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Interactive comment on "Assessment of bias-adjusted $PM_{2.5}$ air quality forecasts over the continental United States during 2007" *by* D. Kang et al.

Anonymous Referee #1

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General Comments: This is a research paper that presents an application and evaluation of Kalman-filter based bias adjustment to the CMAQ-predicted PM2.5 forecasts. The paper is concise and well-written. Overall, the paper demonstrates a very useful technique to adjust CMAQ-based PM2.5 air quality forecasts in near real-time. This is very important given the challenges currently faced in modeling PM2.5 concentrations in real-time. The paper provides a detailed evaluation of the adjusted forecasts, demonstrating an improvement in the model predictions at nearly all regions and seasons. While this approach has been used for adjusting ozone forecasts, I believe this is one of the first attempts to present an evaluation of this approach for PM2.5 predictions for a year-long period over the continental United States (US). I had a few minor

comments as detailed below. Consequently, I recommend that the paper be accepted pending minor revisions. Specific comments are as follows.

Specific Comments:

Page 1378, line 11: "...predictor (hereafter referred to as KF bias-adjustment or simple KF)..."

Page 1379, line 21: "....12-km horizontal grid spacing on ..."

Page 1380, line 19: "...the beginning of one hour to the next."

Page 1381, line 14: How sensitive is the adjustment to the training period of "2 days"?

Page 1381, line 10: The authors use a fixed error ratio of 0.06 based on their previous work in adjusting ozone forecasts. I agree that the authors presented a comprehensive sensitivity analysis of the error ratio in their 2008 paper (Kang et al., 2008, J. Geophys. Res., 113, D23308) while applying the Kalman filter for adjusting ozone predictions. However, I am uncertain if that value can be directly applied for PM2.5 for the following reasons: 1) PM2.5 is a composite parameter composed of different chemical species that have different spatial and temporal variability; 2) Elevated PM2.5 concentrations occur throughout the year, and exceedances may occur during summer and winter seasons; 3) The PM2.5 components responsible for the elevated concentration are different during summer and winter, and varies for different regions. To my knowledge, all studies so far have examined this approach only for ozone, and for the summer season only. Given that this is one of first manuscripts that deal with an evaluation of this approach for PM2.5, I would be interested in seeing at least a preliminary analysis of the sensitivity of the adjustment procedure to the choice of the error ratio value. I realize that a detailed sensitivity analysis of this parameter for PM2.5 may warrant a separate manuscript due to the different variables involved (species, season, region etc). Thus, it may be appropriate to present a sensitivity of the adjustment to the error ratio for a few different values set uniformly for the entire US. I am not requiring

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that the current analysis be redone; but rather just adding a section that shows the effect of adjustments for other error ratio values, and modifying the summary section appropriately.

Page 1382, line 4: "...components, Normalized Mean Error (NME), ..."

Page 1383, lines 9-11: Suggesting that the sentence be rephrased as follows: "...PBL mixing scheme for CMAQ on 17 September, as mentioned previously."

Page 1385, line 26: I believe that the reference Mathur et al. (2008) deals with an evaluation of the winter-time performance. Hence, it seems that the reference is misplaced. It would be appropriate to move this reference to line 19 or so, where the winter over-predictions are discussed.

Page 1386, line 1, "Figure 6" (also applies to other box plots): What distribution does the data in these box plots represent? In other words, is this a distribution of RMSE across the different sites within each region for each month (i.e., calculate RMSE at each site for the whole month, and then represent the RMSE across all the sites within a region as a box plot)? Or is it something else? Please clarify as to what these plots represent.

Page 1386, line 24: Just to verify the terminology "absolute bias" – Is this supposed to be "absolute error"?

Page 1387, lines12-14: "The decomposition of the RMSE displays different error characteristics for PM2.5 relative to those noted previously for O3..." This statement reinforces the fact that the behavior of PM2.5 is different from that of O3 and supports the earlier comment that the sensitivity of the adjustment to the choice of the error ratio must be examined.

Page 1388, lines7-9: I am not sure if this sentence can be stated with certainty. The improvement for the concentration bin < 5 μg/m3 appears to be negligible. There may be a very slight decrease in the median RMSE and bias, but the 25th percentile is

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virtually the same as, or higher than, the unadjusted values. I am being picky here, as these concentrations may fall within the current uncertainty of the models. But the point is that no marked improvement is seen for that concentration bin to make that statement.

Page 1402, Figure 7: The figures need to be labeled as (a), (b) and so on.

Page 1403, Figure 8: It may be better to reverse the order of the box plots to be consistent with the other figures. Instead of showing MOD and then KF, it would be better if it is rearranged to show KF, followed by MOD. An alternative approach would be to change the other figures to show MOD followed by KF. I personally prefer the 2nd approach where we show the raw first and then the adjusted. But I leave it to the authors on how they want to arrange those, as long as they are consistent on all plots.

Figure 8, caption: I would suggest rephrasing it to state "...for all subregions..."

Interactive comment on Geosci. Model Dev. Discuss., 2, 1375, 2009.