We thank the reviewer for the time she/he took and for the very helpful comments provided, which will help us to improve the manuscript. A pointwise reply to the reviewer's comment is given below.

1.) *Equations 3, 4 and 8-10, give a diagnostic value of the 5 variables, given a forcing field of temperature and humidity. These are not prognostic equations, they don't give a time evolution, despite what said in section 2.1. This is ok, but then why are figures 1 to 5 "monthly means": they would each show field diagnosed from the forcing fields specified in section 3. There is a time evolution for the transient kinetic energy u and v and of the momentum flux, <v'u'>, indeed, so I don't understand how these articulate with the diagnostic equations. Is the above correct*?

    Yes, Eqs. 3, 4, 8 - 10 are not prognostic equations. They describe how the state of the model is calculated from the input data (surface temperature, humidity, and cumulus cloud amount). The input data is given as monthly mean data, therefore figures 1 to 5 show the mean state of the model for a given month.
    The transient kinetic equations are also diagnostic equations and the completely derivation of the diagnostic equations has been described in Coumou et al. (2011).

    We have rewritten the manuscript to state this more clearly.

2.) *In fact the 2D equations of Petoukhov et al (2000) for temperature and humidity are prognostic equations, but they are just mentioned at the beginning. Are you integrating these equations along with the equations of the kinetic energies? This is not what it seems to be implied at page 6 line 5. And also, if so, how does forcing comes in?*

    No, we are not integrating the equations for temperature and humidity.
    The scope of this study is to describe and test a new set of equations of the dynamical core only. The prognostic equations for T and humidity and the diagnostic equations for EKE have been described and validated in previous work (notably Petoukhov et al, 2000 and Coumou et al, 2011).

    Therefore, in order to make the manuscript not larger than needed, we prefer to only reference those publications, rather than providing the full derivation again.

    We will rewrite this part to make it clearer.

3.) *As you see these are all very basic doubts that clearly come from a bad structuring of the paper. Note also that the supplementary material is not well articulated with the text. The text should contain enough information to understand the basics (like my doubts above). As for now, the derivation of the equations are divided in the two parts -test and supplement - in a chaotic way. Also note that a section 2 of sup. material is referenced in the text, but it's not in there*

    We agree with the reviewer that we did not sufficiently explain our approach. Therefore we have rewritten the main text such that it has enough information to understand our general approach and also better link it to information in the Suppl. Mat. .
    We have described the model setup and the experiment in more detail and also have explained already in the abstract the novelty of our model.
    In addition, we added Suppl. Mat. 2.

4.) *In addition to the clarity problem, which is in itself bad enough to require a major revision of the article, there is another point that is not clear to me. The Aeolus model as it is presented has already been published in Coumou et al 2011. Is the coupling with the convection model , or the coupling with the temperature and humidity 2D equations of Petoukhov et al (2000) the novelty? Is it the optimization of parameters? Please state this clearly. I have to say that the optimization does not appear to have such a major impact to me.*

From a theoretical point of view, the novelty is the newly derived statistical dynamical equations of the large-scale zonal-mean field and the planetary waves, and their embedding in the model's dynamical core.

From a technical point of view, we present a detailed parameter-optimization scheme to validate the dynamical core against observations. This is also presented the first time.

5.) *Note also that the method of optimization (simulated annealing) should at least be schematically described.*

We will provide the following schematic plot of the optimization process in the Suppl. Mat. as well as an additional reference in the main text (Kirkpatrick, 1984).

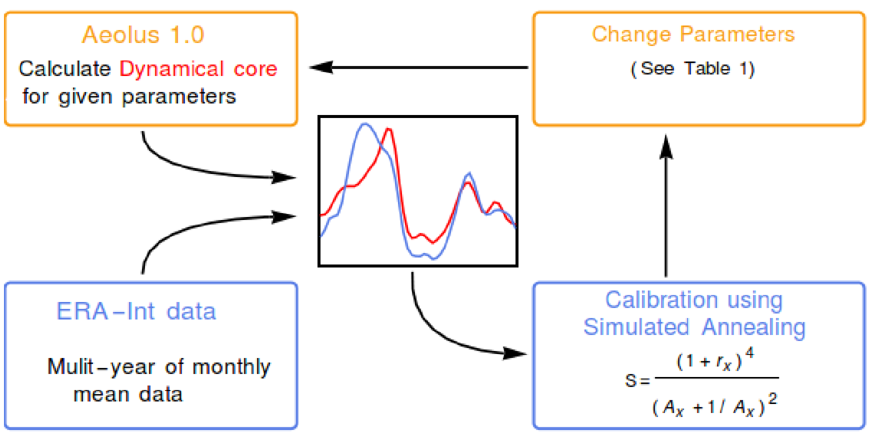

Figure 1 Schematic plot of the optimization process: The dynamical core is calculated for given parameters (presented in Table 1). In order to find the optimized parameters, we cailbrate the dynamical core with Simulated Annealing and using ERA-Int data to construct the skill function.

6.) *page 2 line 32 "convective plus 3 layer stratiform" What does this mean?*

It means, that our cloud model simulates 3 types of stratiform clouds, at low-level, mid-level, and upper-level (as described in detail in Eliseev et al, 2013).
The fourth cloud type represents convective (cumulus) clouds. In the equations for the dynamical core, only cumulus clouds are considered. We will clarify this in the text.

7.) *Section S1.2 "With K_z = 005 and In (4)" incomprehensible*

We will change that sentence to: With $K_z = 0.005\ z$ and

$$A = \frac{\mathcal{L}\overline{\langle P_{co}\rangle}}{H_0}\frac{\overline{\langle u_{sf}\rangle}}{\Gamma_a - \Gamma_0 - \Gamma_1(T_a - T_0)\left(1 - a_q q_s^2\right) + \Gamma_2 n_c}$$

8.) *Supp. mat. at the bottom. Is the independency of the large scale and synoptic waves a reasonable assumption? Comment.*

This sentence was phrased incorrectly. Large scale und synoptic waves are not independent. Due to a “gap” in the three-dimensional (period-wavelength-phase velocity) spectrum of atmospheric processes (see, e.g., Fraedrich & Böttger 1978, Coumou et al. 2011), the interaction of the synoptic-scale wind component with the large scale long-term wind component (on time scales of about 10-20 days and longer) could to a first approximation, be represented in terms of its ensemble (statistical) characteristics (the second and higher-order moments), and not in terms of the individual eddies (Saltzman, 1978).
Because of this gap between the two spectra, it can be assumed that the long-term component is nearly constant over synoptic timescales.
Hence, the equation can be written as:
<xy'>=x<y'>=x*0=0, again this is explained in detail in Coumou et al. 2011.

We will rewrite this part to make it clearer.

9.) *Repetition page 4 sup mat. Paragraph “The contribution to the vertical…”*

We will remove the repetition.

10.) *Page 4 of Supp. Mat. The scale analysis attests,have you done the scale analysis,or is taken from literature?*

This scaling analysis is described in:
Petoukhov V, Ganopolski A, Claussen M (2003) POTSDAM - a set of atmosphere statistical-dynamical models: theoretical background. Potsdam-Institut für Klimafolgenforschung, ISSN 1436-0179, 136 pp, http://www.pikpotsdam.de/research/publications/pikreports/.files/pr81.pdf

Using the magnitude analysis

$$\langle\overline{w}\rangle\frac{\partial\langle u\rangle}{\partial z} = H^*\left(\frac{u^*}{L^*}\right)\left(\frac{\langle u\rangle}{H}\right) \ll 1$$

where
H = 10^4m is the atmospheric density vertical scale,
u* =10 m/s is characteristic scales of the planetary wave velocities
L*= 3*10^6 are the characteristic scales of planetary horizontal lengths, and H*=H Ro*, where Ro*=u*/(L*f) is the Rossby number for the planetary waves.

11.) *Page 3, eq.3, could we call it geostrophic and thermal wind balance?*

Yes, it could be called thermal wind balance, we will add a sentence in the manuscript.

12.) *Page 3, formula for the meridional pressure, where does that come from? Please describe it more carefully.*

It is derived from Pethoukov et al. (2000) eq. (13). In order to make the manuscript not larger than needed, we would like to only reference those equations, derived in other publications rather describe them again.

13.) page 3 line 9 "Supl.Ment"

We will rewrite as suggested.

14.) Page 3 line 25 repetition, reword.

We will remove the repetition.

15.) *page 4 line 5. In fact the parameters gamma and a_q are not at all explained in the table. just listed along with their value*

Gamma is the lapse rate equation, which is assumed to be linear (Petoukhov et al, 2000):

$$\Gamma = \Gamma_0 + \Gamma_1(\mathrm{T_a} - \mathrm{T_0})\left(1 - \mathrm{a_q}\mathrm{q_s^2}\right) - \Gamma_2\mathrm{n_c}$$

We will add this in the manuscript.
The parameters are all described in Table 1.

16.) *pag 4 line7 is n_c constant or is it computed? If it is a constant, what's it value?*

We used observational data: Multi-year averages of monthly mean, El Niño and La Niña months cumulative cloud fraction is taken from ISSCP (Rossow and Commission, 1996). We will add a sentence in the manuscript to make that clearer.

17.) *pag 4 line 9, is U_sf the same as U_Sprofile in the supplementary material? if so, it is not clearly explained, what does "The additional calculating of U_sprofile instead of using the calculated surface zonal velocity is done to avoid instabilities." mean?*

Yes, U_sf is the same as Us_profile. We will rewrite that in the manuscript. Instabilities can emerge due to the strong positive feedback between the meridional temperature profile, surface winds and vertical wind velocity, which can lead to high latent heat release (moisture-convection feedback) (Grabowski and Moncrieff, 2004). In nature these would be damped out but due to the fixed troposphere height in the model, we have to parameterize it.

18.) *Page 4 last line. There is no S.2 in the supplementary material*

We will include Suppl. Mat. 2.

19.) page 5 line 19 "equipartitioned

We will rewrite it.

20.) *in the supplementary material, the explanation of eq.4 is not complete, it is not shown why the introduction of coefficients d1 d2 and d3 is necessary and how they are chosen*

The derived equation for the meridional velocity does not account for latent heat release associated with convective precipitation. To capture this additional term we include convective precipitation and finally introduce tuning parameters, which have values close to 1.
We will add that part to explain it better.

21.) *Page 4 last line. There is no S.2 in the supplementary material*

We will include it.

22.) *Note also that the supplementary material is not references, page numbers, line numbers*

We will change that in the updated manuscript.