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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Multi-model study of mercury dispersion in the atmosphere: Atmospheric processes and model evaluation" *by* Oleg Travnikov et al.

Anonymous Referee #2

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This paper reports the intercomparison of 4 models used to study the evolution of mercury in the atmosphere.

I'm really skeptical to suggest to accept this manuscript for publication on ACP, at least in the present form for the following reasons:

1) The main goal of this paper is to evaluate models performances and to study the atmospheric processes that control the mercury dispersion in the atmosphere. The main issue is that the 4 models used are completely different (see paragraph 2.2) in terms of spatial resolution (1 to 2.8 degrees in lat. and log.), upper boundary domain

(from 30 km to 80 km), primary and secondary emission parametrization, chemical scheme utilized. Under this heterogeneity of the models it is hard to find a process or a chemical scheme that is better than the other or useful to explain or to better understand how mercury is dispersed in the atmosphere.
2) Page 9, lines 12-19. In describing the comparison of model simulations and observations reported in figure 2, authors report: "models predict similar spatial pattern of the observed Hg with pronounced gradient between Southern and Northern Hemisphere". It is correct that qualitatively all the models reproduce the north to south gradient, but the absolute values are completely different from model to model and also the spatial distribution. The question is: Which one is better? Would be interesting to explore the reasons of these differences between models and give the reasons that can be useful to other modeler. Moreover this will help to understand the process and the chemistry important to predict mercury with models.
3) Page 9, lines 22-25: Authors report: "models demonstrate lower spatial variation of annual GEM concentrations than the measurements do. This can be partly explained by relatively low spatial resolution of the model grids (1-3 hundreds of kilometers) that can hardly allow them to reproduce local meteorological conditions at measurement sites". First of all, if this poor spatial resolution of the model explain only partially the disagreement between model and observation spatial variation, explain the other reasons. On the other hand, from this sentence is evident that a model with high spatial resolution is required to explain the observations more quantitatively, otherwise with models we can have just a general picture.
4) Page 10, lines 9-11: Authors report: "The models predict some decrease of concentration further northward, which is not evident from the observations. It can be connected with overestimation of the oxidation chemistry in the Arctic or with underestimation of Hg re-emission from snow and seawater". This is really important to know: is it the oxidation scheme in the model or underestimation of the Hg re-emission from snow and seawater? Addressing this point is something that help to go further in our understand of mercury chemistry, and is mandatory to give some evidence of the

process responsible for the model-measurement discrepancy, otherwise which is the added value of using 4 models?
5) Page 10, lines 33-34 and page 11, lines 1-2: Authors report: “Simulations with different chemical mechanisms (BrCHEM1, BrCHEM2, O3CHEM, OHCHEM) do not lead to considerable changes of both spatial distribution and temporal variation of GEM concentration in the surface air. Somewhat better spatial correlation was obtained for the oxidation reactions with Br (BrCHEM1) and OH radical (OHCHEM) and worse for the reaction with ozone (O3CHEM)”. This conclusion are a bit confusing: if as reported at the beginning, different chemical mechanisms do not influence the models performance, this means that chemistry is not an issue. On the other hand if is correct that somewhat better spatial correlation between measurements and simulations can be achieved with Br and OH oxidation reactions and worse results with O3 oxidation, then it is this the direction to explore: why Br and OH chemistry improve the performance of the models and O3 chemistry does not?
6) Page 13, lines 16-18: Author report: “The deviations between the modelled and measured RM/GEM are again mostly within a factor of 5 and the model-to-model difference is probably resulted from application of somewhat different reaction constants”. If the differences are probably due to different reaction constants used by the models, this is another variable to take into account and this further issue does not help to understand the problem. What happen if all the models use the same reactions constants? This test could be useful to exclude one of the uncertainties of the model simulations.
7) Page 14, lines 2-8 and figure 8: The simulations of RM/GEM is a disaster for the ECHMERIT model that show a maximum during summer with all the chemical schemes, whereas the observations show a maximum in March and a minimum during summer. Also the other models do not do a good job in reproducing the observation of RM/GEM, only GEOS-Chem is quit fine with the BRCHEM1 scheme. This is a big issue that need to be explored and the explanation can help to go further with our knowledge of the mercury chemistry.

8) Page 15, lines 7-12 and figure 10: It is not a good idea to exclude one model (ECHMERIT) results from the statistics only because excluding the results of this model improves the overall performance of the models simulation. This exclusion does not help to understand what is going on, and if one model is excluded only one time the results show in figure 10 are not comparable with those of the other figures since here the statistics includes 3 models and not 4. On the other hand, this exclusion of one model indirectly says that one model is worst the others: is this an indirect conclusion of Authors?

Minor comment:
Page 5 lines 4-5: It is missed the table number.
Page 10, line 19; It is fig 3 (c) and not Fig. 3 (b).

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