Reviewing of the paper NPG80-2014-80

I performed a review with new eyes and I decide to change a few things in the paper that referees did not address.

I changed the title, I added the word equilibrium before temperature, so the new title "Equilibrium temperature and Hadley cell in an axisymmetric model" explains better the paper.

I removed the discussion about the old figure 4, it did not address really the problem I wanted to discuss and in my opinion it was misleading in some respects.

There were too much "temperature" words where I meant "equilibrium temperature", although the temperature tries to adjust to the equilibrium temperature, rarely they coincide. I changed in according with I meant (I hope that all be coherent, now).

Here I reply to the referee (*his comments in italic,* **my comments in bold***).*

Reply to referee #1

I would like to thank the referee 1 for his doubts and for his corrections. I want to thank him even for making clearer the paper. I made his doubts mine and I will answer all questions, however in a very few points I cannot understand the referee.

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
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 stream function maximum.

I cannot understand the statement of the referee since it is very clear that the subtropical jets are located near the poleward edge of the Hadley cell and poleward the maximum stream function (cf. Table 1 -pag 1642 and Figure 3a -pag 1645).

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.

It is not clear to me what the referee means for midlatitude jet since the model is not able to produce midlatitude jets and so I cannot see how it can tell us something about midlatitude jet.

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.*

The referee cites Held and Hou 1980, eq. 9C, page 518, but eq. 9C is a linear formula obtained by approximating the thermodynamics equation, but we are working with a full nonlinear version. Thus it is clear that we are in a breakdown situation of that approximation since the model is able to reproduce well the Hadley cells and subtropical jets. The interesting thing, in my opinion, is that the model produces about the same behavior; a HC more or less extended but with subtropical jets at the same position (about 30°) for almost all the experiments, even with different stratifications. I find also interesting the difference $(\theta - \theta_E)$**when k is different from one (see new Fig. 4).**
The parameter k is equivalent to change the vertical stability, Cessi (1998) showed in her analytical solution that stratification plays a role of diffusivity that can be even negative for unstable stratifications, I suppose that a negative diffusivity is present here when k=0.5 and strong positive diffusivity is present when k=3. This latter case is intuitive enough, k=3 means that horizontal isotherms become closer and closer at upper levels making the atmosphere very stable there, so the upward branch of the HC has to do more work to go in upper levels, and this also the reason the maximum is located at lower levels, whereas it is located at upper levels when k=0.5 (I added a new paragraph, discussing the new Fig. 4, to explain this, the conservation of $(\theta - \theta_E)$**, in the classic definition of Held and Hou is obtained for k=1, instead). Thus it appear quite interesting to me to analyze the model results changing k even though the applications to the real world could be questionable.**

1623 L9: *"have been" –> "have also been"*

Thank you for the suggestion.

1624 L15, *"horizontal variable" –> "horizontal coordinate"*

Thank you for the correction. It is greatly appreciated.

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 this, but you do not address it explicitly

It is largely recognized that what drives the Hadley circulation is mainly the horizontal gradient, however in my opinion the role of the equator-pole temperature gradient is overstressed whereas what that has really importance is the tropical gradient and I kept constant the equator-pole equilibrium temperature gradient, with n changing, to demonstrate this. Moreover, as the referee can see the widening of Hadley circulation can be due to an

increase of temperature if this changes the temperature gradient at the equator. I hope that the new version of the paper addresses the problem.

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.

I meant that in the model there is not a clear edge, I changed the sentence.

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.

As before, I wanted to define an edge in term of stream function, I could chose ¼, but it is absolutely an arbitrary choice, this is the reason I used more or less. But I think that if it is clear that the chosen value is arbitrary I would remove more or less.

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).

Thank you for this suggestion.

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

I removed the part containing this sentence. I did a review myself and I think this part is not clear enough to me too, so it becomes difficult to explain correctly what I really wanted to say.

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

Yes, I meant exactly that the referee wrote.

1633 L20: "by the" –> "when"

Thank you for the correction.

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

Thank you for the correction. It is really appreciated

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.

I agree completely with the referee, I did not mean the mid-latitude jets, but the subtropical jets are located at the edge of the Hadley cell, so I can define here the edge of the cell by using the location of the subtropical jets. The problem of relating subtropical jets and edge of the Hadley cell in an axisymmetric model can be in somewhat problematic and here, in my opinion, becomes also interesting since when forcing changes the subtropical jet remains at

about 28° for most of the experiments. I changed the sentence in order to stress that jet location remains almost constant for almost the n values.

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

Thank you for the correction. However I removed Fig. 4