

Interactive comment on “Transboundary ozone pollution across East Asia: daily evolution and photochemical production analysed by IASI+GOME2 multispectral satellite observations and models” by Juan Cuesta et al.

Anonymous Referee #1

Received and published: 23 November 2017

General Comment

The authors examined a temporal evolution of ozone in a transboundary pollution event occurred in early May 2009 over East Asian countries by using multiple satellite observations and chemical transport models. The use of multispectral satellite data of IASI and GOME2 provides LMT ozone concentration which cannot be obtained by single-band retrievals. They clearly showed how well the IASI+GOME2 approach retrieve the ozone concentration in MLT and applied it to describe an outbreak of transboundary ozone pollution event in East Asia. The large-scale observation of ozone near the sur-

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face (LMT) from the satellite with this approach is apparently a powerful tool for the air quality researches over the globe. This paper is well within the scope of the journal, however, I noticed several issues in this paper which cannot be passed over to be published. I suggested that the authors should consider the following comments: one major and several specific comments.

Major Comment:

My biggest concern is the arbitrariness in the use of model results. The authors used the simulation with two different chemistry transport models to explain the daily evolution of ozone pollution across East Asia. I suppose that both of the two models can simulate the same chemical quantities such as the three-dimensional concentration of O₃, CO, NO₂. However, the authors did not fully utilize the results from both models, but they only used the result from one of the models for one quantity in most cases. I don't think it is fair to arbitrarily pick up only the better and propitious result from one of the models for their interpretation. I strongly suggest the authors to evenly use the results from both models for each chemical quantity. To put my point differently, the author should clearly state the different roles of the different models that they supposed at the very first part of the paper, so that the reader may not feel arbitrary use of the models.

Specific Comments:

- Abstract: The definition of lowermost tropospheric ozone should be provided.
- P3 L18-19: A brief explanation why single-band retrieval cannot provide the information in PBL is better here, not mandatory though.
- P6 L18-19: I could not find IASI+GOME2 data provided in both URL.
- P6 L28: Should more clearly describe the criteria of special coincidence. Is one degree lat/lon criteria between the location of sonde station and the center point of the satellite visual field?

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- Table 1: This table is not referred in the manuscript. If it is not necessary for the paper, it's better to remove it.
- Figure 1: Is the symbol " 1σ " in legend widely used? I think at least a brief explanation is necessary in the caption.
- P7 L19: Why the sonde stations in Asia outside Japan, such as Hong Kong or Hanoi, were ignored? They have data for the year 2009 and 2010.
- P7 L28: EANET and GAW are different NW, so you should refer to them separately here. Also you should provide the source URL for GAW database.
- P8 L10: Is "vertical difference" more appropriate to this quantity than "vertical gradient" here. If you prefer to use gradient, it is natural to me that the unit is ppb/km.
- P8 L18-19: The author can discuss about the following paper here, since the paper also tried to retrieve the lower tropospheric ozone from OMI data.
Hayashida S., Liu X., Ono A., Yang K., Chance K., 2015. Atmospheric Chemistry and Physics 15, 9865-9881.
- Figure 3: Fig3a and b shows the LMT O3 in unit of ppb. Is this an average concentration of O3 in the lowermost 3 km altitude? If so, you should clearly state it somewhere in the manuscript (and the caption of this figure).
- P12 L6-10: The version of each EI should be described.
- P12 L12: Is this an appropriate reference for CHASER model?
- P12 L23-25: Is this reduction of bias for column density? How about the reduction of bias for the surface concentration by assimilation?
- P13 L2-4: Did you do assimilation for the sensitivity analysis too? If so, can we assume the influence of assimilation process is the same for both full and sensitivity simulation? If the impact of assimilation is different in two simulation, the difference of

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ozone concentration between full and sensitivity simulation cannot be regarded as a pure stratospheric contribution.

- P13 L8: Is this reanalysis (ERA-INT) used to drive CHASER?
- P14 L16: You should describe clearly how to set the magenta rectangle in the figures. Are there any objective criteria to draw the four sides of the rectangle? There is no description of the rectangle in the caption of Figure 4 and 6.
- P16 L9-10: Where is "this location" here?
- P16 L16-17: I cannot agree with here, for me, Fig 6b is not so good agreement with Fig 5a.
- P16 L23-25: Which figure does this sentence mention to?
- P17 L26: Should clarify how to initiate the Hysplit calculation for two pollution plumes.
- P17 L27: Typo? (Fig. 9a)
- P18 L17: I cannot see the enhanced NO₂ at the area pointed out here. Is this sentence correct?
- Figure 10e: The altitude of PV contour (300 hPa) should be described in the caption.
- P19 L21 & L28: Typo? 11e and 12e ?
- P20 L1: 11f and 12f?
- P21 L5: Fig 13 should be Fig 13a.
- P22 L4: NO titration
- P22 L6: Only horizontal dilution is important?
- P22 L12: I don't think the absolute values of the ratio dO₃/dCO are consistent with each other from satellites observations and models. The models show apparent lower ratio than the observation. Please clarify what aspect is consistent each other.

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- P22 L14: Typo? “those from models”?
- P22 L21: It is not easy to understand the meaning and implication of “degrees of freedom” for the readers outside the satellite data analysis. It’s better to briefly explain them here.
- P22 L 25-26: Which figure does this sentence mention to?
- P22 L29: What is “3-9” here?
- P23 L1-2: I don’t think the stratospheric contribution remains constant in anyway. It fluctuates a lot during the period.
- P23 L7-10: It is not easy to see consistency in the ozone partial column between satellite observation and model (CHASER) simulation. It’s better to use more words to describe which aspect do you think is consistent to each other.
- P23 L16-17: How does the concentration change in NO₂ on 7 May simulated by WRF-Chem? Is it similar to what simulated in CHASER?
- P23 L23: Typo. red curve -> blue curve
- P24 L2-5: The authors referred to relatively high NO₂ concentration in CHASER as a cause of greater growth of dO₃/dCO ration in the northern plume than the southern plume from 7 to 9 May. However, the ration in CHASER did not show such a growth both in the northern and southern plumes (Figure 13c). So it is not appropriate to refer to the change in NO₂ in CHASER as a cause of the observed change in the ratio.
- P24 L17-19: Can you estimate the impact of this effect on the dO₃/dCO ratio quantitatively? Excluding under- or over-estimation of dO₃/dCO ratio due to the change in the sensitivity of satellite retrievals is quite important to make this approach useable for O₃ production estimation during air mass transport

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-972>,

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