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Geoscientific Model Development Discussions

Interactive comment on "Regional CO_2 inversions with LUMIA, the Lund University Modular Inversion Algorithm, v1.0" *by* Guillaume Monteil and Marko Scholze

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We would like to thank the reviewers for their feedback. Based on their comments we propose to submit a revised version of the manuscript. Please also note that the paper was in review for six months, during that time there have been lots of developments on our side, and therefore we also have our own critical opinion on some aspects of the paper, which we think justifies a revision and of course would like to include in a revised version.

Below are our detailed answers to the reviewer's comments

1 Reviewer 1

We would like to thank the reviewer for their suggestions. The reviewer has made four main recommendations:

The reviewer asks us to to demonstrate that the regional inversiouns outperform global flux inversions

There is no simple answer to that question:

- At the continental scale, the global inversions might in fact perform better than the regional inversions. This is because there is no constraint on the CO2 concentration of the outgoing air mass in regional inversions (and therefore on the net regional flux), so flux adjustments by the regional inversions don't have to be consistent with CO2 observations outside Europe, in contrast to what happens in global inversions where there is by definition a constraint by the global atmospheric growth rate.
- The main benefit of regional inversions over global ones is the capacity to correctly assimilate observations from dense networks and/or more complex sites: the resolution of the global TM5 simulations used for producing our background concentrations is 6°×x4° (450 km at 45°N). This is absolutely not adapted to the density of the ICOS network in Western Europe, and it is also not adapted for assimilating data at sites that are nearby strong CO2 point sources such as urban centers. We expect the regional inversions to be more performant at such smaller sub-continental scales, but global inversions should remain more relevant at large scales.
- It would theoretically be possible to increase the resolution of the global model, but we would then encounter a performance limitation: a one-year LUMIA inversion with ICOS data typically takes 3 to 6 hours. A one-year TM5 inversion with

a 1° zoom over Europe takes up to 6 days.

We agree that the comparison between the performance of regional and global inversions is a very relevant and important topic to study, but it is complex topic and should be treated thoroughly in a dedicated study. Such a study would include a comparison between LUMIA and TM5 (but with TM5 ran at a 1° resolution over Europe because of the above mentioned limitation of a coarse spatial resolution, and initialized with the same prior as LUMIA). It should also include alternative formulations of the regional inverse problem, such as imposing a constraint on the net European C flux from the global carbon budget. Finally, it would require consequent amount of validation data.

We however agree with the reviewer that this performance comparison is an important topic and hence should be discussed more explicitly in the paper. We will reinforce that aspect in the discussion section of the revised manuscript.

The reviewer also suggested that we test alternative parameterizations of the prior uncertainty, to attempt fitting better the "true" net annual flux (when known).

In short: we agree with this suggestion, and we propose to include this in the revised paper. We can do it by scaling the uncertainty to the respiration and not to the NEE, which leads to less seasonal variations of the uncertainty. However, we must stress out that even with a more adequate representation of the uncertainties, there is no way to guarantee that this issue would not happen in an inversion using real observations (as it is impossible to guarantee that the uncertainty matrix is adequate). This issue is probably common to most inversions, which is why we think it is relevant to highlight it.

The reviewer would like to see a validation of the optimized fluxes and suggests doing it via a comparison against independent CO2 concentrations.

It is technically very feasible (and easy) if using only point observations (I.e. in-situ or flasks) for the validation, but we fear that this comparison would be mostly informative of

the local performance of the inversion and therefore would be very difficult to interpret. The possible exception would be airborne or satellite data, but this would also open many questions on the best way to represent these data in a coupled transport model. Another justification for not carrying out such a validation is that, the OSSEs provide a form of validation. At least they demonstrate that the inversion is functioning as expected, the cases where it does not lead to an improvement are explainable by an inadequate setup, and not by a malfunction of the inversion algorithm. The inversions using real observations are mainly here to give an idea of how the model behaves in a less ideal case (I.e. with transport model errors and with a (probably) more complex pattern of prior error) but we do draw conclusions on the optimized fluxes themselves.

The reviewer asks for computational cost of the components of the inversion.

The reviewer is entirely right, and we will add this to the revised version.

In summary, we propose for the revised version to improve the definition of the prior uncertainties in order to improve the estimation of the annual net budget in the OSSEs (and hopefully also in the inversions with real data). We also agree to extend the discussions of the two other points raised by the reviewer in the revised manuscript: 1) validation of the fluxes and 2) comparison with global inversions.

2 Reviewer 2

The reviewer strongly questions the interest of the paper, criticizing in particular the fact that we "are not willing to publish the full code", and that the results are not innovative or new. He/she wonders whether the focus if the paper is on the software or on the scientific results.

- *on the publication of the code*: It is wrong to say that we are not willing to publish it: the code was provided, in its entirety, as SI of the paper. However, we can understand that the reviewer would prefer to see the code in a public repository. One reason for not doing so at the initial submission was that we were in transition from python2 to python3, and therefore the specific branch of the code that was used to produce in this manuscript was not going to be maintained. We have now completed this transition and are willing to publish the code on an online git repository, as part of the revision.

- *On the general rationale of the paper*: LUMIA was developed as a system that would allow us to perform regional CO2 inversions, and to adjust independently various aspects of the inversion system (the transport model, the optimization algorithm, the formulation of the control vector and of its uncertainties, etc.). We did not have an in-house transport model and there were, at the time when we started developing LUMIA, and to our knowledge, no generic enough inversion tools publicly available to do this (the systems pointed at by the reviewer didn't exist or were only in early stage of their development). We therefore built from scratch a new inversion system, based on a new offline coupling between TM5 and FLEXPART, and a completely new python library handling the actual inversion. The main aim of the paper is therefore to describe and publish that inversion library, including the TM5-FLEXPART coupling, so that a) we can refer to it in future studies based on this system, and b) other people can use it also.
- We chose to first focus on developing a robust technical basis (computational efficiency, "cleanliness" and modularity of the code, portability, etc.) instead of trying to directly be innovative with the inversion technique. We therefore first implemented a rather simple and classical inversion approach. This facilitated the testing and the comparison with other similar inversion systems. Therefore don't think that the lack of scientific innovation of the inversion approach is a weakness at this stage.
- The aim of the sensitivity tests is to verify that the system behaves as expected,

and also to detect weaknesses of the current approach to help us identify the components where further developments are most needed. Furthermore, our inversion setup shares many similarities with several other regional inversion systems, the limitations that we identified are likely to apply to these systems as well. We therefore think that these sensitivity test results are relevant not just for us, but more general for the inversion community as a whole and therefore we think it is relevant to include them in the paper.

Besides these clarifications, we agree that several aspects can, and should be improved. As mentioned above, we can now publish the code on a public repository. Since the submission of the manuscript the code has been migrated to python3, is now better documented, and should be easier to setup and understand for new users. Some of the text of the paper is misleading/imprecise and the clarifications written above need to be integrated as part of the revision (mostly by improving the text of introduction and discussion).

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