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Interactive comment on "Joint CO_2 state and flux estimation with the 4D-Var system EURAD-IM" *by* Johannes Klimpt et al.

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This paper considers the joint assimilation of emissions of CO2 and the initial condition of CO2 concentration. It tests assimilations with and without the initial condition in a standard Observing System simulation Experiment case. It shows that, over the short assimilation window it considers, the inclusion of the initial condition improves the assimilation of fluxes, the usual target for such problems. It requires careful balancing of the prior uncertainty covariances for the unknowns. The paper describes a potentially important technical advance in an area of geophysical modelling and data analysis and so is certainly within scope for GMD.

That said I do not believe the manuscript is suitable for publication at the moment. I see two serious problems with it.

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Discussion paper

First, the experiment is not a good reflection of how we perform inversions for surface fluxes in practice. In particular it is unusual to use such short assimilation windows even for regional inversions. This is important since the results here show that the effect of the initial condition is limited to the start even of this assimilation window. If we used the more normal practice in the inversion community of running for a week, a month or a year, it is hard to see that the effect would be important. The cited paper from Peylin et al. (2005) already suggested this with an initial condition affecting the first few days. That study was for a much larger domain where we could expect the flushing time for the initial concentration to be much larger.

It is perhaps unfair to criticize authors for something their paper does not do but there is a related problem in regional inverse studies which is, I think, very important indeed. The impact of lateral boundaries, here highly simplified, becomes more and more important as the domain shrinks. I believe exposing this to an assimilation system will have a much larger impact than the initial condition.

There is also a related problem where I think the joint assimilation is important. This is the analysis of the atmospheric chemical state. Here the variable of interest is the concentration while the emissions are a nuisance variable. Such analyses aren't very interesting for CO2 but are of great importance for other species.

My second concern is the treatment of prior covariances. It is twofold. I think the authors have borrowed too much from the methodology of numerical weather prediction. In NWP, the ultimate test of a good system is a good forecast and it is permissible to do what is necessary to get it. In a more elevated sense we could assume that the statistics of the system are well-tuned if the analyzed state is good. The CO2 inverse case is not quite like that. We generally do not have some exterior metric we can test against. We are forced more to rely on analysis of our prior information in its own right. Thus questions of what the statistics of prior and observational covariances mean and how to verify them become crucial. Various papers from Chevallier and his group, for example, compare the prior flux in an inversion with independent pointwise data and

generate statistics accordingly.

Secondly, although I believe the covariance model employed is suitable for the initial condition, I do not understand its relevance for the emission covariance. Maybe I misread this but I do not see it as evolving according to some advective-diffusive process.

For these reasons I do not believe a paper along the lines of this manuscript will make a substantial contribution. I do believe that the approaches have value when applied to a range of related problems.

I have a series of more specific comments on the paper but I do not believe it is efficient for the authors to correct these until they can satisfy the editor that the overall paper should proceed.

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