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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluating transport in the WRF model along the California coast" *by* C. Yver et al.

Anonymous Referee #2

Received and published: 26 October 2012

This manuscript evaluated the transport along the California coast in WRF model by comparing model results with ACARS data at four airports (3 coastal stations and 1 inland station). The model performances with different PBL schemes, two sets of datasets providing IC/BC conditions, different horizontal resolutions, as well as the WRF transport option vs. WRF/Chem were assessed. The topic is of interest and also appropriate for ACP. The manuscript is well organized and the discussion is overall thorough and informative. However, additional analysis of the results may lead to improvement of the manuscript (please see my general comments below. My overall recommendation is accept with minor revision.

General comments:

1. Only monthly means were analyzed for meteorological variables in this manuscript,

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thus features with smaller time scales were totally smoothed out. The primary purpose of this manuscript is to evaluate the WRF performance in simulating the transport along the California coast, which is normally associated with weather patterns with time scales of less than a week (such as Santa Ana event discussed in the manuscript). Therefore, day-to-day comparisons of meteorological variables to observations can be more important than monthly means in evaluating the model performance for this study.

2. Validation of surface wind and temperature (Figs. 3 and 5) is an important component of this manuscript. There are a number of observational sites recording hourly surface meteorological variables all year round over California. The data can be more accurate than these used in the manuscript, which were based on ACARS data with pressure greater than 990 hPa. Moreover, the transport to the receptor is not only dependent on the surface wind/temperature over the receptor (SIO for this study) and sources (LA and San Francisco for this study), but also surface wind/temperature in other regions of the domain (such as during Santa Ana event). Therefore, comparisons of model results to observations over more surface stations with a figure similar to Fig. 6 in Hu et al. (2010) or Fig. 2 in Zhao et al (2011) could complement the analysis.

Hu, X.-M., Nielsen-Gammon, J. W., and Zhang, F.: Evaluation of three planetary boundary layer schemes in the WRF model, J. Appl. Meteorol. Climatol., 49, 1831–1844, doi:10.1175/2010JAMC2432.1, 2010. 16856.

Zhao, Z, S.-H. Chen, M. J. Kleeman, M. Tyree and D. Cayan, 2011: The impact of climate change on air quality related meteorological conditions in California . Part I: Present time simulation analysis, J. Climate., 24, 3344-3361

Specific comments:

P16852, line 10: The uses of “nesting options” here and some other places in the manuscript are a little bit confusing. Normally, the “nesting option” in WRF means one-way or two-way nesting. The manuscript mostly explored the impact of different

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horizontal resolutions instead of different “nesting options” on simulation results, thus the authors should be careful using this term, and it is better to replace this term with “different horizontal resolutions” here and couple other places in the manuscript.

P16853, line20: “others” should be replaced with “other”.

P16855, line15: WSM 3-class microphysics scheme is a relatively simple scheme. Is there any reason to choose this scheme rather than other more comprehensive ones, such as WSM-6 and Thompson schemes?

Section 2.1: For WRF simulations with relatively long integration period, a month in this study, FDDA (also known as analysis nudging) and update of SST (SST_update option in the WRF namelist.input) can be important to prevent the drift of model results. The SST update can be even more important in this study considering that the main focus over the coastal region. Are they employed in this study? Please specify.

P16856, line 25: Finer resolution will not only improve the WRF simulation of winds, but also other variables, such as temperature and PBL height. Please revise this sentence.

P16857, line 13: The use of “timestep” here is inappropriate. “timestep” in WRF literature normally means the “time step for integration”. It is better to replace this term with “time interval” or other proper terms here.

P16858, line 14-15: The original sentence of “we have added a chemistry module” somehow implied that the authors added a new chemistry module into WRF model instead of the use of WRF-Chem. Thus, revision is needs to avoid misperception.

Section 3.3: It is only described that “scaled it with the 2005–2009 trend reported for California by CARB”. It is not clear if a single scale factor was used for the whole domain, or different scale factors were used for different regions. More information regarding how this scaling was done need to be provided.

P16862, line 24: It is difficult to understand “an improved mean diurnal cycle”, please be more specific.

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P16862, line 23 to 28: It is not common to compare monthly mean model results with three-month averaged observations. The reason given here is not clear. Please provide the justification with more details.

P16868, line 4: Figure 8 suggested that P4E12c results were used. Please correct "(P4E12)" to "(P4E12c)".

P16868, line10: Is the "18 m s-1" observations or model results. Please provide both.

P16868, line21 to 23: This sentence is problematic, please revise.

P16869, line8 to 10: The claim that the wind brought air masses from Los Angles and the other coastal regions to SIO is not convincing based only on the wind direction plots in Fig. 9. One possible way to make this statement more robust is to add two more panels in Fig. 8 to show the recirculation on 21 December.

Fig. 5: Typo in the figure caption. Likely, it should be "For the wind direction" rather than "For the wind speed", please make the correction.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 16851, 2012.