Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2020-1195-RC1, 2021

Comment on acp-2020-1195

Julia Marshall (Referee)

Referee comment on "Estimating 2010–2015 anthropogenic and natural methane emissions in Canada using ECCC surface and GOSAT satellite observations" by Sabour Baray et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1195-RC1, 2021

The study presents an inversion analysis with a well-established modelling system using both in situ measurements from the ECCC network and GOSAT satellite measurements. The targeted region for analysis is Canada, and only fluxes in this domain are adjusted and only measurements over this domain are assimilated, which may be problematic (see discussion below). Various state vector setups are presented, where the anthropogenic and biogenic fluxes are scaled separately in time (annually or monthly, respectively) and based on anthropogenic sector or province. Due to the limited number of measurements, these latter approaches are rightly judged to be less robust. The study is interesting and relevant, although I have some concerns about the approach.

My major concern with this study has to do with the use of a global simulation where only the Canadian emissions are allowed to be optimized, and where only measurements over Canada are assimilated. It is stated that the "initial conditions from January 2009 are from a previous GOSAT inversion by Turner et al. (2015) which was shown to be unbiased globally when compared to surface and aircraft data". Is this only for the initial 3D fields, or are the optimized fluxes also used for the extra-Canada domain? If not, I am concerned that the flux adjustments seen in Canada might actually be the results of errors elsewhere being adjusted the only way the state vector allows. All validation data are within the Canadian domain, but it is not clear that the fit upstream and downstream of these flux increments is equally consistent. This could be easily tested by comparing a forward run of the prior and posterior fluxes to e.g. soundings outside of the optimized domain. If the adjustments lead to a significantly poorer match to these non-assimilated measurements, there may be a problem with this approach. Methane is long-lived, so not only the measurements immediately downwind should be considered. This is the minimum analysis necessary to test if this approach is reasonable.

Figure 3, and the discussion of the "acceptable reproducibility of background methane": Again, I am not convinced by this argument that these background fields are so accurate that one can reasonable optimize only Canadian fluxes because everything else agrees so

well. Yes, the variability is well matched, but a bias of 5.3 ppb is certainly larger than the measurement uncertainty and, more importantly, looking at the bias alone seems to underestimate the difference. In this plot there seems to be an overestimation of the observations in the earlier part of the record, but by the last year the model seems to be underestimating the measured concentrations. If this boundary condition accepted as is, this difference in the trend will be mapped entirely onto the optimized Canadian fluxes, and not the upstream mismatch where the correction belongs. This trend in the model-observation mismatch appears in Figure 6 as well, even though it is only showing a subset of the simulation period (why?), but at least some of this is already apparent in the un-optimized western boundary. (Notably, the trend of the emissions correction that would be needed to correct this error is positive, just as the trend seen in the ECCC inversion for wetland emissions.)

Furthermore, the argument that Maasakkers et al. (2020) showed "relatively minimal" adjustments to US emissions near the Canadian border does not mean that US fluxes from further afield do not affect concentrations measured in Canada. Yes, the winds are generally westerly, but air certainly crosses the border in both directions. Not to mention that the stations Egbert and Sable Island have a great deal of US signal when considering only westerly flow, as they are well south of the 49th parallel.

The suggested increase in biogenic fluxes from 2010-2015 from the in-situ network is massive – this is on the order of 10% per year! This would be an extraordinary finding, if it can be substantiated. How might this be tested? Did you consider looking at isotope measurements, for example? Why might this not be seen in the GOSAT-only inversion? Why were the GOSAT and ECCC measurements not combined in this "standard" inversion setup as well (as they were in the "policy-themed" inversions presented in Section 3.4). It seems an obvious natural step to do so, to see if this trend is still apparent.

Once these concerns have been addressed the study would be appropriate for publication, but until the robustness of this "regional adjustment only" approach has been tested against independent measurements upwind and downwind of Canada in forward runs of both the prior and posterior fluxes, the scientific conclusions cannot be considered sufficiently robust.

Minor/typographic comments

L17: have been conflicting -> conflict

L26: slower -> a slower

L35: specify anthropogenically-influenced GHG: CO2 is less significant than H2O…

L54: because only a 3% source-sink imbalance, -> because a source-sink imbalance of only 3%,

L58: Please specify that the "Canadian greenhouse gas inventory" is not just an inventory of some prior integrated over Canada, but rather the government report of emissions submitted to the UNFCCC. This is a bit confusing. It's mentioned in the abstract and fully capitalized, as if it were the proper name: "the Canadian Greenhouse Gas Inventory". But then it's also called the "National GHG Inventory" (also capitalized, also in the abstract), and then here just "the Canadian greenhouse gas inventory". None of these match the title of the actual document, which should be explicitly introduced in the introduction.

L73: compromising interpretation -> compromising the interpretation

L83: wetlands fluxes -> wetland fluxes

L86-87: „an increase in" and "a decrease in" would be clearer than "upscaling" and "downscaling" in this context, which could be interpreted as spatial extrapolation/(dis)aggregation.

L93: insert comma after first use of "emissions"

L113: insert commas before and after "Estevan Point (ESP)"

L126: mol -> mole

L127: local time for when -> local time, when the

L129: western most -> westernmost

L136: I guess you mean the largest methane fluxes *from wetlands* in North America?

Section 2.2: I was surprised to see biomass burning not mentioned explicitly in the text, but only listed in the table. It can have quite a bit of interannual and regional variability. I was also surprised to see that the termite emissions were identical to those of biomass

burning (in Canada??), and also geologic seeps. Is this just a coincidence, or were these three small sources just distributed evenly over the three (rather different) prior spatial distributions? Please clarify this, also in the text.

L191: A couple concerns here, one minor and one major. Here it is optimistically stated that the spatial pattern of emissions "may" show less agreement: this is almost certainly the case, just from a statistical perspective. The major concern has to do with the use of a global simulation where only the Canadian emissions are allowed to be optimized, and where only measurements over Canada are assimilated, but this is discussed elsewhere.

Figure 2: it seems a mistake that the contiguous US/Greenland is not screened out in panel D (but Alaska is), while it is for the other three panels.

L250: remove "done"

L259: needs a connecter after the comma (e.g. "such that the", "wherein", "and" …)

L281: insert "and" before "other"

L293: space missing?

L294: Did I understand correctly that the in situ data were averaged over the local afternoon each day, essentially giving just one data point per day per station (as described in line 127)? If so, a mean observational error of 65 ppb seems rather massive! Can this be attributed to a poor representation of the spatial distribution of the fluxes, which is not optimized explicitly? The only way the model can adjust the spatial distribution is by changing the weighting of the various categories.

L374, L376 (and elsewhere – find and replace): change "wetlands emissions" to "wetland emissions"

L396: and compares -> and compares them

L400: from region -> either “from regions” or “from the region”

L406: Is Egbert really sensitive to emissions from the Hudson Bay Lowlands? This surprises me. Fraserdale, sure, and maybe Chibougamau, but Egbert? Out of curiosity: for the simulations shown in Figure 6, were the different WetCHARTS scenarios also used for the US fluxes, or were these fixed? Also, please specify that the anthropogenic and “other natural” fluxes from Table 2 were used in the forward simulations shown in Figure 6 (which I assume to be the case).

L588-590: I don’t understand this sentence entirely, there seems to be words missing. Perhaps you mean: While there are about 5 times more GOSAT observations than ECCC observations for use in the analysis and the in-situ observations have larger observational error in Sa (due to model error), the surface measurements are much more sensitive to surface fluxes, which offsets the weight of the larger amount of GOSAT data. Or something like that?

L688: should “or” be “and”?

In Supplement:

P7 L167: out -> our

Figures S4 and S5: I wonder if these figures might not be easier for the reader to interpret if they were presented as matrices/surface plots? The amplitude of e.g. the singular vector decomposition in the bottom plots could still be indicated somehow, or even kept as line figures, which would help avoid confusion about the interpretation of dashed lines in the middle and bottom panels of Figure S5.