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Geoscientific Model Development Discussions

Interactive comment on "A multi-species data assimilation system to retrieve information on land-atmosphere exchange processes" *by* Ivar R. van der Velde et al.

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This paper presents an extension to the CarbonTracker system to ingest measurements of the C13 isotopic signal in atmospheric CO2 samples. It demonstrates the simultaneous optimisation of photosynthetic fractionation and net CO2 exchange. This makes it a significant extension over previous uses of C13 in atmospheric inversions so it is both within scope for GMD and significance.

Like many of us who have battled with inversions using C13, the authors have made some choices about what can and cannot be inferred from this species given the associated nuisance variables of isotopic disequilibrium flux (isoflux). They pretty much

remove the possibility of C13 observations informing long-term mean fluxes by closing the C13 budget with a tuning of the isoflux. they are careful to keep this in mind and refrain from commenting on the long-term mean fluxes.

This is quite a useful contribution to the reemergence of C13 as a constraint, especially addressing the weakness of fixed fractionation of many previous studies. I have two concerns I would like the authors to address.

The first is a bit more detail on posterior uncertainties. This is more difficult in the NKF formalism of CarbonTracker than for the classical synthesis inversion but, especially in the nonlinear case, some sense of ensemble correlations among fractionations and fluxes would be useful. Perhaps these are the correlations already quoted, it seemed from the text these were signal correlations. As a side-note, the p-values attached to the correlations are not relevant here. We are interested in the strength of a relationship while the p-value shows the chance of giving such a correlation if the population value was zero.

My second concern is raised by the authors in the discussion but is not really dealt with. It could affect some of the conclusions. The authors note (P17) that impacts of changing net flux or fractionation on the isoflux are neglected. they correctly diagnose that the problem arises because the isoflux is not included in the optimisation. they suggest one solution, the partition of net flux into its gross components. There is another approximate solution. The main result of this process is a dilution of C13 signals by the isoflux. This can be parameterised as a response function for the C13 signal from a net flux. this was how Rayner et al., 1999 approached the problem, taking response functions from Trudinger et al. 1999. The time-scales for this response are long cf the assimilation window used in CarbonTracker so I'm not sure whether one can even capture the effect but we did find it had an impact on interannual variability. The problem may be less severe for the current paper because the prior signal for this response should be captured by SiBCASA. To quantify the effect I recommend that the authors take the difference between their prior and posterior flux and transport its C13

signature with and without the dilution response. This should at least give a sense of the significance of the problem.

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