Authors' Response to reviewers comments

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Inferring global wind energetics from a simple Earth system model based on the principle of maximum entropy production

1 Point-by-point response to the reviewer #1 comments

General appreciation « 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. »

Answer Our model is not yet to a point where it could give new results, that coupled GCMs would not be able to provide yet. However, we do think it is a valuable approach that is worth publication. May future works solve the main shortcomings, it would become an excellent tool for climate studies. First and foremost, in terms of parametrizations: very few parameters need to be set (and tuned) in the model, as opposed to classical coupled GCMs. Second, in terms of computational time: it is currently about a few seconds on a desktop computer, and should not grow over a few minutes or hours in future versions, to be compared with a complete numerical simulation (of 100 to 1000 years) using a coupled GCM.

1.1 « There is a fundamental inconsistency »

Question « 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? »

Answer A stationary state does not mean an equilibrium state, in the sense of thermodynamics. Thermodynamic equilibrium is defined when all fluxes are null and all state variables are time independent. In the case of a stationary climate model, there is a radiative equilibrium, but this does not constitute a thermodynamic state of equilibrium. Most state variables are time independent, but fluxes are not null. Typically, the total entropy is growing, which proves that it is an out-of-equilibrium, though stationary, state. We refer readers to the relevant literature cited in the introduction, especially O'Brien and Stephens (1995) and Ozawa et al. (2003).

1.2 « Some of the model equations are either flawed or poorly explained. »

Question « 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? »

Answer We did not give the unit of all quantities in the paper, and it seems to induce confusion over some of these quantities. Some equation might be better explained in order to ease the understanding.

To answer specifically to the reviewer's question, the units of the quantities of interest here are:

- A_i are areas of grid cells, expressed in [m^2]
- R_i, the radiative fluxes, are expressed in [W/m^2], for consistency with other models. Note that we actually only need the radiative energy balance of each box: $A_i R_i$, in [W].

- d_i are divergences of fluxes, in the sense that they are the opposite of the rate of convergence of energy in each cell, divided by the area of the cell, thus expressed in [W/m^2].

- $F_{i\to j}$ is an individual energy exchange rates from box i to box j, expressed in [W]. Actual fluxes should be expressed in [W/m^2], and would take into account the area of the exchange surface between boxes i and j. However, to simplify the model, these fluxes are integrated over that area and transformed into a rate of energy exchange, because introducing such extra geometrical parameters is useless for the rest of the model.

Changes to the manuscript We realize that it could lead to misunderstanding of the meaning of some terms, so giving the units of each quantity will be a major change for the final manuscript, together with a more detailed explanation of each term and each equation. All terms introduced in section 2.1 (page 410, line 12 to page 412 line 11) will be introduced with their units.

Question « 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-j} . However, the units of F_{i-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?»

Answer We do not refer to a temperature equation: the temperature in each cell is constant. If we did, we would have to add a term $\frac{c_p}{A_i}\frac{dT_i}{dt}$ to the right hand side of the local energy balance Eq. 1. But this term is null, because we assume the hypothesis of a stationary system with local thermodynamic equilibrium.

Concerning the term $F_{i\to j}$, it has been wrongly defined in the text at line 16. This term is actually defined by a local balance equation: $\sum_j F_{i\to j} = A_i d_i$. The term $A_i d_i$ is a balance of (outgoing) energy rate that we can call divergence (Herbert et al. (2011) used the term convergence for the opposite of that quantity). From a mathematical point of view, the relationship between the $A_i d_i$ and the $F_{i\to j}$ is a discrete divergence operator, defined over a graph, with no specific metric associated with the differentiation, which explains the units. This is the relation that must be inverted to compute the $F_{i\to j}$ field knowing only the d_i field (what we called inversion of the divergence). In that sense, we can write: $Ad = \mathrm{div}\vec{F}$.

Concerning the term $f_{i,j}$, as explained in section 2.3.1, we simply postulate that the energy exchanges between cells, other than radiative processes, are sensible heat exchanges. Thus $F_{i \to j}$, the energy transport rate from box i to box j, must be related to a rate of sensible heat exchange, expressed as something proportional to $c_p T_i$ and $c_p T_j$, where: T_i are temperatures, expressed in [K], and c_p is the specific heat capacity of dry air, expressed in [J/kg/K].

The proportionality coefficient must thus be in [kg/s]: it is the mass exchange rate coefficient $f_{i,j}$. It represents the rate of mass of air at temperature T_i going from box i to box j. Conversely, $f_{j,i}$ is the rate of mass of air at temperature T_j going from box j to box i. Assuming a symmetry in the rate of exchange of mass, we get $f_{i,j} = f_{j,i}$, which immediately leads to conservation of the mass.

Changes to the manuscript First, we propose to introduce in section 2.2.1 (page 412, lines 14-16) an intermediary term $D_i = A_i d_i$ (in [W]), then define the transport terms $F_{i \to j}$ with respect to this rescaled field through the balance equation given above, and then relate it to a divergence equation.

Second, concerning the term $f_{i,j}$, we believe that once the units are clear and $F_{i \to j}$ is clearly defined, the section 2.3.1 becomes straightforward and does not need any modification.

Question « 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. »

Answer Here, we make a strong and important hypothesis: that the dissipation term in the quasi-geostrophic flow equation is proportional to the difference of mean wind velocities between adjacent cells, and that the proportionality coefficient is the *same* mass exchange rate coefficient $f_{i,j}$, as that derived earlier.

Put differently, this hypothesis postulates that the air mass exchanged between adjacent cells not only conveys sensible heat from one cell to the other, but also transports momentum, from one cell to the other. Thus, the dissipation is solely due to a momentum exchange rate balance. The dissipation term $\mathcal{F}_{i,\mathrm{dissip}}$ is expressed in [(kg.m/s)/s], consistent with Eq. 15.

Another way to see it, as the reviewer mentioned, is that the dissipation represents a divergence of momentum fluxes. Our assumption is consistent with this classical way to compute dissipation: due to the discrete nature of our box model, we do not use momentum fluxes, but momentum exchange rates (by integration over exchange surface) $f_{i,j}(\vec{u_i} - \vec{u_j})$, and the divergence operator reduces to a balance equation (Eq. 13), as mentioned in a previous answer.

Changes to the manuscript We propose to stress our key hypothesis concerning dissipation, in order to emphasize the link with the previous part and the importance of the mass exchange rate coefficient $f_{i,j}$ (page 416, lines 15–17). We also propose to stress the conservation of mass (page 417, lines 1–2).

Question « Eq. 12 is the geostrophic balance between Coriolis, pressure gradient force, and dissipation/friction. Why is the specific gas constant R s in there? »

Answer Eq. 12 is the quasi-geostrophic momentum balance equating the Coriolis term to geopotential gradient with added dissipation. We have omitted a simple step in the article: assuming hydrostatic equilibrium along the vertical, we use the geopotential ϕ defined by $\frac{\partial \phi}{\partial p} = \frac{R_s T}{p}$ in the momentum balance. Thus, relating to the sea level pressure p_0 (considered as constant), we get $\phi = R_s log(p/p_0)T$, for a given pressure level p. Thus, at this pressure coordinate, the gradient of temperature is substituted to the gradient of geopotential in the momentum equation, leading to Eq. (12). The use of the pressure coordinate explains the appearance of the specific gaz constant R_s.

Changes to the manuscript We propose to add, prior to Eq 12 (page 416, line 10), a short paragraph stating the vertical hydrostatic equilibrium hypothesis and describing the change of coordinate.

Question « 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. »

Answer Talking about a boundary layer in a model that does not consider a vertical structure for the atmosphere can be quite surprising. However, we consider the interface between atmospheric cells and ground cells: the dissipation of kinetic energy that occurs at this interface can be thought of as a dissipation in a hypothetical ABL, that would be located between these two boxes. Thus, despite the absence of an actual ABL in our model, we define a term that relates, according to us, to dissipation at the boundary: the part of the dissipation term that concerns only vertical exchanges of momentum between ground and atmospheric cells. In fact, for an atmospheric cell i and its corresponding ground cell j, the term $f_{i,j}u_i^2$ is exactly the rate of kinetic energy that is transported from cell i to cell j characterized, again, by the rate of mass exchange coefficient $f_{i,j}$, where as the same amount of air is coming from cell j to cell i but without any kinetic energy, as the velocity in cell j is null. The kinetic energy provided to cell j is thus dissipated at the boundary, hence the definition of our term D_{ABL}.

Changes to the manuscript We propose to define more accurately the term D_{ABL} that is used and explain how we think it is related to ABL dissipation in other models, even though our model doesn't represent an actual ABL.

1.3 « The resulting 2D fields of heat fluxes are not in agreement with prior maps. »

Question « 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. »

Answer Heat fluxes mainly transport energy from the equator to the poles. At a land/ocean border, only the atmosphere cells above can transport heat, as land cells cannot realize the poleward heat transport, especially when the coastline is perpendicular to poleward meridional heat flux. The sharp increase in vertical heat fluxes at such borders in our model is directly correlated to this, while in the IPSL-CM5A model, the high number of vertical layers probably allows for a smoother transition.

Changes to the manuscript We propose to add a paragraph at the end of section 3.2, stating this discrepancy and explaining our understanding of it.

1.4 « The wind field is too strong and too noisy at the Equator »

Question « 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? »

Answer As we explained, our method becomes unstable near the equator, which leads to solving an ill conditioned system, resulting in spurious, strong components of the wind field at very low latitudes. However, this anomaly is rather localized to one or two latitude coordinates in each hemisphere, and its influence on global energetics features is not critical. For instance, if we discard the meridional component of the wind and artificially impose a zonal component of 9m/s (the global mean of the in the 4 latitudinal bins that are closest to the equator), the global mean kinetic energy in the atmosphere is only reduced by 20%. It is much lower that the range of other estimates, so we think this anomaly is still acceptable, in the present state. Ongoing works are also directed toward stabilizing the numerical problem near the equator.

Changes to the manuscript We propose to add a sentence stating this quantitative estimate of the spurious wind components generated close to the equator, after the line 27, page 418.

1.5 « What methods are new in this paper? »

Question « 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). »

Answer This paper is based on Herbert et al. (2011) and Herbert (2012). It is intended as a follow-up to their work as it uses most of their model. It is therefore hardly completely independent. However, to make things clear, we can say that Herbert et al. (2011) went to the point of determining the temperature field, top-of-the-atmosphere radiation budget, and total meridional energy transport with a MEP climate model. In Herbert (2012), the inversion of the flux divergence to recover the repartition of energy transport between ocean and atmosphere is already proposed, but it is only accessible in French. First, we reuse this flux inversion technique and explain it. Second, our main original contribution is to derive a wind field from both the energy transport distribution and the temperature field.

Changes to the manuscript It seems that the border between pervious works reported in the literature and the present work is not clear in the manuscript, so we propose to improve it by clearly stating in the introduction our original contribution, and how it is related to previous works.

2 Point-by-point response to the reviewer #2 comments

2.1 What's new

Question « Where does Herbert et al. (2011) end and this one begin? I lose confidence regarding what is new here when I dont see this clearly defined anywhere in the text. »

Answer See last question of reviewer #1.

Changes to the manuscript See last question of reviewer #1.

2.2 Dry atmosphere

Question « 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) »

Answer The radiative code uses standard humidity profiles to compute the radiation coefficients. However, this is the only place where water is accounted for. There is no hydrological cycle at this stage in our model.

Changes to the manuscript We propose to clarify the sentence p.415 line 5 and p.416 line 3, by stating that latent heat exchanges are not taken into account and the hydrological cycle is not represented.

2.3 Vertical structure

Question « [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. »

Answer We simply wanted to underline the fact that there is only one vertical level for the atmosphere. Thus, the derived wind field is to be compared with vertically (mass) averaged wind field for multi-layered models.

Changes to the manuscript If the footnote is not clear, we propose to simply remove it.

2.4 Resolution change

Question « 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 Im being shown results? »

Answer The wind field figure in higher resolutions was hardly readable, so we chose to present it only in the 36x48 resolution. When the resolution increases, the main difference on the wind field is what happens near the equator where the method is, at this stage, unstable. The rest of the wind field seems quite similar, and is simply better resolved.

Changes to the manuscript We propose to chose one resolution and stick to it: 36x48. We will, however, keep our remarks on the instability of the method near the equator, as it is a key point to overcome in future works.

2.5 Atmospheric boundary layer

Question « What? I dont understand how the one level atmo- sphere 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 derive is 400-800 TW. The rates seem ok, but again, I do not see how you get there at all. »

Answer We answered a similar question from reviewer #1, but let us clarify again. The term "atmospheric boundary layer" might be abusive here. Indeed, we do not have an ABL in our model, but we can still compute a rate of energy dissipation at the boundary between atmosphere and land/oceans. This is the closest quantity that we can refer to, and it seems to be in pretty good agreement with actual ABL energy dissipation rate in other models.

Changes to the manuscript We propose to better define the term D_{ABL} at the beginning of section 3.4.3, and stress that, though there is no actual boundary layer, there is a boundary, at which a dissipation occur in the model.

2.6 Poleward heat transport

Question « 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? »

Answer These are the same model, but the figure in Herbert et al. (2011) represent the *total* meridional heat transport, as the oceanic and atmospheric transport are not separated yet. The zonal mean, total meridional heat transport is directly available from the radiative budget by integration along the latitude coordinate. To distinguish between oceanic and atmospheric transport, we propose a new method (inversion of the Laplacian) that has not been published yet, except in Herbert's PhD thesis (only available in French). So, in both cases, a 50% increase on the total meridional heat transport is needed if we refer to Trenberth and Caron (2001).

Changes in the manuscript We propose to add the total meridional heat transport to figure 3, so that it mimics figure 2 in Trenberth and Caron (2001), and it can be easily compared to figure 4a in Herbert et al. (2011).

2.7 Units

Question « 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. »

Answer We agree that it would be easier to follow if all units where given at each stage of the model description, as reviewer #1 suggested.

Changes in the manuscript We will give the units of each term, so that it becomes more clear.

2.8 Wind power application

Question « 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? »

Answer Determination of the available wind power and effects of large scale wind farms on the climate is an interesting and important question, and has become a recurrent subject in recent literature: Gans et al (2010), Miller et al. (2011), Marvel et al. (2012). This constitutes, from the authors' point of view, an interesting motivation as a potential application of a more complete MEP climate model. Current results rely on weeks of intensive high performance computation, needed to integrate the dynamics of a coupled GCM on long time series, to finally average all these data to derive climate related quantities. If the MEP hypothesis was to give directly these quantities, it would be a huge step forward for climate studies, and this particular application could directly benefit from it.

Changes in the manuscript In the main sections (ie. past the introduction), we propose to emphasize the construction and the philosophy of the model, rather than the potential applications.

2.9 MEP hypothesis

Question « What makes the Maximum of Entropy Production (MEP) principle (or is it an ap- proach) so applicable to this models intention as to include it in the title, abstract, etc.? »

Answer The model only rely on the MEP hypothesis, geographical data (land/sea mask and albedo), and a radiative code. This is extremely simple, compared to the numerous parametrizations needed in a typical coupled model, and this simplicity is mainly due to the use of the MEP hypothesis. The authors think that it is useful to underline the use of the MEP hypothesis, as it is not often used, nor well known.

2.10 Zonal winds

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

Answer Figure 7 shows the zonal mean of the wind speed $\|\vec{u}\|$ vs. latitude. It is not clear from this figure which component dominates in the anomalous peak near the equator. However, figure 6 shows very strong meridional component near the equator, above South America and Africa. The zonal wind also shows an anomalous peak near the equator, and strong zonal components are present over Africa, at the equator.

Changes in the manuscript We propose to modify our sentence p.418 line 25-27 and to mention a "strong wind speed" instead of a "strong meridional component", so that neither component is preferred, as both components actually show anomalous values.

2.11 Gridded data

Question « 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 »

Changes in the manuscript We provide new figures with gridded data.

2.12 Colorbar ranges

Question « 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 »

Answer We agree with the reviewer. Data was not accessible in the same format and different tools were used to plot MEP data and IPSL data. Even though we tried to use colormaps as identical as possible, the result was not entirely satisfying. This technical issue has been solved.

Changes in the manuscript The new figures we provide have been plotted with the same tool, and thus use the same color map and colorbar ranges.

Things I would like to see more of

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

Answer Ranges were given through the Results section (section 3), except for the temperature field and heat flux field, which show very little variation with resolution. Ranges and comparison to other estimates are given in sections 3.3, 3.4.1, 3.4.2 and 3.4.3.

Changes in the manuscript We propose to mention the fact in section 3.1 and 3.2, that this part of the results is not resolution dependent.

Question « 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 atmospheric energetics first »

Answer With all the improvements proposed to answer the issues raised by the reviewer, the authors are confident that the paper will give a clearer view of the model and its applicability.

Changes in the manuscript As stated earlier, the bulk of the paper will concentrate on the model and potential applications will simply be mentioned in the introduction and conclusion.

3 New figures

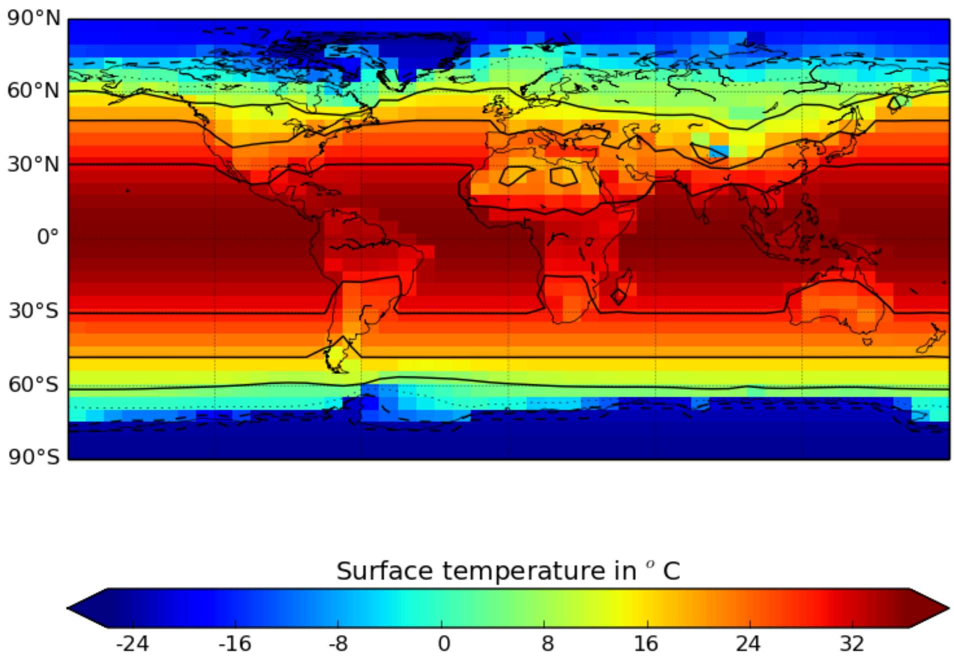

Figure 1: MEP climate model surface temperature (gridded data, 36x48 resolution).

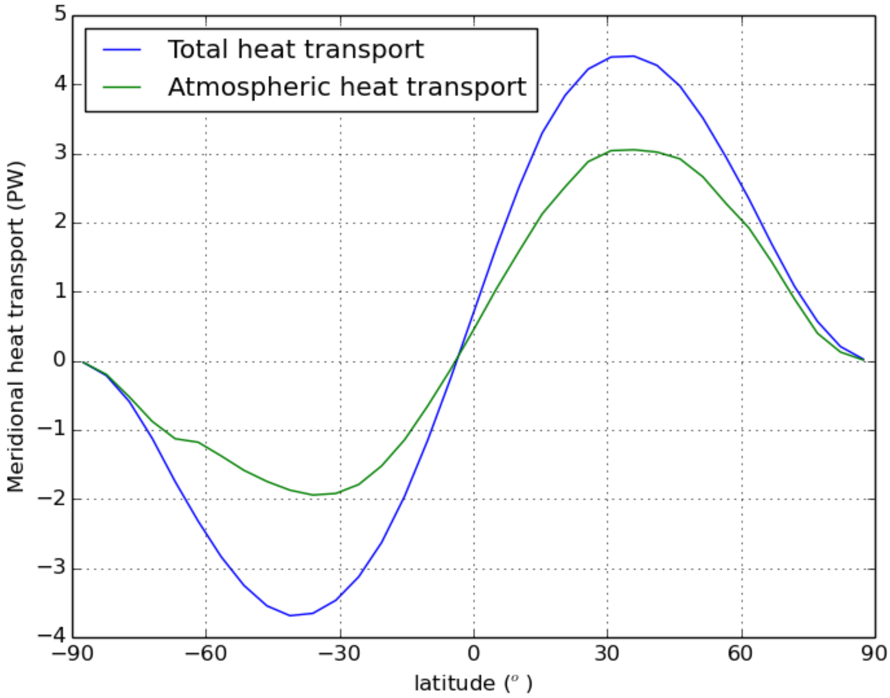

Figure 2: Poleward meridional heat transport of the MEP climate model: total and atmospheric transport

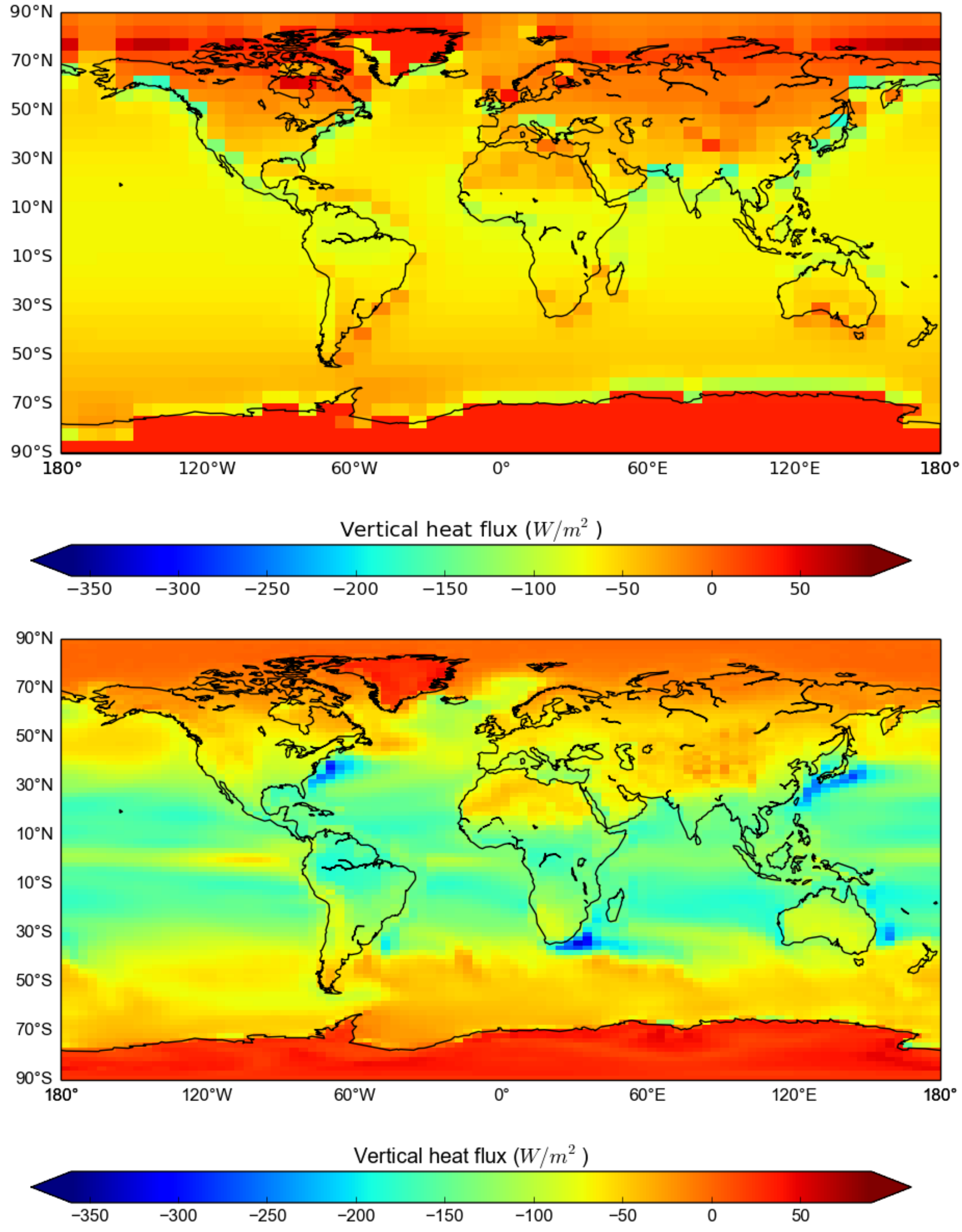

Figure 3: Surface heat flux (gridded data): MEP climate model at 36x48 resolution (top), and IPSL-CM5A at 96x96 resolution (bottom).