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Geoscientific
Model Development
Discussions

Interactive comment on "Development of Korean Air Quality Prediction System version 1 (KAQPS v1): an operational air quality prediction system with focuses on practical issues" *by* Kyunghwa Lee et al.

Anonymous Referee #2

Received and published: 19 September 2019

General Comments: This paper discussed an application of offline WRF-CMAQ simulations and evaluation both with and without DA during the KORUS-AQ measurement campaign in Korea during May 1 – June 12, 2016. While the paper is well written, and the DA methodology is sound, there are some concerns on missing details and discussion throughout the paper (see Specific comments), rather expected model performance results, and using a such a case study/campaign application as an appropriate demonstration for the readiness of an "operational" air quality forecast system (below). While I recommend accepting the article for publication, these issues should

be addressed.

I think it is a bit misleading to call this application of WRF-CMAQ simulations a true "operational" air quality forecasting system, as it is currently only applied for a very short time period (May 1 – June 12) during a detailed measurement campaign. Such detailed observations are likely not available to demonstrate a continuous refresh of satellite/surface observations for DA in an operational system. To support this model system's use as "operational", what confidence is there that the statistical performance will scale to the remainder of the calendar year under different meteorological and chemical (i.e., emissions) environments? I think the "operational" focus of the paper should be dialed back in the paper, in place of a focus on the application of offline WRF-CMAQ chemical DA and evaluation during an intensive measurement campaign in Korea. Towards the end of the paper, it could be discussed of this WRF-CMAQ configuration could be further developed and more comprehensively tested to become an operational air quality forecasting system for Korea.

This issue is compounded by the somewhat expected results in the paper of improved model performance by assimilating data (compared to no DA), the impacts of a higher frequency rate of DA, and lack of testing the important "trade-off" that exists between increased precision and computational cost of DA. In fact, the authors explicitly state that this unsurprising result needs to be further tested, and that the true operational system should be designed under these considerations. These facts further substantiate my concern that it may be premature to consider this paper a description of an "operationally ready" model, but rather an application of testing DA in CMAQ during a relatively intensive KORUS campaign, which is a region that would certainly benefit from further development and testing of an operational air quality forecast model.

GMD Review Questions: 1. Does the paper address relevant scientific modelling questions within the scope of GMD? Does the paper present a model, advances in modelling science, or a modelling protocol that is suitable for addressing relevant scientific questions within the scope of EGU?

Yes.

2. Does the paper present novel concepts, ideas, tools, or data?

Yes.

3. Does the paper represent a sufficiently substantial advance in modelling science?

Yes, but some exceptions (see Specific Comments).

4. Are the methods and assumptions valid and clearly outlined?

Yes.

5. Are the results sufficient to support the interpretations and conclusions?'

Yes, with a few exceptions.

6. Is the description sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? In the case of model description papers, it should in theory be possible for an independent scientist to construct a model that, while not necessarily numerically identical, will produce scientifically equivalent results. Model development papers should be similarly reproducible. For MIP and benchmarking papers, it should be possible for the protocol to be precisely reproduced for an independent model. Descriptions of numerical advances should be precisely reproducible.

Yes.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes.

8. Does the title clearly reflect the contents of the paper? The model name and number should be included in papers that deal with only one model.

Not necessarily, as I have issues with this model being a true "operational" ready at

this point. See General and Specific Comments.

9. Does the abstract provide a concise and complete summary?

Yes.

10. Is the overall presentation well structured and clear?

Yes.

11. Is the language fluent and precise?

Yes.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

No.

14. Are the number and quality of references appropriate?

No. There are missing references.

15. Is the amount and quality of supplementary material appropriate? For model description papers, authors are strongly encouraged to submit supplementary material containing the model code and a user manual. For development, technical, and benchmarking papers, the submission of code to perform calculations described in the text is strongly encouraged.

Yes.

Specific Comments:

S1. Lines 88 – 93: Don't like the interchanging of air quality and chemical weather terminology here, as it is too similar to suggest that online chemical weather feedbacks are necessary in air quality models. I suggest revising to avoid any confusion.

S2. Lines 95 – 96: This is partially being overcome by new high spatial and temporal resolution satellite observations of composition (e.g., TEMPO, TROPOMI, GEMS, etc.). I think although they lack the longer term records, it should be mentioned that strides are being made at tackling these issues for air composition observations.

S3. Lines 98-101: What about the issues of coarse model grid scale for CTMs?

S4. Lines 142-155: This section is lacking information. 1) What are the dynamical/physical configurations for WRF (e.g., LSM, land use data, sfc layer, PBL, grid scale microphysics, convective cloud parameterization, etc.) ? 2) The met processor needs to be defined (i.e., MCIP) and explained for important derived variables from WRF to drive CMAQ. Many physical inconsistencies can arise between WRF-MCIP-CMAQ, and this is pivotal information in understanding the physical linkages between the upstream physics in WRF to drive CMAQ. 3) How exactly is OBSGRID applied in this model, and how does it relate to the WRF physical configurations? 4) Why was 15x15 km chosen, as opposed to commonly applied forecast models at 12x12 km? While much of this could be provided in supplemental/appendix to preserve brevity in the main text, it still needs to be included somewhere in the manuscript.

S5. Lines 157-177: This section is also lacking information. 1) What are the main chemical configurations for CMAQ (e.g., gas-phase chemistry, aerosol mechanism/size, dust/sea-salt, aqueous phase, dry/wet deposition, etc.)? 2) How does the CMAQ configurations interact and physically link with the upstream, driving physical configurations of WRF (comment S4)? 3) Clarification is needed on why MEGAN, rather than in-line BEIS, was used for biogenics in CMAQ. Is this based on literature for the Korean region?

S6. Lines 230 – 255: I think this is an important discussion, but please make it clear

how much this is a different formulation of AOD compared to the other pre-existing AOD calculations in CMAQv5.1 (e.g., the reconstruction method).

S7. Lines 311 – 316: I have some issue with assuming that ΔPM2.5 exactly scales with ΔPM10, because the inherent differences in some of the sources that make up the PM10 bias compared to PM2.5. In other words, if most of the ΔPM10 is due to missing coarse mode aerosol emissions (e.g., dust etc.), we wouldn't expect this difference to have the same effect on ΔPM2.5.

S8. Lines 391 – 392: Could this also be due to underpredicted NO2 with the DA run and not enough nighttime ozone titration? This perhaps could be better explored using Ox relationships and looking into different regions of the domain.

S9. Lines 397 – 398: It is concerning to draw such a conclusion based only on a single SNU lidar site comparison. Is this truly a widespread issue for nocturnal boundary layers in Korea? While this may indeed be common and well-defined previously using similar WRF physical options as in this study, there needs to be appropriate references here to provide support for your argument.

S10. Lines 406 – 408: This methodology is not clear. What set of MLH observations would be used for this effort? It certainly cannot be based on the single SNU lidar site. How would this be done "operationally" in the offline MCIP step between WRF and CMAQ? Bias correcting the MLH may lead to physical inconsistencies with other driving meteorological fields from WRF that were based on a particular set of physical configurations. Overall, this requires more thought likely, raises some concern, and should probably just be removed from the paper.

S11. Lines 412 - 413: CMAQ already has the "capability" to predict aerosol composition. Thus, it should be restated to say a "…a strong capability of our DA system is to improve predictions of CMAQ aerosol composition".

S12. Lines 416 – 424: These changes in model performance would be elucidated if an addition column showing the absolute bias difference plot (colored in Red Blue shading) for the two runs compared to surface observations in Figure 10. This can be achieved by interpolating the closest model to the observations points.

S13. Lines 428 – 429: This is confusing, as it appears you are talking about an additional model simulation to the DA run. I thought that the adjusted NOx observations are used in the DA run, as discussed for the results in Lines 363-373 and comparing results from Figures 7a-b.

S14. Lines 525 – 529: Is the AI referring to its application to rapid refresh of emissions, chemical reaction/mechanism replacements, or something else? Is there a large body of research that shows AI can even "improve" air quality forecasts? I think the body of work shows that AI can be used speed up the gas chemistry in regional CTMs, while not suffering model performance degradation. Also, if the provided citation to Kim et al. (2019) is of no help, because it is not included in the reference list (see T6 correction below).

Technical Corrections:

T1. Line 120: Tang et al. (2017) is not found in the reference list.

T2. Line 145: Replace “dynamic” with “dynamical”.

T3. Line 147: Replace “National Centers for Environmental Prediction Final Analysis data (NCEP FNL)” with “National Centers for Environmental Prediction (NCEP) Final (FNL) Operational Global Analysis data on 1°x 1° grids”. Is 1°x 1° correct?

T4. Line 413. Replace “matters” with “matter”

T5. Line 501: Replace “ground” with “near-surface”

T6. Line 527: Kim et al. (2019) is not found in the reference list.

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