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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical note: A high-resolution inverse modelling technique for estimating surface CO_2 fluxes based on the NIES-TM – FLEXPART coupled transport model and its adjoint" *by* Shamil Maksyutov et al.

Anonymous Referee #2

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The 'Technical note' by Maksyutov and co-authors describes the technical details of a CO2 flux inversion technique based on the coupled Eulerian-Lagrangian transport model NIES-TM-FLEXPART. The coupled system operates at a high spatial resolution of CO2 fluxes of 0.1° x 0.1° globally and also attempts a flux inversion at this resolution. As such the approach is novel and promising. The paper is well structured and written. The performance of the inversion is documented by time series comparisons/evaluations of different data sources assimilated and not assimilated by the system and a comparison to another independent inversion system. However, the inverted flux fields are never shown/discussed in the manuscript, which makes it rather difficult to judge if the inversion yielded reasonable results. Even though the manuscript is a 'technical note', it would be very beneficial to overcome this shortcoming (detailed suggestion below) before publication.

Major comments

Section 5: Evaluating the performance of an inverse modelling system is not straightforward. Restricting this evaluation to a comparison of model skill for the assimilated and additional independent concentration time series is not sufficient. By over-fitting the flux fields a very good agreement of the posterior concentrations with the observations may be achieved but the flux fields may contain unrealistic detail in order to achieve this. Given the large degree of freedom in the fluxes, as indicated by the fact that grid and time resolved fluxes are inverted from a relatively limited set of observations, there seems to be a high risk for the presented inversion setup to over-fit the solution. Since no flux fields are presented, it is impossible to judge this possibility. Therefore, I would encourage the authors to extend their discussion of results in section 5 to include a brief analysis of the obtained flux fields. I can see that the authors have planed this for a later publication and, hence, I don't think this needs to be very quantitative here, but the presented flux fields should document the validity of the inversion approach. A qualitative comparison with flux fields obtained from CarbonTracker (as done for the concentration time series) would also be beneficial.

Minor comments

P3,L18: Resolution of coupled Eulerian-Lagrangian models. I find it a bit misleading to say that the transport in these models is run at a resolution of (as in the cited publication) 1 km. Yes, technically the transport is not run at any fixed resolution in the Lagrangian sense, but the driving meteorology is still determining what scales of motion can be correctly resolved by the model. The Lagrangian model may still have some skill in the sub-resolved range, but basically it degenerates to a Gaussian plume at these scales with constant wind direction, speed and dispersion characteristics. This fact is not sufficiently highlighted throughout the manuscript. Another example is P4, where the transport system for the current study is introduced. The driving meteorology is 1.25°x1.25° this is certainly not sufficient for a detailed transport description in complex, mountainous terrain, but also not for coastal areas. Since sites from both environments are contained in the list of sites used for assimilation, this limitation should be discussed in more detail. Next to spatial resolution also temporal resolution is important for a transport description in the mentioned environments. Although in the following, the use of observations was restricted to certain times of the day, these observations still carry the transport history for a longer period and if temporal variability in the transport is not sufficiently described may lead to biases in the simulated concentrations as well.

P5,L13f: If I understand this correctly, there is no diurnal cycle of the biospheric CO2 flux considered in the model setup. How much is this simplification limiting the model performance? Not all sites used for the inversion are remote coastal sites but are surrounded by dense vegetation. How much does the constant diurnal flux and the restriction to afternoon observations introduce a bias in the flux inversion? In general, I have the feeling that the low temporal resolution of fluxes does not keep up with their high spatial resolution in the current setup.

P6,L8: How much do the posteriori fluxes actually depend on the chosen biospheric flux climatology? Given the large year-to-year variability in biospheric fluxes, is it sufficient to operate with a climatology of prior fluxes? Was this evaluated by choosing a different averaging interval or even individual year for the prior climatology.

Section 3.3: Are all biomass burning emissions considered to be released at the model surface or was any kind of vertical emission profile used? Again, this may be crucial when considering transport simulations at the mesoscale.

P7, L9 and P9, L21: According to the first text location only afternoon samples were

considered for the inversion. However, according to the second location daily average footprints of the Lagrangian model were applied. If that is really the case the footprints are not representative for the used observations, largely neglecting temporal variability once again!

P9, L25ff: The description of the forward model steps. From this description it is not clear to me how the concentration increments from the different models are added to avoid double counting of the fluxes (step 2.c). Shouldn't the Eulerian model use different fluxes than the Lagrangian (cropping those that are covered by the Lagrangian model)?

P10, L24: Initial conditions are used from an optimised run from the previous year. But then the question remains how the previous year was initialised. Was this done with a spin-up run?

Section 5.1: Why is RMSE used as the sole estimator of model performance? RMSE will decrease even if only the baseline fits better after optimisation. Most of the regional flux information, however, is stored in the peak concentrations, for which a more robust performance estimator could be a bias corrected RMSE or the coefficient of determination. The bias should be reported as well. Taylor skill score could be another performance parameter that would be more suited to focus more on the short term variability.

Sites: It would be useful to see the sites and the aircraft locations on a map. Would help to judge which areas are not well covered by assimilated observations in comparison to validation data. Such a plot should also contain the information of flask vs. continuous observations.

Technical comments

P3,L10: Start a new sentence after '... Kaminski et al. (2001). This is addressed by ..."

P12,L14: 'previous' instead of 'pervious'.

Figure 1, caption: Label as 'Examples of ...'

Figure 3+4: Since the x-axis is not along a continuous variable, I would suggest to not include lines in the plot or even use a barplot instead of symbols. The lines are just confusing and have no physical meaning.

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