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Geoscientific Model Development Discussions
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Interactive comment on "Forecasting of regional methane from coal mine emissions in the Upper Silesian Coal Basin using the on-line nested global regional chemistry climate model MECO(n)(MESSy v2.53)" *by* Anna-Leah Nickl et al.

Anonymous Referee #1

Received and published: 10 December 2019

General Comments: This paper provides an evaluation of a modeling forecast system setup to forecast methane plumes emitted from coal mines in the Upper Silesian Coal Basin in Poland. The aim is to forecast the methane plumes in order to assist with the flight planning of several measurement campaigns. An evaluation of the skill of the model at two different resolutions (2.8 and 7 km) as compared to three different airborne observational datasets is presented.

The authors present a comprehensive overview of the biases in this model evaluation paper which is of interest to the scientific community and fits within the scope of the

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GMD journal. However, in general the paper is lacking an in-depth interpretation of the results. More could be discussed in terms of the sources of uncertainty and error. While the authors provide a reasonable interpretation of the results in the Discussion and Conclusion sections, the paper would be more interesting to read if more interpretation and analysis was given throughout the paper, rather than just reporting the biases. In terms of grammar, the paper is generally well written, but the author is inconsistent in using the past and present tense. The paper should be written consistently in either the past or present tense.

Specific Comments: Title and Abstract: I find the title and abstract misleading because most of the evaluation presented in this paper is focused on the analysis simulation rather than the forecast simulation. The title should be changed to something like "Modeling and forecasting of. . ." to reflect this. In addition, the abstract and introduction should also reflect that most of the evaluation is focused on 1) assessing the impact of the model's spatial resolution on the simulation of methane plumes originating from ventilation shafts in the coal mines, 2) assessing the uncertainty in the model's methane concentrations using different air-borne measurements, and 3) comparing the results of using two different emission inventories on peak methane concentrations over the coal mines.

1 Introduction: Page 2, Line 7: Explain isotope carbon-13 and how it can be used to infer sources of ch4 emission.

2 Model and Forecast System: The authors mention updating the applied emissions inventory to EDGARv4.3.2 which could help in reducing the biases. They could also consider using the CAMS-GLOB-ANT anthropogenic global emissions which are currently used by the ECMWF-IFS models and are based on the EDGARv4.3.2 and CEDS inventories and extrapolated to the current year (https://eccad3.sedoo.fr/).

2.2.1 Methane Tracers: Please provide an explanation as to why you are evaluating two tracers. Is it to compare the different emission inventories? If so, it should be clearly stated. Otherwise, if it is just to get the other sources of methane emissions, and if the internal inventory of point sources is more accurate, why not replace the EDGAR emissions over the coals mines with these point sources? The authors should also consider using the CAMS-REG-AP regional inventory for Europe which is developed by TNO in the Netherlands and can be downloaded from the ECCAD data repository (https://eccad3.sedoo.fr/). These emissions are provided up to the year 2016 and are based on more detailed regional information than the global inventories.

Page 7, Line 12: Explain in more detail what is meant by "…however, for the RCP8.5 scenario…". Why is a scenario used?

3 Evaluation of Analysis Simulation: Please clearly explain exactly what is meant by "analysis simulation" for readers who are not familiar with forecast systems. It should be stated that the analysis simulation is constrained by the meteorology.

3.1 Observational data: The flight pattern for J1 and J2 should also be provided. Regarding the flight pattern shown in S2 for P4 and P5, it is redundant to show altitude on the y-axis and in the color-scale. Instead latitudinal information would be more useful.

3.2 Comparison with Analysis results: It is shown here that the model has a slight systematic negative bias in background CH4. Possible sources of the bias, which is presumably inherited from the global model, should be discussed (i.e. emissions, representation of chemical processes, etc.). Specifically, what impact could the prescribed OH field have on the simulated CH4 concentrations in terms of a chemical sink? Moreover, since the aim of the forecast system is to simulate methane plumes arising from the coal mines, I would suggest that the rest of the analysis be performed on the anomalies (with respect to background values) of the simulated and observed methane concentrations rather than the absolute values. This would remove the model's bias in the background methane concentrations and allow for a more straightforward comparison of the peaks related to emissions from the ventilation shafts of the coal mines.

Page 11, Line 13: It can't be seen on the graph that small scale patterns and are better resolved in CM2.8. The sentence should be taken out or more proof provided.

3.2.2 Comparison with in-situ measurements: In discussing Figure 8(a) and (b), the authors state that "Despite the negative bias, peak mixing ratios of CM7 and CM2.8 reach values close to those of the observations. . .", however, it seems that since the simulated background methane has a negative bias (>10 umol/mol) then the model is actually overestimating the increase in the peak methane mixing ratios in the plumes compared to the observations, in particular for CM2.8. Could the authors comment or clarify this point? An evaluation of the anomalies would have been an effective way to remove the model's systematic bias from the background methane and evaluate the model's ability to reproduce the peaks observed in the methane plumes.

Can any conclusions be drawn regarding the relationship between the stability of the boundary layer, spatial resolution and the model's performance in simulating the methane plumes? Accurately simulating the PBL is critical in forecasting the methane plume. Has their model's PBL scheme been evaluated elsewhere? If so, it should be referenced here and discussed.

3.2.3 Taylor Diagram: I don't think that this section adds much information that hasn't already been presented in the timeseries plots. I would suggest to either summarize the results in a meaningful way or to remove it. For example, can you draw any conclusions about the model's bias with regard to the different types of observations? Why are the biases with the J observations lower than the P observations, and why do Tables 3 and 4 show the contrary? The authors should either present a full analysis of the differences in the biases (i.e. instrument type, PBL height, time of day, concentration in the plume, location, windspeed and direction, etc), or simply report on the range of uncertainty that is found using these three datasets which is already quite useful information in terms of assessing the model's skill.

4.1 Theoretical Forecast Skill: I'm not convinced that the Taylor diagram brings any additional information that can't be deduced from Figure 12.

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4.2 Expected Skill Score: Please explain exactly what the expected skill score is because the fact that the model's skill does not decrease in the same manner as the theoretical skill score does not make sense to me. If we assume that the analysis is a "perfect simulation", and compared to the forecast simulation the theoretical forecast skill decreases to almost zero by day 6, how is it possible that the expected forecast skill in comparison to the observations is essentially the same on day 1 as day 6?

The authors present the model biases using different observations but more explanation or interpretation would be appreciated. For example, on page 19 line 1 it is stated that "…Sv is highest for J1 and J2…" but no explanation/speculation is offered as to why.

Again, I don't think that the Taylor diagram presented in Figure 16 adds any new information. It is clear from the plots in Figures 14 and 15, that the model's skill score for predicting the J observations is higher than for the other observations, especially the P observations. What would be interesting is for the authors to offer an explanation as to why this is the case. Why is there more variability in the skill score for the P observations than for the J or C observations? Unless the authors can draw some interesting conclusions such as this, I would suggest removing the Taylor diagrams and replacing them with the HALO and D-FDLR flight patterns.

Page 20, line 7: The authors state "All forecast days show a normalized standard deviation close to 1…meaning that all forecast days show similar amplitudes…". In theory, this can't be deduced from the standard deviation alone.

Technical Corrections: The author should go through the entire paper, especially (but not only) Section 3.1 and make sure they are consistent with either using the past or present tense.

Abstract: Line 4: change "measuring" to "measurement" Line 8: Change the sentence to read "In order to help with the flight planning during the campaigns…"

1 Introduction: page 2, Line 20: change “climate change strategies” to “climate change mitigation strategies”

2 Evaluation of Analysis Simulation: Page 12, line 10: change sentence to “. . .observed peaks in the afternoon flight are lower those of the morning flight.”

Page 13, line 4: change “very precisely” to “more precisely”

Page 13, line 6: change “constant offset” to “systematic bias”

Page 13, line 12: change “. . .simulated boundary layer. . .” to “. . .simulated boundary layer height. . .”

5 Discussion: page 22, line 5: There is something wrong with the sentence “This the intended result given the fact, . . .”. Perhaps a word is missing.

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