

Interactive comment on “Atmospheric transport and chemistry of trace gases in LMDz5B: evaluation and implications for inverse modelling” by R. Locatelli et al.

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Dear,

We are very grateful to the referee for his careful review of the manuscript and his suggestions to improve the text. Here we respond to the referee point by point.

Robin Locatelli

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Comments

- **p. 5007, line 27: How do you know "deep convection" actually occurred during these days, and it did not carry Rn up? Otherwise this statement is misleading, and this one of the examples why insufficient analysis may not be very helpful. As you noted in the paper site representation errors are a major issue when we compare model results with observations at the surface stations. Horizontal transport is very important too, and I am curious how the horizontal winds (synoptic and diurnal) are simulated by model at these sites.**

Deep convection simulated by LMDz does not occur during these days because convection trends (blue line on Figure 5) are null. We have also checked meteorological data at stations close to Lutjewad to see if deep convection has been observed during these days. In particular, we have looked at the amount of convective precipitations between the 1st and the 15th of April 2009 and we didn't find any convective precipitations. We clarify this point in the text.

Concerning horizontal winds, we did not clearly specify in the original text that horizontal winds were nudged towards ERA-Interim reanalysed winds. We add a sentence in the revised text in order to clarify this point (page 5002 l.7) : "Moreover, horizontal winds in all versions are nudged towards ERA-Interim reanalysed winds". Consequently, horizontal winds used in LMDz are very close to ERA-Interim fields, which have quite good scores at least in the northern hemisphere.

- **p.5010, line 1-2: Please mention the source of SF₆ emissions.**
We specify sources of SF₆ emissions by saying : "SF₆ is an anthropogenic compound, which is mainly produced worldwide in the electrical industry (transform-

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ers, circuit breakers, etc.). Indeed, SF₆ is used in equipment for electrical transmission and distribution systems and in the reactive metals industry (Maiss and Brenn, 1998). According to EDGAR inventory (<http://edgar.jrc.ec.europa.eu>), SF₆ global emissions reached 5.5 Gg in 2005. Moreover, SF₆ has been widely used to study atmospheric transport (Denning et al (1999), Law et al (2008) and Patra et al. (2009)). Indeed, since it is inert, SF₆ is very long-lived in the troposphere and stratosphere (between 800 to 3200 years; Ravishankara et al. (1993) and Morris et al. (1995)), which makes SF₆ a powerful tool for assessing large-scale modelling of transport processes in the atmosphere (Maiss et al. (1996))".

We have also specified which SF₆ emission dataset we are using in this study: "SF₆ emission fields are taken from EDGAR 4.0 and scaled to Levin et al. (2010) as it has been done for the TransCom model intercomparison of Patra et al. (2011)."

- **p.5011, line 13: I agree, that's a long standing problem for transport modellers. But if you can choose background air sampling conditions and as long as the same level is chosen for different model versions, you should be able to discuss NP-SP-TD differences for interhemispheric transport. You may also consider using aircraft measurements.**

Here, different versions of the model using different vertical resolutions are compared. Then, the model layer chosen to represent a specific station differ between models. In the diagnostic presented in Patra et al. (2011), the inter-hemispheric exchange time is computed using South Pole and Cape Grim to represent Southern Hemisphere air and Barrow and Mauna Loa to represent Northern Hemisphere air. In particular, Mauna Loa is a station located at ~ 3400 meters above sea level, on a volcano. The "best" model layer to be extracted for high-altitude stations, in the sense of a minimisation of model/observations mismatch, is often an issue. Moreover, the proper layer may differ between model versions depending of their vertical transport characteristics for instance.

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We found that this uncertainty due to the sampling of the level was bigger than the gap in the inter-hemispheric exchange time found for each version of the model. We think that this issue does not cause major problem in Patra et al. (2011) because differences in IH exchange time are large between different models (the spread reaches ~ 0.8 years). Here, although different parameterisations are tested we only explore part of the model error spread, which makes this diagnostic less relevant. Moreover, we tried to change the surface stations used to compute the diagnostic and, one more time, found differences related to the choice of the station of the same order of magnitude than the difference in the IH exchange time derived by our models. This is why we chose not to use this diagnostic. A few sentences have been added in the revised version of the paper about this issue.

- **p.5011, line 25: I would argue for adjusting the values in reference to the southern most site - as is typically done in TransCom analysis, which I think reveal the IH differences well although care should be taken to define the offsets.**

When adjustment of inter-hemispheric difference is done with South pole as a reference, changes seem to occur and to be due only to the northern hemisphere. That is why, after discussion, we have decided to adjust the values in reference to the mean of all sites. This allows to overcome a possible over-emphasis on northern hemisphere transport only.

We now also precised in the text the different offsets applied to each version of the model in the text.

- **p5013, line 23: which emissions are used in these emissions, CASA monthly? or something diurnally varying? Such information are required**

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to imagine the causes of model - observation differences.

Yes you are right, we did not specify which emission field we used. We used CASA fluxes with monthly resolution and a zero annual mean flux everywhere. This field is coming from Randerson et al. (1997). We have specified it in the text.

- **p5020, line 10ff: What one could do is check the synoptic or hourly model-data difference for sites individually and then apply an appropriate observation error to each site at the time intervals of the assimilation windows. This may be updated continuously. Here we assume that longer term biases, compared to the assimilation window, are subject to correction. Do you have any recommendations?**

Specification of errors included in inversions is still an issue at present. Indeed, several recent studies have proposed statistic methods to quantify prior and observation errors used in inversions (Berchet et al. (2013), Cressot et al. (2014) and Ganesan et al. (2014)). These methods are very promising but they are very time-consuming and they can not be fully applied to global atmospheric inversions. In practice, one can also use proxy methods. Bergamaschi et al. (2010) propose to look at spatial variability of concentration around surface stations to find errors. It is also possible to use the method you propose based on sub-temporal variability in the model world. However, regardless of methods used to compute observation errors (statistical or proxy), one needs to check afterwards if errors are consistent with the capabilities of the model to reproduce the observations. For instance, we know that TD versions of LMDz has difficulties to properly reproduce high peaks of synoptic variations close to source regions (Geels et al., 2007). Therefore, observations errors associated with such peaks must be large enough not to convert a model-observation mismatch due to transport issues into an unrealistic change in surface emissions only. We think it is essential to check the consistency between observation errors and skills of

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the model before running any inversions. We add a few sentences based on this recommendation in the revised text.

- **p.5021, line 1: Why potentially?**

Yes, potentially is not necessary here. We removed it from the text.

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