Review of "Inferring global wind energetics from a simple Earth system model based on the principle of maximum entropy production" by Karkar and Paillard, submitted to Earth System Dynamics

The paper proposes a simplified approach to understanding general patterns of temperature and wind on Earth. It is valuable to use simplified approaches to understand fundamental geo-physical issues. So in principle this paper could be useful. However, its assumptions are weak in the first place, some equations appear to be wrong (or perhaps are just poorly described), and the results have too many weaknesses and weird behaviors, to the point that the value of the simplified approach itself vanishes. What do we learn from using this approach that we did not know already? I am afraid that the answer is "nothing" and therefore my recommendation is for a rejection.

Major issues

1) There is a fundamental inconsistency.

The model proposed in the paper is based on the so-called "Maximum Entropy Production" (MEP) principle. From the literature (e.g., Martyushev and Seleznev 2006), the MEP principle applies to non-equilibrium situations only. Therefore it cannot be used for equilibrium, or steady-state, or stationary conditions. Yet, the basic equations used in the paper are valid at stationary state (e.g., Eq. 1). How can this fundamental inconsistency be explained?

2) Some of the model equations are either flawed or poorly explained.

Eq. 1, valid at equilibrium, states that there is a balance between radiative fluxes R_i (W/m^2) and the divergence of other energy fluxes d_i at a grid cell. What are the units of these other energy fluxes (later described in 2.3.1 as just sensible heat fluxes)? Being a divergence, these other energy fluxes then must have units of W/m. What type of flux has these units of W/m? Also, where does this equation come from? The classic temperature equation links temperature changes to the divergence of ALL heat fluxes, including radiative, sensible, and latent. Why are the radiative fluxes out of the divergence here?
In Eq. 11 it appears that these other energy fluxes with units of W/m are actually sensible heat fluxes only (no latent heat) and are indicated as $F_{i\text{-}j}$. However, the units of $F_{i\text{-}j}$ appear to be J kg/s from the un-numbered equation right above Eq. 11, which is different from the units from Eq. 1 (W/m=J/m/s).
There needs to be a mass conservation equation, but yet there is not one listed. How can we be assured that this model conserves mass?
Eq. 13 links dissipation to simple velocity differences between adjacent cells. Where does this equation come from? Dissipation is due to stresses, which are divergences of momentum fluxes, which in turn can be thought of as proportional to simple velocity differences (via proportionality coefficients called eddy diffusivities), but there should still be the divergence.
Eq. 12 is the geostrophic balance between Coriolis, pressure gradient force, and dissipation/friction. Why is the specific gas constant R_s in there?

Un-numbered equation about the boundary layer dissipation of kinetic energy D_{ABL} is again unjustified and undocumented. First, KE dissipation rate has numerous terms, how was this one term selected? Also, with a single-layer model, why is this term the dissipation in the ABL as opposed to the dissipation in the entire atmosphere? You cannot differentiate the boundary layer in this model.

3) The resulting 2D fields of heat fluxes are not in agreement with prior maps. Fig. 4 shows very sharp increases in fluxes at the borders between continents and oceans. This feature is not there in Fig. 5 from IPSL-CM5A. What are these jumps caused by? There is clearly something wrong in the treatment of the connections at the interface land/ocean.

4) The wind field is too strong and too noisy at the Equator (as the authors pointed out), to the point that the annual mean speed distribution has its highest peak there (Fig. 7). How can we study kinetic energy dissipation rate with this model if the main features of the wind field are wrong?

5) What methods are new in this paper, compared with the previous Herbert et al. (2011)? I did not have time to read it, but the authors state that the temperature field in Fig. 2 is the same as in Herbert et al. (2011) ("the reader is referred to Herbert et al. (2011) for a mode detailed discussion of the temperature results", p. 417, last sentence). If the results are the same, then what are the "original methods" in this paper (p. 410 first sentence), as opposed to being the same methods as Herbert et al. (2011)? If the methods are different, the results should be different (at least in some details).