The authors would like to thank the anonymous Referee 2 for her/his careful reading of our manuscript and for finding it consistent and interesting. We appreciate that he/she recommends minor revision and we have considered all of the specific comments done in the review.

1. <u>Specific Comments:</u>

- **<u>Section 2.1:</u> which kind of simulations have been performed with GCW? Which SSTs have been used and how long are the simulations? Which GHGs, aerosol, O3 etc. . . ?**

A short description on the simulation used in the paper has been added in the Section 2.1 *"Model description"* .

- **Are the 1997-2008 period the same for the LMDz model simulations if observed ssts have been used?**

The LMDz simulations correspond to the period 1986-2005 whereas the GPCP datasets cover the period 1997-2008. Results from the model concern a period twice larger than the GPCP ones, but we prefer to keep the entire LMDz period to increase the statistical significance of our results. On the same way, the LMDz simulations do not exactly overlap the ERAI period analysed (1989-2009), but the difference concerns five years only.
To answer Referee 2 question, we mentioned in the manuscrip that the LMDz runs cover the period 1986-2005.

We have added the following sentence in the manuscript to answer to the two above Referee 2 questions:

"Both simulations are forced by SSTs, sea-ice cover, ozone and carbon dioxide that vary annually corresponding to the 1986-2005 period" **(l. 87-89)**

- **How are those spectra in agreement with those derived from the OLR (see Maury et al 2011) ?**

In Maury et al. (2011) , we used the OLR field as a proxy for convection, essentially because the OLR data cover a longer period than the GPCP data (almost three time longer). This favors the detection of marginally significant signals between CCEWs and SEWs in the reanalysis. Meanwhile, we found that OLR and precipitation are not as well correlated in LMDZ as in observations. So we preferred using precipitation data which are a more direct proxy of the convective activity. We added a reference to Maury et al. (2011) in the manuscript.

"Note also, that OLR LMDz spectra can be found in Maury et al. (2011) for the Emanuel scheme (1991 & 1993)." **(l. 103-104)**

- **Section 4.2 and 4.1, in order to better compare with reanalysis, given the role of wave filtering by the wind, why not performing this analysis only in a specific qbo phase in era-interim?**

We agree with Referee 2 on the significant role of the wave filtering by the wind. By considering the wave propagation theory, our composite method naturally selects each kind of waves during the more favorable QBO phase. The KWs (with positive phase speed) easily propagate during a westward QBO phase (negative zonal wind at 50hPa). On the contrary, RGWs (with negative phase speed) easily propagate during a eastward QBO phase (positive zonal wind at 50hPa). As a

consequence, in the reanalysis the KWs (or RGWs) dates are selected during negative (or positive) QBO phase. Given the continuous negative zonal wind in LMDz, the KW composites are directly comparable with those from reanalysis, but the RGW composites are not . To strictly compare the RGW composites from LMDz and from ERAI, we perform another composite analysis by picking dates during negative QBO phases in ERAI.
The results of this composite analysis are presented in manuscript Fig. 8, which is now more properly described by adding the following paragraph:

"... However, several pieces of evidence indicate that the continuous negative zonal wind in LMDz lower stratosphere dynamically filters the RGWs. The first one is given by the longitude-time plots in Fig.7b and Fig.8b, which show that the negative absolute phase speed of the waves in LMDz is larger in amplitude than in ERAI. In the model, the absolute phase speed of the RGWs needs to be larger to maintain a subsequent negative intrinsic phase speed when the background zonal wind is negative. The second one is given by the zonal-vertical profiles of the RGWs, which show that the RGW packets do not propagate above 30hPa in the model (Fig.7c) compared to those in ERAI (Fig. 8c). The RGW vertical wavelength is smaller in LMDz (Fig. 8c) than in ERAI (Fig. 7c) due to the wind filtering, and is likely too small to be properly resolved by the model. Finally, we recall here that our composite method only selects dates in the ERAI RGW index during positive QBO phase (cf. Lott et al., 2009). Thus, the LMDz and ERAI RGWs composites are not directly comparable. To strickly compare both RGW composites, we perform another composite analysis by picking dates in the ERAI RGW index for negative zonal wind at 50 hPa, i.e. during negative QBO phases. The resulting composites (Fig. 9) are very similar to those from LMDz (Fig. 8). Under such dynamical conditions, the weak amount of RGWs in the reanalysis can be attributed to the wind filtering only. This result is supported by recent work with a new LMDz version with a QBO (Lott et al., 2012), where it is shown that the simulated RGWs are improved with positive zonal wind in the lower stratosphere. It confirms that the model potentially simulates the right amount of RGWs, despite the misrepresentation of the tropospheric convection variability." **(l. 273-291)**

We hope that the above paragraph also answered to Referee 2 next point:

- **Figs 8-9, I have missed the discussion about panels c)**

- **Section 6, lines 10-12: her you suggest that there exist other sources than equatorial convection because of the disagreement in the precipitation spectra between model and observations, however, above this was explained as the role of filtering by the wind. Could you clarify your point?**

According to the linear theory, the wave amplitude depends on both the amplitude of the sources and the subsequent filtering. To clarify our approach, the following paragraph has been added to the end of Section 5.2 entitled "Rossby-gravity waves":

"According to the linear theory, the wave amplitude depends on both the amplitude of the sources and the subsequent filtering. Given the wind filtering, Section 5.2 confirms that LMDz simulates realistic stratospheric RGWs and KWs while Section 3 shows that the tropospheric convection variability is underestimated in the corresponding wavenumber-frequency window. In the linear view, the wave amplitude is directly related to the sources amplitude, indicating that the model has other sources than equatorial convection that can be substantial enough to supplement the lack of the convective forcing." **(l. 293-299)**

- **page 22620, lines 4-6. Could you please specify that those are again composites on the KW index ?**

To answer the Referee 2 question, we use the answer to Referee 1.
We clarify the composite method by adding at the beginning of Section 5 « Composite analysis » :

*"To characterize the spatial structure and the life cycle of the SEWs, we follow Lott et al (2009) and make a composite analysis of band-pass filtered fields. For the Kelvin waves, the band-pass filter operates in the frequency-wavenumber Fourier space, by multiplying the Fourier components of all fields by a transfer function that largely contains the broadband spectral maxima associated with Kelvin waves (Fig.4), and guarantees that the filtered fields include them well. To finalize the filtering we then return to physical space. To diagnose when a Kelvin wave is present at 50hPa, we evaluate an index whose value equals the maximum of the filtered Temperature averaged between 10°S-10°N, and identify the longitude λ_m at which this maximum occurs. The composites are then built from averages over dates when maxima of this index exceeds a given threshold and shifting the maps selected by λ_m . We also average the dates at various lag before and after the central dates, so our composite are 41-day long. In each dataset the threshold is chosen so that the number of cases selected equals the number of years in the dataset. We choose here to select a rather low number of events to guarantee independence between the selected wave packets, bearing in mind that each wave packet can have a life cycle that lasts near a month. To ensure that the same wave cannot be selected twice, no day within 20 days after a case event can be selected. Finally, we have tested that none of our results are affected by moderate changes in the thresholds or in the filters (for instance, including more horizontal wavenumbers)***.** *In the following, the composite of a filtered dynamical fields X is note $\widetilde{X}^C$ "* **(l. 226-245)**

In same way, this new paragraph may answer the following question:

- **Section 5: lines 9-10. Could you add at the end something like "as explained below" ?**

To be consistent, we also changed the paragraph that explains how the KWs EP-fluxes have been calculated.

"To locate the sources for the KWs, we next evaluate the EP-flux (Eliassen and Palm, 1961) of the KW composites presented in Fig. 6, and adapting Andrews et al. (1989):

$$\overline{F^{\phi}}=\rho_0 a\cos\phi\left(\overline{u}_z\frac{\overline{v^C\theta^C}}{\overline{\theta}_z}-\overline{u^C v^C}\right) \quad , \qquad (5)$$

$$\overline{F^{z}}=\rho_0 a\cos\phi\left(\left(f-\frac{(\overline{u}\cos\phi)_{\phi}}{a\cos\phi}\right)\frac{\overline{v^C\theta^C}}{\overline{\theta}_z}\right)-\overline{u^C w^C} \quad . \qquad (6)$$

Here $\overline{u}$ and $\overline{\theta}$ refer to the zonal mean composite of the unfiltered zonal wind and potential temperature respectively. In our context, the composite fields $\tilde{u}^C, \tilde{v}^C, \tilde{w}^C$ and $\tilde{\theta}^C$ are used as disturbances, which is justified for the filtered fields because none of our band-pass filters keeps the s=0 component." **(l. 301-307)**

- **The composites do not show any significance level, this could be especially important for the EP-fluxes, as it is the first time a see those kind of composites (e.g. EP fluxes on KW and RGW indexes). For the EP fluxes, could you also add a panel with a climatology ?**

A 99% significance interval for the EP flux has been added in grey shaded areas in Fig.~10.

- **For the EP fluxes, could you also add a panel with a climatology ?**

To our understanding, to obtain climatologies of KWs EP-flux, one should filter the data to keep

KWs. This may be what our composite EP-flux method induces (as composites maps are close to correlation maps). An other way to obtain such climatologies could be to calculate the EP-flux corresponding to each disturbances without prior data filtering.
In this case, the contribution from waves with negative phase speeds and positive phase speeds would in part balance. We feel that such approach is not adapted to our study and goes beyond the scope of the paper.

- **In order to understand composites, it could be important to know if there is any trend in GHGs or O3 etc. . . and to know if there is any trend on the indices and Epfluxes (in reanalyses as well). For example, does the large signal on the EP flux at 50S, have anything to do with trends on the tropospheric eddy driven jet? Are we just correlating trends when performing the composites (trend in the indices and trend in the EPfluxes)? Or is there a real physical mechanism?**
  Moreover, have you seen if in those trends the largest contribution is from the wave 1-2 ? I mean, if you separate EPfluxes from wave 1-2 w.r.t EP fluxes from synoptic waves (e.g. 5 and highers) and then you composite, do you see their contribute comes from different regions in the SH?

Note that all of our data are detrended before doing composites. Note also that we know from other model analysis with fixed SSTs, ozone, and sea ice (Maury et al.(2011)) that the results in the present paper do not differ much than those obtained with a fixed climate.

The trend Referee 2 refers to might be a negative trend that originates from a reduction in the positive upward EP flux carried by the mid-latitudes large scale Rossby waves under climate change conditions. In our study, the negative value of the vertical EP flux observed in Fig. 10 is not a signature of any trend, but a dynamical property of KWs that bear a positive intrinsic phase speed.

We hope that our answer to Referee 2 question helped in making our manuscript clearer. But we did not find the right place to add more information in the manuscript without adding any confusion.

2. Specific Comments concerning the Figures

- **Fig1: how many years of GPCP have been use to create fig 1?**

The GPCP datasets are used over the period 1997-2008. We added the information in the Fig. 1 caption.

- **Fig 2-3: Are these kind of spectra already been published on GPCP? Or are these in agreement with similar spectra published on other datasets?**

These kind of precipitation spectra have already been published in Lin et al. (2007) for the GPCP datasets. These spectra are also in good agreement with those published by Cho et al (2004) using the TRMM rainfall and with the OLR spectra published in the Wheeler an Kiladis (1999) as it refers in the manuscript.

Reference:
Lin, Jia-Lin, Myong-In Lee, Daehyun Kim, In-Sik Kang, Dargan M. W. Frierson, 2008: The impacts of convective parameterization and moisture triggering on AGCM-simulated convectively coupled equatorial waves. *J. Climate*, 21, 883–909.
doi: http://dx.doi.org/10.1175/2007JCLI1790.1

- **Would it be possible to add the period on the right axis?**

The periods have been added on the right vertical axis in all the spectra.

3. <u>other typos/minors:</u>

- **page 22609, line 26: " the differences with the models in Horinouchi et al. (2003) are not just their convection schemes" –> within the models? among the models ?**

The sentence has been modified:

“This result needs to be further analysed because the models used by Horinouchi et al. (2003) do not only differ by their convection schemes. As a consequence, the differences found between SEWs cannot be directly attributed to the used convection scheme.” **(l. 48-51)**

- **p 22610, line 3: analyses –> analyse or analyse (it’s a verb)**
- **beginning of sections: 4.2, 4.3: The Fig. 5, The Fig. 4 –> Fig.5. . . Fig.4. . .**

We fixed both errors.