Geosci. Model Dev. Discuss., 8, C798–C800, 2015
www.geosci-model-dev-discuss.net/8/C798/2015/


Geoscientific Model Development Discussions

# *Interactive comment on* "Evaluation of the high resolution WRF-Chem air quality forecast and its comparison with statistical ozone predictions" *by* R. Žabkar et al.

**G. A. Grell (Referee)**

georg.a.grell@noaa.gov

Received and published: 19 May 2015

This paper describes the use of the community version of WRF-Chem for real-time ozone and aerosol predictions. The authors perform statistical evaluations over a 3 month period, comparing the model forecasts with observations as well as statistical forecast methods. In general his paper is well written and should be published in GMD. This can be done with only minor modifications. Although the authors provide much information on model set-up there are a few details that I was looking for and couldn't find. Is this 2-way nesting or 1-way nesting? If it is 1-way nesting, how was it applied? Is the choice of physics parameterization the same on both domains? Which photolysis



model have you been using? All evaluations I am assuming are done on the high-resolution domain. Also, the color choice for figures 5,6, and 7 is unfortunate. The two blue colors are almost impossible to separate – at least with my aging eyes. Why not a different color? Figure 5 is even more difficult to read, a bit too small for me. Some other questions I have:

(1) There is a negative temperature bias, but a positive short wave bias? Since you are using the interaction flag for convection/radiation the SW bias could be interpreted as not enough cloud cover, which could give you a low bias at night, but at day? Are you cycling soil temperature and soil moisture or is that always a new initialization with coarse resolution GFS data?

(2) The statistics I assume are always over domain 2. The fact that the precipitation under-forecast is a lot less on day 2 may indicate some spin-up issues, especially also when taking into consideration the coarse initial conditions (did you use .5 degree data from GFS?)

(3) On page 1047, line 22 you talk about WRF-Chem under-predicting Ozone maxima, wile before you had a positive bias. Do you mean under-predict excedences?

(4) In the summary and conclusions you should mention again (you have that hidden somewhere in section 2.1, pg 1034) that different choice of physical or chemical parameterization will influence and possibly change outcomes. However I think your choices are good choices, since they are well documented in other real-time applications.

(5) Pg. 1031, line 7: The MM5 reference should be 1994, not 1995 – if I remember correctly

(6) Pg 1032, line 11: 2011 should not be a reference for WRF-Chem. Just 2005 is good enough.

(7) Pg. 1049, last line: If you want you could add the recent Pagowski et al publication in GMD (also WRF-Chem special issue) as an example of chemical data assimilation

available for WRF-Chem users.

Interactive comment on Geosci. Model Dev. Discuss., 8, 1029, 2015.