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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A multi-model intercomparison of halogenated very short-lived substances (TransCom-VSLS): linking oceanic emissions and tropospheric transport for a reconciled estimate of the stratospheric source gas injection of bromine" *by* R. Hossaini et al.

Anonymous Referee #1

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I realize that this paper concerns a multi-model effort, and that it is difficult to analyze a large number of models and find common physical threads and results. Still I feel that this manuscript is lacking discussion of some things that raise obvious questions, and it would be worth the effort for the authors to make serious revisions.

Hossaini and co-authors describe a multi-model intercomparison that attempts to develop a reconciled estimate of the stratospheric injection of bromine. The paper is mainly descriptive. I suggest revisions that will place results in better context and

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Discussion paper

strengthen the paper.

Main suggestions for revision:

1) Rewrite the objectives. Although the paper meets the first two of the stated objectives (lines 95-100), the third and fourth objectives do not receive the attention of the first two. Objective (c) examines trends and inter-annual variability in the stratospheric loading of VSLS and (d) investigates how these relate to climate modes). The discussion of point (c) is limited to transport (mostly derived from assimilated meteorology) and point (d) is barely considered.

2) Include some discussion of CTM/CCM differences, and the factors that control whether or not CTMs with the same meteorological fields yield the same or similar results. Where different, the differences should be attributable to differences in CTM setup. Four of the 11 CTMs use ERA-Interim, and in addition, one version of EMAC is 'nudged' to ERM-interim. Two CTMS use JRA-25, one uses MERRA. There are three free running models; these will give similar results to the CTMs only if free running climatology is similar to the assimilated climatology. Although differences are said to be 'transport' – does that mean real differences in meteorology (e.g., differences between assimilation or free-running), differences in implementation of a single analysis, or differences among the analyses? When 70% of the models (or 8 of 11 models) do something does that mean the 8 models that use assimilation differ from the free running models? If convection and boundary layer mixing are dealt with differently among the CTMs, and are demonstrably different from the CCMs, then there should be some mention.

1) Include physical interpretation and a sense as to what we learn from 'lack of sensitivity of the simulated seasonal cycle to the choice of inventory' (line 615). If the mean value is sensitive to the inventory but the seasonal cycle is not, does that mean anything more compelling than that the seasonal cycle of the loss process (input to the simulations and the same in all models) is realistic?

2) Quantify the importance of SGI of VSLS to the total stratospheric bromine budget. It would be helpful to put the difference in SGI from WMO best estimate ($\sim$ 1.3 vs 2.0 (this work)) in the context of the stratospheric budget. IAV is +/- 5%? Is that significant? Is uncertainty in SGI more or less important than uncertainty in product gases? How large is the uncertainty in SGI + product gases relative to the total stratospheric bromine budget? Is the uncertainty in SGI + product gases smaller than the uncertainty in SGI?

3) How important is it that SGI does not show a transport trend? Isn't it just as likely that a trend (if any) would be due to a trend in the sources (as mentioned in penultimate paragraph)?

Specific Comments

Imprecise language throughout – paper has sufficient quantitative statements and comparisons that qualified descriptions detract from overall message. These are examples: 'reasonably well' 'not particularly sensitive' 'at most sites the amplitude of the seasonal cycle is generally consistent' 'to some degree likely reflects' 'likely' – followed by 5 references – how many do we need to make a concrete statement?

Why 'models are able to reproduce'? Why not 'models reproduce'?

You don't need to repeatedly say 'participating models' (unless you are also showing output from models that did not participate).

Abstract and Introduction Not clear until section 2.3 that most of the models are CTMs. Very surprising and possibly misleading that nothing is said about input meteorology. Differences among CTMs that use the same source for meteorology are differences in implementation since all of them would claim that they are trying to solve the same general equations with the same meteorological input.

Line 30 – transport driven variability in the annual mean SGI is 5% - why is that 'however'? Isn't that small?

Line 52 delete last phrase 'in recent years' – very long sentence already says 'recent'

Line 55 and following: why is it important to differentiate the product gas injection from source gas injection? Is the NET impact of VSLs (PGI and SGI) better constrained that SGI or PGI separately?

Line 64 – should be 'coincides'

Line 83 – it seems to me that the robust evaluation of the ACTUAL SGI needs observations, not just a concerted model evaluation. All the models could give the same answer (especially if they all use the same input meteorology) and data might reveal them all to be wrong.

Line 115 – you specify the chemistry – thereby ELIMINATING (rather the minimizing) its contribution to inter model differences. Also – since most of the models use ERA-interim, and there is no discussion of differences in its implementation, it is somewhat misleading to say that this study isolates differences due to transport processes.

Line 136 – is aseasonal the same as 'annual average'?

Section 2.1

It would be useful to have some visual comparison of the emissions (perhaps supplementary material)? The words don't give a sense of how large the differences in emissions are, and without that the sensitivity to emissions or lack thereof does not make sense.

Line 160 the words after the semi-colon should have a verb, or the sentence should be re-written without a semi-colon.

Line 184 'diagnosed convection' – do you mean used the standard parameterization for transport? Identified convection? Not clear.

Section 2.3 Did the CCMs use observed sea surface temperatures (relevant for El Nino)?

Section 3.1.1 Line 300 – MHD, THD, CGO, PSA - simulated seasonal cycles do not

agree with data – does the simulated seasonality look like the imposed seasonality of the loss terms? (If it does, then how can the models perform differently?)

Line 315 – Is it important that the observed annual variation is much smaller at SMO and CGO than at many of the other sites? Disregarding SMO (some really weird behavior), CGO and PSA amplitude greatly overestimated although shape is vaguely similar. Any commonalities among the 60% of models that do not correlate with observations > 0.5? Resolution? Meteorology? Transport scheme? Boundary Layer dynamics? Anything?

317 – virtually all do not reproduce – how about 'almost none of the models reproduce' or 'virtually all of the models fail

I don't understand the point of this discussion (lines 317 ff) At MHD, seasonality in the local emission flux is suggested to be the dominant factor controlling the seasonal cycle of surface CHBr3 (Carpenter et al., 2005). This leads to the observed summer maximum (as shown in Figure 3) and is not represented in the models' CHBr3_L tracer which, at the surface, is driven by the aseasonal emission inventory of Liang et al. (2010). Why did Carpenter et al. make that 'suggestion'? This sort of model can only do what you tell it, so if Carpenter et al. are correct – then you would never expect the models to do this. So then, what is the point of going to the MMM? Why aren't you discussing whether an emission inventory that has a seasonal element does better? It would make more sense if there was a better sense of the differences in inventories. Specifically – why would TWO aseasonal inventories give different answers at MHD, if the seasonality of the emissions is speculated to be a controlling factor?

Section 3.1.2 338 between a model value (M) and an observation (O), why parenthesis around 'for each model tracer'?

Figure 6 – ok these are the minimum percentages – but how does the reader know that the difference between a 'best' comparison and a comparison with one of the other inventories is significant?

350 I presume you don't get MAPE for both species with the same inventory because loss processes are different time scale? Replace low CHBr3 MAPE (good agreement), at a given location using a particular inventory does not necessarily mean a corresponding low CH2Br2 MAPE can be achieved using the same inventory, at that location. with At a given location low CHBr3 MAPE (good agreement) does not necessarily accompany a corresponding low CH2Br2 MAPE using the same inventory 355 – is this also related to how the inventories are created in the first place – i.e., how much do the inventories themselves depend on models and/or ERA-Interim? 365 – you attribute all differences to physical processes – e.g., convection and boundary layer mixing. Since most of these use assimilated meteorology, does that mean implementations differ among CTMs. Also, there are some pretty large differences between a free running simulation and a nudged simulation, so differences among 'variants' should not be surprising. Finally, in the prior modeling studies that had best agreement with different inventories, the loss terms were presumably different.

370 – why are differences in model variants surprising? In one case, this is the difference between free-running and nudged, and it is more likely than not that convection differs between these two in both intensity and location. In the second case, the chief difference that is discussed in convection, so again, performance is more likely to be different than it is to be the same.

395 It would be better to say 'For the N (fill in number) models that submitted hourly output . . . After that, paper says "Generally, the models reproduce the observed mixing ratios from SHIVA well, with a MMM campaign MAPE of 25% or less for both VSLS." This good agreement clearly depends on who is looking, and whether it makes sense to compute MAPE for the multimodel mean when the spread indicated by shading can be as much as 1 ppt (lowest value) and about 2 ppt (MMM) (top panel). It is also confusing since each model is using its 'preferred' inventory, and seriously in the real world there is only one actually set of emissions. In the best of circumstances, I think the MMM conceals physical differences and/or deficiencies in a subgroup of

models. Here, with each model using its 'preferred' inventory, I think it is nearly impossible to understand they significance of good or poor agreement with the MMM. Section 3.2 412 – by using the model 'preferred' inventory, what you are testing here is given surface values, how similar is the transport to higher levels to that inferred from observations in the real atmosphere. There are other ways to do this of course – in fact, looking at EACH tracer profile as a fraction of its near surface value might be even more instructive. Nonetheless – the discussion is convoluted and should be re-written to state the main (physical) point clearly. I presume 'paramerized transport schemes' later in this paragraph refers mainly to convection?

447 Only the number of flights controls the variability comparing Pre-AVE to CR-AVE? Nothing seasonal or spatial? Are the models sampled like the aircraft to produce average profiles? Is the error bar the range of values, the standard deviation? Would standard error of the mean be better? The correlation coefficient – is that the correlation for the whole profile? Isn't that guaranteed to be large since observed and simulated profiles general decrease with altitude?

ATTREX higher values at higher altitude 'possibly reflects the location'? Isn't this true (and backed by other observations?) If it is only 'possible', what are the other causes? grammar - CR-AVE had nearly twice the number of flights AS Pre-Ave and . . .

Section 3.3

470 ' likely reflects the location at which the measurements were made' Why so many words, why 'likely' (what else could it be) and why no direct statement about zonal asymmetry? Would the model zonal means compare better with Carpenter and Reimann? Or should it be model mean in a different region compared with Carpenter and Reimann?

Section 3.4

515 If most of the models are using assimilated fields, how can they fail to locate

the areas of deep convection and the seasonal dependence therein? It is all right to describe this behavior, but I would hardly call it a prediction. Too much discussion, especially since the result is not novel.

525 – variations in the importance of Monsoon – any connection to the input data or model type? I don't think this is evidence that UKCA-HI has a more faithful representation of convection – you would need some other information about HI vs LOW to make this statement.

Section 4

605 previously when you talked about variability it was physical (e.g., seasonal etc. – something real). Here you are talking about differences among models for different inventories. It is confusing to call this 'variability'.

622 – model variants are identical except for tropospheric transport schemes. Based on everything else written, I don't think this statement is correct. E.g, the tropospheric transport of 'nudged' vs 'free-running' will differ for physical reasons, not just 'transport schemes'.

The problem with single model studies of inventories or deriving inventories is that they don't typically include model error. If they did then the inventories would be more robust – or the differences among studies would likely fall within the errors.

625 – For both CHBr3 and CH2Br2 the 'best' inventory for the tropics is the lowest – but at the same time agreement here is 'less sensitive to choice of inventory'. What point are you trying to make? I don't see how the statements about seasonally resolved air-to-sea fluxes follow from anything in this paragraph (noting this is the 'discussion and conclusions' section).

665 – the very long sentence beginning 'Although . . . ' should be clarified.

Picky comment Example: Do you really need to include so many references – e.g., five references to say that Bromine + chlorine destroys ozone more than chlorine by itself?

There are other examples of many references fo

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