

Interactive comment on “Temperature distribution and Hadley circulation in an axisymmetric model” by N. Tartaglione

Anonymous Referee #1

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This paper takes an idealized approach to understanding the relationship between the atmospheric circulation and the atmosphere's thermal structure. The model is axisymmetric and there are parameters that allow for varying both the meridional and vertical structure of the equilibrium temperature profile. The experimental design is different from earlier studies in that the equator-pole temperature contrast is held fixed throughout, but the global mean temperature is allowed to vary as the thermal profile is changed.

Overall, the approach taken in this paper is an interesting one, but I have serious reservations about the physical basis for using this model. I understand that the model is idealized, but there should be some way of connecting phenomena in an idealized model to the real atmosphere, and in my view, this paper does not succeed in doing

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this. One problem is that the "jets" in this model appear to be driven by completely different physics than the jets in other models, or the jets in the real atmosphere. In more realistic models, the subtropical jets would form near the poleward edge of the Hadley Cell (HC), reflecting a strong contribution from angular momentum conservation. And the midlatitude jet would be located beyond the HC edge. In this model the jets sit equatorward of the streamfunction maximum. So I am not sure what is generating the jet in this model and what it tells us about the midlatitude or subtropical jets in the real atmosphere.

More problematic is the discussion on p.1632 and Fig. 5, which shows that the difference between potential temperature and equilibrium potential temperature ($\theta - \theta_E$) is not conserved. This model uses a "Newtonian cooling" scheme to mimic radiative processes, and in such a scheme, $\theta - \theta_E$ is directly proportional to the diabatic heating, which is in turn directly proportional to the vertical velocity (Held and Hou 1980, eq. 9c). So the fact that $\theta - \theta_E$ is not globally conserved implies that the global integral of vertical velocity is non-zero, which implies that mass is not being conserved. Unless there is some way of correcting this, I do not see a physical basis for using this model at all, and I cannot consider this paper acceptable.

Other notes:

1623 L9: "have been" → "have also been"?

1624 L15, "horizontal variable" → "horizontal coordinate"

1627 L20: It is interesting that you choose an approach that allows the global mean temperature to change as you vary n and k , and I think you should justify this a bit more. Chen et al. (2013) showed that for a uniform SST forcing there is Hadley Cell expansion. Of course, uniform SST warming does not necessarily mean that the warming of the atmosphere is uniform. But it does raise the possibility that some of the HC widening is due just to the increase of global mean temperature, rather than changes in the temperature distribution. Your discussion of HL92 on p.1629 seems to hint at

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this, but you do not address it explicitly.

1630 L9: "Since the circulation intensity changes greatly in our experiments, it is problematic to define a width of the Hadley cell based on the absolute value of the circulation itself." This is not an issue in the real atmosphere because there is a clear zero line, which is not the case in this axisymmetric model. So this sentence is a bit misleading.

1630: "The width of the cell will be defined more or less by isolines having 1/4 of the maximum value of the stream function." This is not a precise definition. Was that intentional? "More or less" is sloppy language.

1630: "Hence in general when n increases, and the total energy input is larger, the stream function is weaker but poleward." You should mention that this is in agreement with Lu et al. (2008), Gastineau et al. (2008), and Tandon et al. (2013).

1631: "Evidently for $R = 0.121\dots$ " I don't understand this sentence. Please be clearer about how the numerical and theoretical predictions are different.

1633: "Nevertheless the time-dependent solutions never attain..." -> "The instantaneous HC never resembles the symmetric circulation."?

1633 L20: "by the" -> "when"

1633 L23: "weaker in " -> "weaker than"?

1635: "However, when we use the jet latitude to define the edge of the Hadley cell..." Here, you appear to be treating the jet in your model as analogous to the eddy-driven midlatitude jet, and there is no basis for such a comparison.

Fig 4 caption: "Eq. (10)" -> "Eq. (11)"

References: Chen, Gang, Jian Lu, and Lantao Sun, 2013: Delineating the Eddy–Zonal Flow Interaction in the Atmospheric Circulation Response to Climate Forcing: Uniform SST Warming in an Idealized Aquaplanet Model. *J. Atmos. Sci.*, 70, 2214–2233. doi: <http://dx.doi.org/10.1175/JAS-D-12-0248.1>

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Gastineau, G.; Le Treut, H. Li, L. Hadley circulation changes under global warming conditions indicated by coupled climate models *Tellus*, Blackwell Publishing Ltd, 2008, 60, 863-884. doi:10.1111/j.1600-0870.2008.00344.x

Held, I. M. Hou, A. Y. Nonlinear axially symmetric circulations in a nearly inviscid atmosphere. *J. Atmos. Sci.*, 1980, 37, 515-533

Lu, Jian, Gang Chen, and Dargan M. W. Frierson, 2008: Response of the Zonal Mean Atmospheric Circulation to El Niño versus Global Warming. *J. Climate*, 21, 5835–5851. doi: <http://dx.doi.org/10.1175/2008JCLI2200.1>

Tandon, Neil F., Edwin P. Gerber, Adam H. Sobel, and Lorenzo M. Polvani, 2013: Understanding Hadley Cell Expansion versus Contraction: Insights from Simplified Models and Implications for Recent Observations. *J. Climate*, 26, 4304–4321. doi: <http://dx.doi.org/10.1175/JCLI-D-12-00598.1>

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