Replies to: Dr. Nassar (Referee). Interactive comment on “The ENSO signal in atmospheric composition fields: emission driven vs. dynamically induced changes” by A. Inness et al.

Received and published: 14 June 2015

We thank Dr. Nassar for his 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.

Inness et al. use the Monitoring Atmospheric Composition and Climate (MACC) reanalysis to investigate the effects of El Nino on atmospheric composition, specifically CO, ozone, NOx and aerosol in the region of the Maritime continent. The manuscript was very well-written and clearly presented, with minimal errors and high quality figures. The work described in this manuscript builds off of many previous studies on the impact of El Nino on atmospheric composition. While most previous studies focused on a single El Nino event relative to a neutral or La Nina year, Inness et al. investigate October, November and December composites from three El Ninos (2004, 2006, 2009) com- pared with composites from those months during La Nina (2005, 2007, 2008, 2010) from their 10-year MACC reanalysis. This reanalysis is at a far higher spatial resolution (80 km) than any known past global modeling studies on this topic, so in this sense the study is an advance relative to earlier work, however, the scientific investigation does not go as far as in some earlier work, which was a bit of a disappointment. For example, the authors separate the El Nino impacts on atmospheric composition into emissions and dynamics, and conclude that the ozone enhancement is mostly dynamical, but according to their method, their dynamical component must include the contribution from lightning NOx emissions, which is only briefly mentioned without an attempt to quantify the lightning impact on ozone.

Dr. Nassar is correct that our dynamical component includes the contribution from lightning NOx on ozone, as this contribution is not isolated in our study. Alike, also the wet scavenging is not separated from the dynamical component or the impact of cloudiness on photolysis rates. It is fair to say that we basically mean meteorological (or atmospheric) factors when we say „dynamics“. In this study we wanted to isolate the impact of the biomass burning emissions. We argue that lightning NOx production is considered as an inseparable aspect of the dynamical component in this study, as the flash rate density that is used to calculate NO emissions from lightning is based on parameters of the convection scheme and is calculated using convective precipitation as input parameter. This parameter is affected by changes in the dynamics and not the fire emissions. In a future study it could be of interest to assess the chemical budgets in more detail but that is beyond the scope of the current paper.

In general, a more quantitative evaluation of the MACC reanalysis would have been desirable. For example, the authors state in their conclusion (p 13721) that the results of the paper show that “the MACC system is able to successfully model the ENSO signal in atmospheric composition fields, and could therefore be used in further studies to investigate the ocean-atmosphere response to ENSO induced changes in atmospheric composition.” However, they do not demonstrate that the ozone, NOx and CO enhancements in the reanalysis during an El Nino do indeed match observations. Inness et al. 2013 is cited, but this is just a general comparison paper and does not demonstrate the agreement specifically in this region during El Nino. Perhaps this is because observations have been assimilated, so the fields are assumed to match observations, which may generally be the case, but reader has no knowledge of the degree of agreement with observations without it being demonstrated here. This contrasts with for example, Nassar et al. (2009) in which GEOS-Chem CO, ozone and water vapour composition fields generally agree with satellite observations, however, attempts were made to explain remaining differences between the model and observations by investigating issues like: the magnitude and timing of CO emission, possibly related to the model and biomass burning inventory’s neglect of peat smouldering; the impact of enhanced lightning NOx and soil NOx on the ozone enhancement; or the impact of convective transport on CO, ozone and water vapour. Since Inness et al. does not quantitatively confirm the magnitude and timing of the anomalies in the reanalysis with independent observations, one can only make conclusions regarding the relative contributions of emissions and dynamics in the MACC system, but cannot reliably extend such conclusions to the real earth system.

In summary, while this paper in its current form (with minor corrections) can be considered a reasonable and a useful introductory analysis of MACC during El Nino, a quantitative verification of the MACC El Nino composition fields in this region using observations, AND hypotheses to explain any differences, would make this a stronger paper, perhaps enhancing our scientific understanding of the topic.

A detailed quantitative verification of the MACC El Nino composition fields is beyond the scope of this paper, but we agree with Dr. Nassar that it would be worth while to do this in a follow up study. A basic validation of the fields was done in Inness et al (2013), Inness et al. (2015) and Flemming et al. (2015) and more detailed validation is constantly carried out by the MACC validation team whose validation reports are available from http://www.copernicus-atmosphere.eu/. We already mention the reanalysis validation reports in Section 2.1. We have added a reference to Inness et al. (2015) and have also added in Section 3.1: ***A basic initial validation of CIFS-fields can be found in Flemming et al. (2015) and Inness et al. (2015) and more detailed validation of C-IFS can be found in the validation reports available from http://www.copernicus-atmosphere.eu/.***

Specific points

p 13706, line 12: “nitrogen oxide” should be “nitrogen oxides”

Corrected.

p 13714, line 14: “EL” should be “El”

Corrected.

p 13714, line 23: “upper the troposphere” should be “upper troposphere”

Corrected.

p 13715, line 4: the longitude for the anomaly in Figure 9 that they are referring to would be helpful to provide. They mention an anomaly over Africa, which I’d expect at 30 E, whereas a positive anomaly appears over 300E or South America.

Sorry, this was wrong in the text. We have changed it to: ***Now a small positive anomaly is found over South America.***

p 13716, line 15: “lighning” should be “lightning”

Corrected.

p 13718, line 15: “surrunding” should be “surrounding”

Corrected.

p 13721, line 11: “Comapring” should be “Comparing”

Corrected.

p 13721, line 17: “affected” should be “affected”

Corrected.

Figure 10. A more detailed interpretation of the NOx anomalies is desirable.

We have added in the discussion of Figure 10:

The positive NOx anomalies around 100^{o}E in October and November are collocated with high O_3 values in the lower troposphere (seen in Figure 8) pointing to enhanced O_3 production due to enhanced NOx concentrations from biomass burning. In December, when NOx does not show such a positive anomaly any more, O_3 concentrations in the lower troposphere are lower and the maximum of the O_3 anomaly is located above 700hPa.

Figure 15. The authors fail to comment on the fact that in October, the peak in specific humidity is south of ozone enhancement. Nassar et al. (2009) showed that the equa- torial component of the October ozone anomaly was related to fire emissions, with the southern component of the ozone anomaly due to other factors.

We have added in the paper:

In October the peak in specific humidity is located south of the ozone enhancement. This agrees with Nassar et al. (2009) who showed that the equatorial component of the October ozone anomaly was related to fire emissions, while the southern component of the ozone anomaly was due to other factors.

Furthermore, that fact that the elevated humidity over southern Africa corresponds to decreased ozone, but a similar feature over in the region of Saudi Arabia and Iran does not, warrants some comment.

We have added in the paper:

It should be noted that the positive specific humidity anomalies over the Arabian peninsula and over Australia in October do not correspond to decreased ozone values, while the ones over southern Africa, South America and the Central Pacific do. The reason for this is that relative anomalies are shown and that the absolute humidity values over the Arabian peninsula and Australia are much lower than in the other areas, so that the absolute humidity changes between 2006 and 2005 are actually relatively small. This all suggests that the correlation of O_3 to specific humidity is strongest in tropical regions with large variability in water vapour, combined with low NOx conditions.

Figure 17. It would have been useful to show a larger longitude range for the map here (especially westward) since in panel b, for example, major features are cut off at the map boundaries.

We have increased the area to the west so that it now extends from 40E to 130E.