Replies to: Anonymous Referee #3. Interactive comment on "The ENSO signal in atmospheric composition fields: emission driven vs. dynamically induced changes" by A. Inness et al.

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We thank Referee 3 for their useful comments about our paper. We have tried to address all the suggestions and revised the manuscript accordingly. Our replies to their comments are given below in blue and changes to the manuscript in bold and blue.

General comments:

The manuscript presents results on the changes in atmospheric composition in the MACC system resulting from the ENSO. Differences in ozone, CO and NO2 concentrations between composites of El-Nino and La-Nina years are used to evaluate the role of changes in emission and dynamics on the atmospheric composition in the tropics. The first part of the paper presents differences in chemical composition in the MACC system dataset over a 10 year time period. The specific role of changes in emission or changes in dynamics is addressed in a second part with the C-IFS model which is run during a El-Nino year and a weak La-Nina year with different emission scenario. The authors conclude that changes in ozone over Indonesia are associated with changes in photochemical production due to an increase in biomass burning emission during El- Nino periods. Large scale ozone anomalies are found over the Pacific due to changes in vertical transport. Anomalies in CO, NO2 and AOD are mostly found over the maritime continent and are related to changes in biomass burning emission. I recommend the paper for publication after addressing the following comments.

Specific comments:

1) Last paragraph, page 13721: the authors claim that the MACC system can successfully model the ENSO signal. Because there is no validation of the ENSO signal against measurements, I cannot agree with this conclusion. Even though the MACC system was compared to satellite products in Inness et al. (2013), we need to see such validation for The ENSO signal, as it is estimated by subtracting El-Nino and La-Nina time periods. Inness et al. (2013) discussed only monthly averaged biases between MACC and satellite products. Bias and/or uncertainties specific to the ENSO signal in the MACC system could exist. It is particularly important if subsequent studies will deal with ocean-atmosphere interactions and ocean-atmosphere response to ENSO. If the atmospheric response in terms of terrestrial emission and dynamics is not well represented, how one can expect to have meaningful conclusions on ocean-atmosphere response and impact on atmospheric composition?

We have changed that sentence to:
The results from this paper show that the MACC system is able to model changes in atmospheric composition fields found under El Niño and La Niña conditions. After a more thorough validation of the MACC atmospheric fields against observations, it could be interesting to investigate the ocean-atmosphere response to ENSO induced changes in atmospheric composition in a further study.

The way atmospheric dynamics is treated in section 2 is not convincing. The affirmations on the impact of dynamics on atmospheric composition in section 2 is only discussed in general terms since not enough meteorological fields are presented. Section 3 is much more convincing because it uses vertical velocity and specific humidity. Vertical velocity and specific humidity should be used in the first part of the analysis as well.
We have produced composites of vertical velocity and specific humidity at 500 hPa from the MACC reanalysis and added the El Nino minus La Nina difference plots as Figure 2 to the paper (the numbers of the subsequent figures have been changed in the revised manuscript, but we use the original numbers in our replies to the reviewers further below). We have added in the text:
Figure 2 shows that the increased precipitation over the Central Pacific and the reduced precipitation over the Maritime continent are collocated with increased ascent and increased descent at 500 hPa, respectively. At the same time, specific humidity at 500 hPa shows a positive anomaly in the area of increased ascent and precipitation over the Central Pacific and a negative anomaly over the Maritime continent.

2) Changes in cloud cover during La-Nina and El-Nino years can also affect ozone photochemical production. Maps of J(O1D) photolysis rate would provide additional insight into section 2 and 3.
*Unfortunately, this is not available from the MACC reanalysis and would require a re-run of the experiments in Section 3. In general, the impact of increased cloud cover results in a reduction of JO1D below and increase of JO1D above the clouds, which is often compensating the OH production. Anomalies of cloud cover at 500 hPa show a similar signal to humidity (our new Figure 2) with decreased cloud cover over Indonesia and increased cloud cover over the central Pacific. A detailed analysis of the chemical budgets for this situation would make an interesting future study, but is beyond the scope of the current paper. We have added a sentence about the cloud cover in Section 2.2**: Cloud cover shows a similar signal to humidity, with a negative anomaly over the Maritime continent and a positive anomaly over the central Pacific (not shown).***

3) Why formaldehyde is not treated in the paper? Atmospheric composition should not be limited to ozone, CO and NO2.

The reviewer is correct that formaldehyde (HCHO) is an interesting species as it points at varying isoprene sources in the region, which may in turn affect O3 production, depending on availability of NOx. Unfortunately, in current simulations HCHO is not constrained in the MACC system by observations and biogenic emissions are applied without inter-annual variability. Therefore we do not believe that HCHO is a suitable tracer to analyse for this paper.

4) How biomass burning is injected vertically in the model? Since the injection height will be affected by fire intensity and atmospheric stability, one can expect a change in injection height during El-Nino vs La-Nina. If a fixed injection height is used, it could bias the CO and AOD fields at 500hPa.

We have added in section 2.1 :

The emissions are injected at the surface and distributed over the boundary layer by the model's convection and vertical diffusion scheme. Despite the distribution being very efficient, this is a limitation of the current system that and will be addressed in future versions. Experiments have been carried out with a new version that uses injection heights based on the Plume Rise Model of Paugam et al. (2015). They show a significant impact on BC AOD for single large fires; the impact at a global scale is smaller: BC AOD is increased by around 5%. Most of the injections heights calculated with the Plume Rise Model lie within the boundary layer and only a small fraction of smoke (often from particularly intense, and well-studied fires) is injected directly into the free troposphere. The largest smoke transport from the boundary layer to the free troposphere occurs through larger-scale meteorological processes. The lowering of the boundary layer height, when air is advected from land to sea, and strong updrafts in frontal system have previously been identified as efficient smoke transport mechanisms. Similarly, Veira et al. (2015) has studied the sensitivity of AOD in a global climate model to different injection height parameterisations and the above-mentioned plume rise model, with the conclusion that a simple parameterisation reproduces the average larger-scale distribution sufficiently well.

Extra reference: Paugam, R., Wooster, M., Atherton, J., Freitas, S. R., Schultz, M. G., and Kaiser, J. W.: Development and optimization of a wildfire plume rise model based on remote sensing data inputs – Part 2, Atmos. Chem. Phys. Discuss., 15, 9815-9895, doi:10.5194/acpd-15-9815-2015, 2015.

Veira, A., Kloster, S., Schutgens, N. A. J., and Kaiser, J. W.: Fire emission heights in the climate system – Part 2: Impact on transport, black carbon concentrations and radiation, Atmos. Chem. Phys., 15, 7173-7193, doi:10.5194/acp-15-7173-2015, 2015.

5) How ocean emission of halogenated species, VOCs and deposition on ocean surface is treated?

We have added in Section 2.1: ***The MACC models do not contain halogenated species, which would contribute to a small additional loss term to O3 and its precursors in the tropical marine boundary layer. Ocean emissions of volatile organic compounds (VOCs) originate from climatological data from POET. Deposition on ocean surface depends on the species solubility, which is negligible for O3 and CO, but not for some of the VOCs. All these aspects may contribute to overall biases in the model, but are not considered essential for the signals investigated here.***

6) section 2: Why the AOD anomaly reach the lower troposphere at 200E, but no such anomaly is found in CO, NOx and ozone?

We have added at the end of Section 2:

In the lower troposphere there is a negative aerosol anomaly over the Central Pacific that is not seen in the other atmospheric composition fields. This anomaly is likely to be the result of the increased precipitation in this area during El Niño conditions (see Figure 1) which leads to increased wet deposition and removal of aerosols, while not removing the gas-phase species in the same way.

Technical comments:

line 18, p13711: la nina ...

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line 11, p13721: comparing simulations ...

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