Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-213-RC1, 2017

Geoscientific Model Development Discussions

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GMDD

Interactive comment

Interactive comment on "Description and evaluation of REFIST v1.0: a regional greenhouse gas flux inversion system in Canada" *by* Elton Chan et al.

Anonymous Referee #1

Received and published: 3 January 2017

This study investigates the performance of a regional inversion of anthropogenic CO2 emissions using synthetic experiments varying the prior fluxes, transport model and optimization method, and inversion setup. Conclusions are drawn regarding the optimal number of regions to be optimized, and the relative importance of transport model uncertainties. However, as will be explained further below, it is not clear what we learn in the end. Most of the findings that are presented will depend on the specific setup of the inversion that is chosen. However, since this setup is far from realistic – the practical significance of the results for regional inverse modeling of CO2 using real data remains unclear. In my opinion, this point will have to be addressed clearly in the revised version of the manuscript to make this manuscript acceptable for publication.

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In addition, to improve readability I recommend that the authors focus only on the main findings, which could substantially reduce the size of the paper.

GENERAL COMMENTS

The authors recommend that an inversion system is first tested in a synthetic environment before it is applied to real data. I fully agree to this, however, since the outcome of such an experiment depends on the specific details of the setup it should be realistic in the sense that the same setup could directly be applied to real data. This is clearly not the case for the setup that is presented here, since it only addresses anthropogenic emissions of CO_2, ignoring the natural component of the regional carbon cycle. In addition, the boundary conditions of the regional domain that is optimized are assumed to be known exactly. Since the regional biospheric fluxes are expected to be the main uncertain component, it is unclear to me how this inversion is supposed to work when applied to real data. It is mentioned that the methodology could be applicable to wintertime CH_4 fluxes. But if this is the application that the authors have in mind then why perform a test inversion for fossil CO_2 instead of CH_4?

The authors comment on the estimated posterior uncertainties in relation with the actual deviations from the predefined true fluxes, concluding that the estimates are too optimistic. However, this conclusion depends on how the assumed a priori flux and data uncertainties reflect the actual errors. It seems that no effort was made to analyze the statistics of the difference between for example CT2011 and CT2010 before specifying the a priori flux covariance. The same is true for the model-data mismatch. In this case, how can the method of calculating posterior uncertainties be blamed of inconsistencies? I wonder also how representative the difference between CT2011 and CT2010 is for uncertainties in fossil fuel CO_2 emissions. They can certainly not be considered independent estimates of fossil fuel fluxes.

Conclusions are drawn regarding the relative performance of Bayesian cost function minimization and the MCMC method. However, how can different methods to find the solution of an inverse problem be compared if they are applied to different inverse problems? A clearer distinction should be made between optimization method and inversion setup. If the MCMC method was applied to the same optimization problem, one would expect to find the same solution – unless one method fails to find the optimum, e.g. because of non-linearity. In this case, at least the CFM inverse problem seems linear, so it should be capable of finding the right solution. The difference between MCMC and CFM seems more in the assumed statistics (inverse Gamma versus Gaussian). But then if MCMC performs better it is probably because of a different weighting of outliers. Further analysis is needed to gain understanding of what is causing the difference between the two methods.

The discussion about aggregation errors and the optimal number of regions seems to have been influenced by the treatment of a priori flux uncertainties. If a region is split up in two equal parts, then care should be taken to specify the uncertainty of the separate regions such that they don't alter the uncertainty of the combined region. In this study, it seems that 100% uncertainty is assumed regardless of the size of a region. But then if the individual fluxes are assumed to have independent uncertainties, the aggregated uncertainty of the whole region will go down as it is split up into a larger number of sub-regions. This is because the errors of the sub regions will partially cancel out in the regional integral (with the square root of the number of regions). Unless this issue is dealt with carefully, it will confuse any inferences about aggregation errors.

SPECIFIC COMMENTS

line 10: 'Increasing the number ...' Does this mean that none of the set ups is significantly different from 'unstable' and 'unrealistic'?

line 13: 'prior R2 $\sim$0.8' Wouldn't it be better to quantify transport model error using the true fluxes (otherwise it is unclear which part of R2 is due to the prior flux uncertainty).

line 16: 'a poorly simulated station' Why not just mention the station here?

line 19: 'improvements are needed with the current inversion setup ...' It seems that the data availability is the problem. This sentence suggests that an improved setup can compensate for missing measurements. It may be that the problem is in the word 'setup', but then this should be formulated more clearly.

line 28: 'These atmospheric mole fractions fractions ...' Here reality is described as if it is a model. Please change the formulation to avoid confusion.

line 75: What is a positive definite flux distribution. The term 'Positive definite' refers to a symmetric matrix.

line 81: the spatiotemporal distribution of regional CO2 and CH4 fluxes are rather different.

line 94: The sensitivity of the estimation error and uncertainty to what?

line 128: What makes these two estimates suitable as prior and truth?

line 130: How were the fluxes redistributed to 0.2 x 0.2?

line 157: Something must have gone wrong with this definition of retroplume.

line 161: The unit of the multiplication is kg/kg*m3 i.o. mole fraction

line 162: Prior means before here, rather than a priori, right? Please avoid confusion here.

line 174: How were the station specific baseline time series quantified? It sounds like you calculate the baseline contribution from the back plume initializations which you then subtract. However, in reality you don't have the 'true' initial values.

line 209: but you apply an a priori constraint to lambda, which is effectively equivalent to the regularization in CFM.

line 222: If no regularization term is used in MCMC than how can it use the same prior error?

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line 249: Why is it necessary to compare means? Since in the Gaussian assumption mean and median are the same, you might as well compare medians.

line 251: Why do you take the average of intermediate solutions in the iterative optimization? Shouldn't the optimum solution be the end point to which the iterative chain converges?

line 252: Since lambda is defined as time independent this sentence is not needed anymore (better would be to state explicitly that lambda is time independent at the point where it is defined).

line 263 - 265: But those scaling factors are intermediate solutions in a optimization process, therefore they are not independent optimal solutions of the inverse problem. For this reason, I don't see how the statistics of the scaling factors could represent the posterior flux uncertainty.

line 321: I would rather call the prior flux error a disaggragation error, since it is mostly the spatial disaggragation which is different between CT2010 and CT2011.

line 338: It is still no cler to me what causes the baseline error in this inversion.

line 344: 'an example of one inversion experiment', which inversion experiment?

line 363: Why is the same representation error of 30% used for all sites, when some sites are easier to simulate by the model than others? By the way, 30% is 30% of what? The deviation from the baseline?

line 374: 'representS'

line 471: This paragraph refers to the same figure as the one before, but why then do you explain the figure here and not before?

line 483-484: I think it is clear that an improved fit to assimilated data is not the right way to validate inversion-estimated fluxes. What is done, however, is to test whether the optimized model does a better job simulating independent data (i.e. that were not used in the inversion). You could do this test as well, which would yield a more meaningful answer regarding inversion validation.

line 493-494: It is not clear how the seasonal variation can become larger if the state vector is time independent.

line 517: But in set I the CT2010 fluxes were used.

line 546: Whether or not the results can be considered significantly different obviously depends on the spatiotemporal scale over which fluxes are integrated. The scale should be specified more clearly.

line 547-549: This formulation is too vague and needs to be supported by actual numbers.

line 553-551: It is not clear why going from 1 month to 3 months is increasing the observational constraints. What is expressed on the y-axis is the annual flux error. Whether this is composed of 4 block of 3 monthly fluxes or 12 blocks of monthly fluxes doesn't make a difference regarding the number of data that are used. The only difference would be difference in the temporal degree of freedom of the fluxes, but this is not the way it is explained in the text. This comparison needs to be explained more clearly.

line 559: It is unclear why the transport error statistics would be so different for two regions that are not very different regarding transport

line 561-563: Another possible strategy to do what? Please explain more clearly.

line 572-573: But if the prior flux is the truth, then increasing the observational constraint is expected to increase the posterior flux error (an 'inversion' without any observations will yield the correct flux).

line 610-612: Statistically it is not expected that the true flux is always within the 2 sigma interval. If the actual error exceeds the posterior uncertainty this could simply mean that the prior flux uncertainty doesn't properly reflect the prior flux error, or that

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the model-data mismatch doesn't properly account for transport model error. Both of these options are likely, given that ad-hoc assumption on these uncertainties were made in the inversion set-up.

line 622-624: Flux error contributions occasionally cancelling each other out are not a sign of non-linearity. If the inversion is linear, as seems to be the case here, you would actually expect the error contributions to add up. If they don't, it raises the question why this happens.

line 630-631: Unrealistic results for some months and sub regions are expected when increasing the degrees of freedom beyond the point that can be resolved by the data.

line 635: A reference is needed here.

line 646: But systematic differences in simulated concentrations during nighttime are probably not just caused by horizontal resolution.

line 635: If the representation error is not a concern, does this mean that the 30% uncertainty that is assumed was too large?

line 685: Given the difficulty to separate the contribution of aggregation errors from other errors, how do you know that using CH4 as a prior causes the largest aggregation error? Where has this been shown?

line 698: What is meant with 'degree of spatial resolution'?

line 726: What do you mean by optimization procedure error?

line 776: What is the difference between equation 1 and A1? (same question for 2 and A2)

line 847-877: This is not right. The most commonly used inverse modelling methods define the state vector elements as random variables.

line 917-919: This is not right. The size of matrices in analytical inversions is limited by computer memory, but this happens for state vector sizes that are much larger than 'only a few parameters'.

line 925-927: In many cases the inverse problem is approximately linear, and the statistics not far from normal. In this case, you won't have multi modal distributions and the use of means, or medians together with an estimate of the width of the distribution is perfectly fine.

TECHNICAL CORRECTIONS

line 5: 'analysis' i.o 'analyses'

line 52: remove 'with known bounds'

line 129: 'are summarized'

line 188: The GMD formatting policy is to use bold roman for matrices.

line 315: 'using' i.o. 'used'

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