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Interactive comment on "Top–down estimates of black carbon emissions at high latitudes using an atmospheric transport model and a Bayesian inversion framework" *by* Nikolaos Evangeliou et al.

Anonymous Referee #2

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This work by Evangeliou et al. performed a Bayesian inverse analysis of BC emission fluxes at northern high latitudes using ground-based observations and a Lagrangian particle model. The authors accounted for the uncertainties associated with BC wet removal with simulations using a range of scavenging parameters and prior emissions. The inversion found high BC emissions from gas flaring in Russia and Canada. The inversion also found that retrieved emission seasonality is different from that in emission inventory in N. America, N. Europe and N.Siberia. The topic is interesting and the paper is overall well organized but the method description still needs some clarification.

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Discussion paper

I support the publication of the paper if the following major comments are addressed.

Major comments The paper lacks sufficient description about the specification of error covariance matrices, B and R, which are useful for readers to evaluate the validity of the inversion method. How large is the observation errors and prior errors? Are they assumed to have any spatial/temporal correlations? Are model errors considered in R?

Also, I find the method description a little confusion. In Line 191, the authors claim that they conduct "ensemble of inversions" and conduct "the inversion for BC represented by 12 different scavenging coefficients and for four different prior emission datasets. However, in Line 358, "the posterior emissions were calculated for the best performing species and best prior emission inventory". So, what are the results of "ensemble of inversions" used for? Are they used in Section 3.1 and 3.2 (The text of these two sections does not mention inversion. I assume that these are results from forward model simulations rather than inversion)? Or are they used for uncertainty estimation? If so, authors need to describe in the Method section how to compute the final uncertainty value from the ensemble inversions.

The authors show that different wet removal parameters have little impact on simulated BC, which is interesting. But the authors can provide more information for readers to better evaluate this conclusion. (1) how BC aging and its hydrophilic/hydrophobic state is considered in the model? What is the uncertainty associated with this process? (2) what is the size of emitted BC, whether the size changes during transport, and is the simulation sensitive to the assumption of BC size? (3) are there evidence to support that the choices of parameter in Table 2 is adequate? (Grythe 2017 tested a larger ranges of parameters).

The uncertainty estimates do not account for model transport errors and instrument errors. Section 2.2 and Figure 1 (g)-(i) show that the footprint varies greatly spatially. One may wonder how does this affect the inversion uncertainty? Can transport errors be more important than the choice of prior emission inventory and scavenging parameters in some regions distant from surface sites? Are observations informative for these regions? One of the major finding is gas flaring emissions from Russia but Figure 1 (g)-(i) show small footprint in these regions. Also, Figure 12 shows that locations of intensive gas flaring are shifted from prior inventory but Figure 1 (d)-(f) shows the inversion grid there is very coarse. It is worthwhile to check the posterior error covariance matrix to see now confident the inversion is about the conclusions?

Minor comments Abstract. The writing of the abstract can be improved. Line 17-19 and 23-25 seem to be out of place.

Line 37: main of atmospheric particular matter (?) Line 39: gas flaring is open high-temperature combustion of natural gas in the oil/gas field. This source may be worth mentioning here because relevance to the results.

Line 80: maybe worthwhile to compare the results with this emission dataset?

Line 152: the removal efficiency depends on BC aging (hydrophilic/hydrophobic) and size. How are they treated in the particle model?

Line 363-366: not clear how these uncertainties are individually computed and then combined. Equations are useful here. And it may also be a good idea to move the description in Method sections.

Line 467-469: This type of uncertainty is not included in current uncertainty estimation but should be available by analyzing posterior error covariance matrix if B and R are properly assigned.

Section 4.4: The inversed emission seasonality is very interesting. I'd suggest summarizing the major finding about the seasonality in the abstract.

Line 553-555: what type of errors in the inversion?

Table 3: "four different optimized emission estimates are giving"? Are they prior or

posterior emissions? I am confused.

Figure 1: It's impossible to read labels in panels (a)-(c). It may be a good idea to use the projection in Figure 6 for reader to compare different figures easily.

Figure 2: Use a consistent projection for panels (a)-(g) and Figure 6 so readers can compare easily.

Figure 3 and 4: Two figures provide very similar information. Consider removing one.

Figure 7 - 9: These evaluations are quite interesting. But since the inversion is done with ECLIPSEv5, it is a fair comparison between posterior emissions with averaged RMSE from four emission inventories. RMSEpri from ECLIPSEv5 should be listed as well.

Figure 9: The difference between posterior and ECLIPSEv5 is very small, which may be indicative that the observations are not informative to emissions in this region. See major comments for uncertainty estimations.

Figure 11-12: "Black rectangles show vegetation fires adopted from Hao et al." Choose another color. Black color is already used for map borders. Also, they are better referred to as dots rather than rectangles.

Figure 13: The prior emissions plotted in panel d (N.Siberia) of Figure 13 seems to be wrong as they are much lower than the annual total emissions reported in Table 3.

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