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Interactive Comment

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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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-

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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.

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

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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.

Specific points

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

p 13714, line 14: “EL” should be “El”

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

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 300°E or South America.

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

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

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

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

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

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-

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torial component of the October ozone anomaly was related to fire emissions, with the southern component of the ozone anomaly 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.

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.

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