Dear Dr. Georg A. Grell,

We appreciate and would like to thank you for all the comments and raised questions, which have helped to improve the quality of the paper. Below we provide our point-by-point replies, where for clarity the comments are displayed in bold italics.

> ***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.***

We thank for this comment.

> ***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?***

It is a 1-way nesting applied by two consecutive simulations (using ndown). We added this information the following way (section 2.1): »A 1-way nesting is applied by two separate consecutive simulations, where outputs from the coarse grid integration are processed to provide boundary conditions for the nested run every 15 minutes.«

> ***Is the choice of physics parameterization the same on both domains?***

Yes, schemes are the same on both domains. To include this information in the paper we changed in Section 2.1: »We decided to apply the same schemes as were used…« to »In both domains we decided to apply the same schemes as were used…«.

> ***Which photolysis model have you been using?***

Fast-J photolysis scheme (Fast et al., 2006), this information is now added in section 2.1.

> ***All evaluations I am assuming are done on the high resolution domain.***

Yes. We included this information in the first sentence of Section 2.3: »We evaluate the 1-day and 2-day WRF-Chem meteorological and AQ forecasts on the high resolution domain during a 3-month period (June - August 2013).«

> ***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.***

We replotted these figures with two different colors. Still it is hard to distinguish between 1-day and 2-day forecast (Fig. 5-7), but the purpose of these figures is more to separate model forecast from observations. 1-day and 2-day forecast are more easily distinguished by the use of statistics. Figure 5 is now divided into two parts.

> ***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?

All meteorological variables, including soil temperature and soil moisture are always initialized with GFS data, which is now mentioned in the paper. This explains higher negative bias for T2m during the first day of simulation (not valid for daily maxima, where bias is the same on the first and the second day of simulation). For all hourly values T2m bias decreases from -2.1 C to 0.8 C due to reduced bias for nighttime temperatures on the second day of simulation. Looking at results station by station the link between T2m and SW bias is not straightforward (they appear not to be directly correlated). On the first day of simulation higher SW is due to less cloudy conditions (more cloud cover on the second day).

(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?)

Yes, we used the 0.5 degree data from GFS, this information is now added in section 2.1 as »…with meteorological initial (ICs) and lateral boundary conditions (BCs) taken from the 0.5° data from the Global Forecast System (GFS)…«. We also agree that under-prediction of precipitation indicates some spin-up problem, where it must also be taken into account that in 3.4.1 model version ndown procedure does not allow to include the information about hydrometeors at boundaries of the nested domain. Since the intensity of (rare) summertime precipitation events was expected to have a less significant impact on ozone concentrations, we considered this issue less problematic in our study focused on ozone. But we agree that applying a different initialization methodology should reduce the precipitation error. The following text was added: »It must be mentioned that the 3.4.1 model version does not allow to include the information about hydrometeors at the boundaries of the nested domain (in the applied 1-way nesting procedure), which contributes to the negative simulated bias of precipitation. A large decrease in the precipitation bias from day 1 to day 2 suggests that different initialization methodology (e.g. using 1 day spin-up for meteorology) could improve the prediction of precipitation events.«

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

We replaced »ozone maxima« to »threshold exceedances«.

(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.

We added the following sentence to the conclusions: »Since the selection of physical or chemical parameterization schemes influences and possibly changes the outcomes, we decided to apply the

schemes that are well documented and have previously been used in other applications (e.g. AQMEII).«

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

This error is now corrected.

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

We deleted the 2011 reference.

(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

The following sentence was added: »For WRF-Chem model a technical note on the implementation of the aerosol assimilation and a guidance for prospective users has been recently published by Pagowski et al. (2014).«