Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-938-RC2, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interannual variation, decadal trend, and future change in ozone outflow from East Asia" *by* Jia Zhu et al.

Anonymous Referee #2

Received and published: 24 December 2016

A review of "Interannual variation, decadal trend, and future change in ozone outflow from East Asia" by Jia Zhu et al. submitted to ACP

General comments: The paper describes a model analysis of the past (1986-2006) and future (2000 vs. 2050) changes in the continental outflow of tropospheric ozone from East Asia. For the past and future changes, the authors ran the GEOS-Chem model driven by meteorological fields from GEOS4 and GISS GCM3 (under SRES A1B scenario), respectively. Basically the topics of the paper are of substantial interest. However, I found that the paper is rather descriptive and the discussion is not thorough. In many parts of the paper the authors show statistical results and interpretation rather than in-depth analyses that they could do with such a suite of model simulations. My another concern is that the authors' approach using the SREA A1B scenario now sounds old model sets, and I wonder why the authors did not try the

Printer-friendly version

Discussion paper

simulations with RCP scenarios. Well, reserving this criticism, the paper still needs to be more focused on a new science with respect to continental outflow of ozone from East Asia that the authors can deliver from the current model runs.

Major comments:

(1) Why SRES A1B scenario? - I think this scenario is now out of date and would not be realistic for the future, suggesting the model studies less useful than before, say several years ago. If the authors stick to the SRES scenario, they would need to justify why they used this scenario not the RCP one. Also, I found that the discussions read a bit superficial, with a lot of interpretations by referring to previously published papers based on similar model settings with the SRES A1B scenario (i.e., Wu et al., Pye et al., Jiang et al.). The authors should focus on a new science with respect to continental outflow of ozone from East Asia, provide in-depth analysis in terms of meteorological and climatic mechanisms or key factors. In Abstract, the authors mentioned "Sensitivity simulations indicated that the large IAVs of O3 outflow fluxes were mainly caused by the variations in meteorological conditions.", but this statement reads rather general. What meteorological factors or mechanisms are key for IAV? The authors showed statistical analysis but the mechanisms behind the large IAV is much more informative to the community.

(2) More robust model validation (Section 3) - Because of the large uncertainty in the retrieval of tropospheric ozone, comparison to satellite is not a robust way to quantitatively evaluate the model performance for the lower tropospheric ozone, in particular. The authors can make satellite comparisons with the reasons they mentioned in the Reply to the other reviewer, but why don't the authors evaluate the model by comparing to surface and sondes observations available in East Asia? I strongly believe that the model validation should be intensively made on seasonal basis since the authors are discussing the past and future ozone flux based on the model runs. The data from EANET are often used in evaluating the regional and global models by many groups in Asia (e.g., MICS-Asia) and in the international projects (e.g., HTAP) (e.g., Nagashima

Printer-friendly version

Discussion paper

et al., ACP, 2010; Li et al., 2008). In Figures 3 and 4, the model overestimated the satellite-derived TCO over central-eastern China through the western North Pacific, and the phase of the seasonal cycle in TCO is not as great as the current state-of-science models could be. I do not see the model doing a good job in reproducing the distributions and seasonal cycles, so cannot be positive to support the further analysis. The model overestimates TCO in spring, so this would give the overestimates in the calculated eastward flux. On the other hand, maybe the satellite-derived TCO is not too low (Figure 3), or the maximum shifts later than should be (Figure 4). I would encourage the authors to examine the model-observation comparison for the boundary layer, and middle and upper troposphere. Recent paper by Tanimoto, Zbinden, et al. (2015) showed robust observations for the seasonal cycles and interannual variations over Japan, and would be useful for this comparison.

(3) Is 135 degE appropriate? - The authors mentioned in the title "ozone outflow from East Asia" and used a longitudinal transect at 135 degE to diagnose the eastward flux of ozone. I wonder why at 135 degE, not 120 degE, to be more close to ozone production region in central-eastern China. I think, if the authors look at the flux at 120 degE, they would obtain higher signals in the ozone flux, and this would be much more direct in interpreting the model simulations. Also, the authors mainly discuss central-eastern China or North China Plain, rather than whole East Asia. This should be explicitly phrased, for example, "outflow from central-eastern China", since this paper is not looking at the impacts on the western North America but focusing on export region.

Specific comments:

Abstract, L22: insignificant decadal trend of -2.2%/decade. Add +/- uncertainty, or just delete the number here.

L28-29: spring and summer. The maritime flow from the Pacific Ocean is predominant in summer. Is summer really effective in the enhancement of continental outflow? I do

not see strong enhancement in summer in Figure 9.

L31: important implications for long-term air quality planning. For whom? For US? For northern midlatitudes? For China? For East Asia?

P2, L8-11: ... influences ozone air quality in the downwind regions, such as the US and Canada. Downwind regions are not only the western US, but should include the neighboring regions and the Pacific Ocean. Ou-Yang et al. paper is already cited here, so the sentence should be rephrased to be something like "... such as the western North Pacific through the western North America", and add some other references, for example, papers reporting long-range transport to Korea, Japan, and the Northern Pacific (Han et al., ACP, 2015; Tanimoto et al., GRL, 2005; Pochanart et al., 2015, and many others!).

P3, L8-12: Tanimoto, AE, 2009 should be cited here (decadal trends of ...)

Figure 4: The authors showed comparison of TCO for GEOS-Chem and TOMS/SBUV, suggesting the biases in the model. Again, why don't the authors make comparison to the surface and sonde observations?

Also, in P8, L10-13, the authors state that "although GEOS-Chem overestimates TCO values over eastern China and the western Pacific Ocean, the model exhibits reasonable performance in simulating the spatiotemporal distributions of the tropospheric ozone column burden over China and downwind regions, which lends us confidence to simulate the temporal evolutions of the Asian ozone outflow." I would not agree with this statement, since model and satellite are quite different in the tail of outflow from China (the region of >40 DU, shown in orange), and this difference would lead to large biases in calculating outflow flux in particular, as the authors set the diagnosis line at 135 degE, off China, and over Japan. Also, technically, the authors said "the western Pacific Ocean" here and also in the Figure 4 caption, but the region where the authors pointed is mostly Japan, so the description must be accurately modified.

Printer-friendly version

Discussion paper

P9, Section 4.2 IAV and decadal trends: The authors basically said that the influence of Met. is larger than Emiss., which makes sense if they diagnosed at 135 degE, off the Asian continent, where Asian monsoon impacts are substantial.

A number of important references are missing: Pochanart, P. et al, 2015, Boundary Layer Ozone Transport from Eastern China to Southern Japan: Pollution Episodes Observed during Monsoon Onset in 2004, Asian J. Atmos. Environ. 9, 48-56.

Tanimoto, H., et al., 2005, Significant latitudinal gradient in the surface ozone spring maximum over East Asia, Geophys. Res. Lett. 32, L21805.

Tanimoto, H., 2009, Increase in springtime tropospheric ozone at a mountainous site in Japan for the period 1998–2006, Atmos. Environ., 43, 1358-1363.

Nagashima, T., et al., 2010, The relative importance of various source regions on East Asian surface ozone, Atmos. Chem. Phys., 10, 11305-11322, doi:10.5194/acp-10-11305-20109077-9120.

Li, J., et al., 2008, Regional-scale modeling of near-ground ozone in the Central East China, source attributions and an assessment of outflow to East Asia - The role of regional-scale transport during MTX2006, Atmos. Chem. Phys., 8, doi: 10.5194/acpd-8-13159-2008.

Tanimoto, H., et al., 2015, Consistency of tropospheric ozone observations made by different platforms and techniques in the global databases, Tellus B, 67, 27073, doi: 10.3402/tellusb.v67.27073.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-938, 2016.

Printer-friendly version

Discussion paper