

Interactive comment on “Inferring global wind energetics from a simple Earth system model based on the principle of maximum entropy production” by S. Karkar and D. Paillard

Anonymous Referee #2

Received and published: 23 April 2015

This paper uses a simple model to estimate the energetics of the atmospheric circulation. Admirable for its pursuit to break complexity down into simpler parts and the lack of parameterizations, I too am quite curious p.409 “Is it possible to devise the energetics of atmospheric circulation, without computing the entire dynamics, or...how far can we go, in terms of wind energetics, with a simple box model?” The authors partially address this point throughout the paper, but are also continually side-tracked by wind power estimates. I don’t disagree that the two topics are closely related, but in my view, one first needs to be confident in the applicability and limitations of the simple approach. Confused by abrupt changes in horizontal resolution and discriminating between this work from Herbert et al. (2011), I do think the authors have something new

C182

to add here. In its present form, I am uncomfortable with the number of critical unanswered questions that remain. My recommendation would therefore be for a major revision.

Please be advised that resolving the major points below would only allow me to more fully understand your unique scientific contribution. Ultimately the decision of the editor, addressing these major points would not automatically cause me to recommend acceptance. I am suggesting a major revision rather than a rejection because I think you have some unique aspects here, and should have the opportunity to clarify them.

Major points of concern

1. New - Where does Herbert et al. (2011) end and this one begin? I lose confidence regarding what is new here when I don’t see this clearly defined anywhere in the text.
2. Dry atmosphere - What does this mean for this model? (p.411 line 5: standard humidity profile of each grid cell [but only one vertical level right?]; p.415 line 5: dry atmosphere so all energy is exchanged as sensible heat)
3. [considering that the atmosphere is one vertical level, how can one say in the footnote] p.416 “Here, we only intend to represent the vertical mean of the winds in the troposphere, however no hypothesis is made on the vertical structure.”
4. Resolution changes throughout the Results section - This is a major concern. You state p.417 “Note that results do not always improve with finer grids, as they do with most models...” Section 3.2 uses 72x96, then 36x48 in Section 3.3, 72x96 in Figure 4, 36x48 in Figure 6 - You state in the Discussion (p.422 line 4-5) that resolution strongly influences the result, so how am I to interpret all the different resolutions for which I’m being shown results?
5. Atmospheric boundary layer - What? I don’t understand how the one level atmosphere is re-interpreted this way. Before, the total atmospheric dissipation rate was 1000-2500 TW (p.420), and now the atmospheric boundary layer dissipation rate you

C183

derive is 400-800 TW. The rates seem ok, but again, I do not see how you get there at all.

6. Poleward heat transport - Herbert et al. (2011) Figure 4a (which I saw after looking at the surface temperature comparison referenced in your paper) looks a lot better than the Figure 3, in that it does not need a 50% increase — Are these the same models or not?

7. Units - What is gained by leaving them out of the text (even as a table)? Not including them makes the model and its result less transparent.

Minor points

8. Why is the wind power application so prominent (first 2 sentences), while the model itself and its ability to reproduce complex modeling results difficult to follow?

9. What makes the Maximum of Entropy Production (MEP) principle (or is it an approach) so applicable to this model's intention as to include it in the title, abstract, etc.?

10. Zonal winds, not just the meridional component (Fig. 7) appears to be quite strange, which is not noted on p.418 [line 23-27]

11. Plot gridded data rather than the resampled contoured data (Fig. 2, Fig. 4, Fig. 5), as I want to see output rather than interpolated output

12. Figure 4 & 5 should at a minimum, use the same color bar ranges, so I can compare the two results as you recommend to show validity in your simple model

Things I would like to see more of

a) Giving ranges in the Results (such as in Section 3.4.2) and placing them in context with other estimates

b) Applying such a model to revisit wind power estimates seems useful, but I first need to understand the model and have confidence in its applicability to estimating

C184

atmospheric energetics first

Interactive comment on Earth Syst. Dynam. Discuss., 6, 407, 2015.

C185