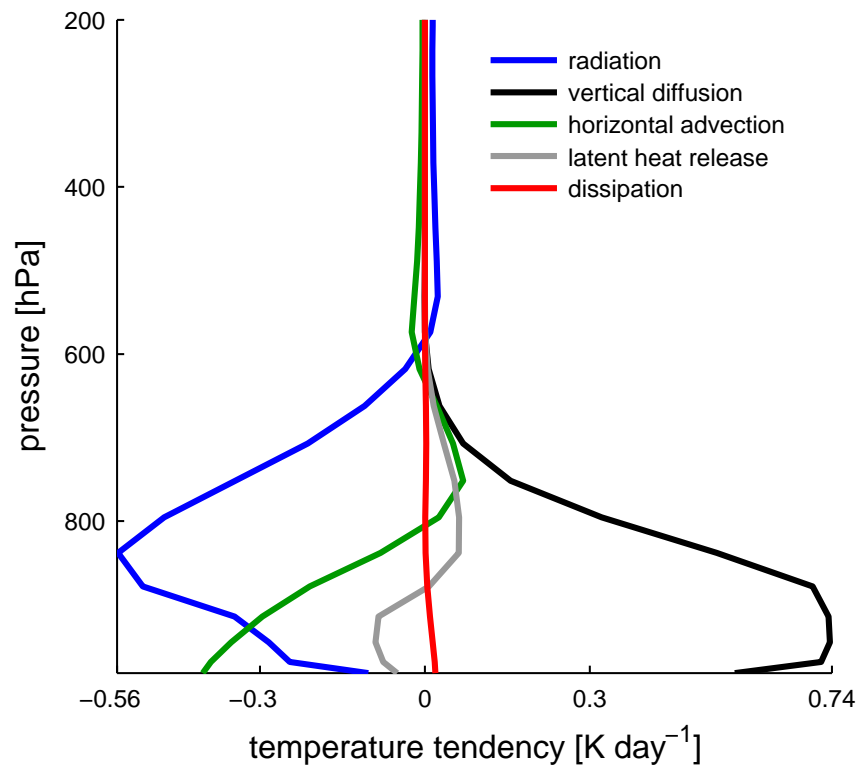
Figure 1: Temperature tendencies for perpetual off-equatorial insolation in the region of maximum solar heating, between 15 and 30 deg N. The heating tendency from vertical diffusion contains a dissipative heating (red line). The figure is an extension of Fig. 5 of the manuscript.

still play a major role there?

**Response:** In the dry atmosphere, the fact that transient eddies are not important for the Hadley circulation is an outcome of the paper (the general circulation model, of course, allows for these eddies in principle). In "normal" Earth conditions with a moist atmosphere, vertical diffusion of momentum would be suppressed because free-tropospheric temperatures in the Hadley cell region are on the moist adiabat. Vertical diffusion of momentum therefore does not play a significant role for the present-day Earth Hadley circulation. A corresponding sentence was added to the last paragraph of the conclusion section.

**5.** In chapter 2, the author may briefly note that no orography is present (i.e. there are no stationary waves and no mountain torque which may affect the Hadley cell as well).

**Response:** Thanks. This is now mentioned in Section 2.

**Reviewer #2 (Tapio Schneider)**

I thank the reviewer for his encouraging comments and his remarks about how to better relate this study to previous dry studies.

**1.** The vertical diffusion in the model depends on stability (through the TKE and the Richardson number and its effect on mixing length). Therefore, the vertical diffusion scheme is essentially a convection scheme, here primarily for dry convection, as latent heat release in phase changes of water seems negligible (it would be nice to quantify this!). The contrasts drawn to moist convection thus seem too strong (there is convection here, just without much latent heat release, which can be viewed as the dry limiting case of moist convection). It might be worth pointing this out clearly, as it affects the comparisons the author makes with several previous studies. In the latter, what was meant by "dry dynamics" was dynamics in an atmosphere resembling present-day Earth, but without a latent heat release that depends on temperatures etc. So the stratification in those studies was stable with respect to the parameterized convection throughout much of the atmosphere (except in the ITCZ). Vertical diffusion of momentum with a similar parametric dependence on stability would not play as prominent a role in those studies as it does in the present study, as convection in the center and descending branches of the Hadley circulations in the earlier studies is unimportant. Therefore, I think statements such as "the results suggest that vertical diffusion might alter the scaling laws for dry Hadley circulations derived in idealized atmospheric circulation models that neglect vertical diffusion of momentum" (p. 948) are too strong: It needs to be kept in mind that the author investigates a different dynamical regime than the (more present-day Earth-like) regime investigated in the earlier studies.

**Response:** I have revised the manuscript in order to bring out this issue more clearly. I made appropriate changes to the abstract and introduction. Most importantly I rewrote the last paragraph in the conclusion section, which now reads

> An important implication of this result is that theories for Snowball Earth Hadley circulations, and re-alistic dry Hadley circulations in general, ought to take into account vertical diffusion of momentum, although this might be difficult because of the small-scale nature of vertical diffusion. Moreover, the results suggest that the scaling laws for dry Hadley circulations derived in dry theories and idealized atmosphere models that neglect vertical diffusion of momentum (e.g., Held and Hou, 1980; Walker and Schneider, 2006; Caballero et al., 2008) might not hold in Snowball Earth atmospheres. This should not be necessarily interpreted as a failure of these theories and idealized models as they were often developed to capture the dynamics of present-day Earth Hadley circulations, for which surface sensible heat and dry buoyancy fluxes are comparably small and vertical diffusion does not play a dominant role. Nevertheless, the fact that these dry theories and idealized models do not take into account vertical diffusion limits their applicability to the Snowball Earth atmosphere, as well as to realistic dry atmospheres in general.

**2.** "The presence of strong vertical diffusion is explained by the radiative cooling of the troposphere." (p. 947 and similarly elsewhere). I do not think the radiative cooling "explains" the strong vertical diffusion; it merely is in dominant balance with it energetically. A better "explanation", in my view, is that on Earth currently, the dominant loss term in the surface energy balance is evaporation (latent heat flux). This is not, or only to a very limited degree, available on snowball earth. Instead, sensible heat fluxes are strong to balance the net radiative energy gain of the surface. This means there are much stronger buoyancy fluxes at the surface than on Earth currently, driving strong (dry) convection, which is represented diffusively in the model here. (And, of course, the resulting enthalpy gain to the atmosphere must be balanced by radiative cooling, but it does not mean the cooling drives or explains the sensible heat fluxes?in the same way that drag on a sliding object cannot be said to explain the sliding...). This, in my view, is the essential difference of this study compared with previous studies of "dry" Hadley circulation dynamics: In those previous studies, forcing parameters were chosen such that buoyancy fluxes etc. are Earth-like, which meant that the corresponding radiative driving at the surface would have been relatively weak (it was not represented explicitly). In the present study, the author chooses to close the energy budget with "realistic" radiative fluxes (which, of course, is a fine choice, and relevant to snowball Earth). But it results in much stronger buoyancy fluxes than we presently have on Earth.

**Response:** Thank you very much for this comment, which encouraged me to rethink the argument. Indeed, I agree with the reviewer that the main reason for dry convective activity is the destabilization of the lower troposphere by

sensible heat fluxes. While it is conceivable that internal radiative destabilization could also drive internal convective activity, pure-radiative equilibrium simulations would be required to test to which extent the pure-radiative equilibrium temperature profile is indeed unstable. I have changed the paragraph accordingly. The new paragraph reads:

> The temperature tendencies in the region of maximum insolation are shown in Fig. 5. Above the tropopause at 500 hPa the atmosphere is close to radiative equilibrium, with nearly zero radiative heating rate. Within the troposphere, which extends from the surface up to 500 hPa, however, the main balance is between longwave radiative cooling and heating by vertical diffusion. The heating by vertical diffusion includes a very small and negligible contribution from dissipative heating, which ensures that the vertical diffusion scheme conserves energy. Temperature tendencies from other processes are comparably small: the near lack of atmospheric moisture implies only small latent heat release, and vertical advection has no effect on temperature because of the dry-adiabatic temperature profile. Horizontal temperature transport by the Hadley circulation provides some heating in the upper troposphere by displacing isentropes from the vertical, which enables the dry Hadley circulation to transport energy (Caballero et al., 2008; Voigt et al., 2012). Overall, however, the troposphere is close to a radiative-convective equilibrium. Absorption of solar radiation at the surface leads to strong surface sensible heat and buoyancy fluxes, reaching about 20 Wm$^{-2}$ at the insolation maximum. This near-surface destabilization of the troposphere causes dry convection, which explains the strong presence of vertical diffusion and the dry adiabatic temperature profile. The longwave radiative cooling balances the upward transport of dry static energy by dry convection. The magnitude of dry convection would be increased if longwave radiative cooling destabilized the troposphere on its own, i.e., in the absence of upward energy transport by dry convection. Similarly, atmospheric solar absorption could stabilize the troposphere, which would limit the dry convective activity. Pure-radiative equilibrium simulations would be needed to test and quantify these effects.

I also adapted the corresponding paragraph of the conclusion section.

**3.** Eqs. (3) and (4): It is not clear from these expressions and the surrounding text what is meant by the "mean component" of Psi, i.e., whether that includes only the mean horizontal advection (relative vorticity) term in (1), or also the mean vertical advection term. (The figure captions are clear on what is plotted, though.)

**Response:** In the revised manuscript, I have included equations for *M* and *E* to clarify this point.

**4.** "The tropical annual-mean precipitation minus evaporation pattern exhibits net evaporation in the region around the equator" (p. 945). It might be worth pointing out that the same occurs on Titan today, for the same reasons the author discusses (e.g., see papers by Mitchell and collaborators, Schneider et al., Nature, 2012).

**Response:** The studies of Mitchell (2008) and Schneider et al. (2012) are now mentioned in Section 5.

**5.** Section 5: It would be helpful to reference papers by Neelin and collaborators (e.g., Chou and Neelin 2004) and Held & Soden (2006) here, who make analogous arguments. But here dynamic variations (changes in HC strength) dominate, rather than the thermodynamic variations dominating in HS2006.

**Response:** Thanks. The studies of Chou and Neelin (2004) and Held and Soden (2006) are now referenced.

**6.** A few typos (there are more).

**Response:** Thanks. I carefully checked the manuscript for typos.

# References

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