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Interactive comment on “The dynamics of the Snowball Earth Hadley circulation for off-equatorial and seasonally-varying insolation”

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Voigt studies the dynamics of the Hadley circulation on snowball Earth under perpetually non-equinoctial and seasonally varying insolation. This paper follows an earlier paper (Voigt et al. 2012) on analogous equinoctial situations.

The paper is interesting, contains novel results, appears correct, and is well written. It should be published. However, I do have some suggestions:

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

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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 parameteric 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 di \tilde{r} usion might alter the scaling laws for dry Hadley circulations derived in idealized atmospheric circulation models that neglect vertical di \tilde{r} usion 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.

2. "The presence of strong vertical di \tilde{r} usion 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

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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.

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.)

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).

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.]

A few typos (there are more):

"the the cell" (p. 935)

"The dynamics *of* the transition" (p. 943)

"reaches the similar values" [remove 'the'] (p. 943)

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"the inertial terms causes" (p. 944)

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