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 $F_{ij}$. However, the units of $F_{ij}$ 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 more 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).