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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The ENSO signal in atmospheric composition fields: emission driven vs. dynamically induced changes" *by* A. Inness et al.

Anonymous Referee #3

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"The ENSO signal in atmospheric composition fields: emission driven vs. dynamically induced changes" By Inness et al.

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

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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?

2) 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.

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3) 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.

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

5) 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.

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

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

Technical comments:

line 18, p13711: la nina ...

line 11, p13721: comparing simulations ...

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 13705, 2015.