

We would like to thank the referee for the review of this manuscript and their constructive comments. Our response to each comment is below with the referee's comments highlighted in italic typeface.

*Ziehn and Coauthors evaluate the ability of different atmospheric CO2 observation networks to constrain the CO2 fluxes of the Australian continent, by calculating the a-posteriori uncertainty achieved by the different candidate networks in a regional atmospheric transport inversion. For the design of observational infrastructure, this is an important tool. The study provides interesting insight for Australia, but may also be helpful for groups considering other parts of the world. Method and findings are presented in a clear fashion. Possible limitations are discussed. I clearly recommend this paper for publication.*

Response: We are grateful for the overall positive assessment.

*The only part I did not find convincing is the argumentation around Eq (14) (and the corresponding paragraph in Sect 4). While I fully agree to the conclusion that the boundary influence on the presented results is small, I do not see at all how that can be concluded from Eq (14). Rather, to my knowledge, the reason why the boundary influence on the a-posteriori uncertainties is small, is that the local fluxes are related to concentration gradients within the regional domain, such that the signals from the boundary largely cancels out. If the Authors decided to keep Eq (14) it would need substantially more explanation.*

Response: The uncertainty in the boundary can affect the concentrations measured at a certain point. These boundary effects can be included in our modelling approach in two different ways: (a) we can solve for them, or (b) we can treat them as contribution to noise. In the current study we decided for the latter case. If the contribution from the boundary uncertainty to the observed uncertainty is only minimal then we can safely ignore it. This is assessed with Eq. (14), which provides an estimate of the uncertainty contribution of the boundary concentrations.

We will revise section 2.3 including the corresponding paragraph in section 4 and clarify how we assess the contribution from the boundary concentrations on the observed concentrations by using Eq. (14).

*I'd further suggest to somewhat re-arrange sections 2 and 3, because both are on Methods. I suggest to either combine them into one section, or to rename section 3 into "Methodology: Network design for Australia". Further, I would move section 2.2 (plus the first paragraph of Sect 3.2) into an appendix as it is unexciting technical detail not specific to network design and not actually relevant to understanding the paper. Moreover, there are some repetitions that could be removed (e.g., part of page 7569 paragraphs 1 and 2).*

Response: We agree with the referee that section 2 and 3 can be combined in one section and we will do this in the revised version. However, we do not agree that section 2.2 is only technical detail not specific to network design. In fact, section 2.2 is a key part of the methodology that we introduce in this manuscript. It shows how the particle counts from any Lagrangian particle dispersions model

run in backward mode can be used to derive the source-receptor relationship for a point source. We believe that section 2.2 is compact and relevant enough to be part of the main paper. We will revise paragraphs 1 and 2 on page 7569 to remove any repetition.

*p7559 | 10: GLOBALVIEW is not a measurement program. Consider to replace "consists" by "summarizes data"*

Response: We agree with the referee and this will be corrected as suggested in a revised version.

*p7560 | 9: The word "cost function" (here and further down) is used for two separate items (Eqs (17)/(18) versus Eq (3)). It would be better to use different wording.*

Response: We agree that this might be confusing for the reader and we will highlight the difference in the two cost functions (inversion versus optimization) in a revised version of the manuscript.

*p7569 | 22: Is 4 weeks enough? How long does it take the air to travel across Australia?*

Response: We consider four weeks to be long enough. In general, the time period should be large enough to capture the range of relationships between the sources and the observations. In our case we assume that sources change every week (weekly fluxes). Diffusion is considered to be fast enough, so that the influence of a surface flux from a previous week has only a small influence on a current observation. We will also add this explanation to the manuscript.

*p7571 | 9: Mention whether or not the ocean fluxes are adjusted in the inversion. I actually think they should, because otherwise the a-posteriori uncertainties of the land fluxes will be unrealistic.*

Response: Ocean uncertainties are usually quoted as a factor of ten smaller per  $\text{m}^2$  than the land uncertainties (i.e. Chevallier (2007)). However, because of the size of the ocean they do matter in global inversion studies. In our case, the modelled domain contains only a small ocean part. Nevertheless, the contribution of the ocean fluxes to the posterior covariance matrix and the optimal location of stations is investigated for a South African test case in part 2 of this paper (Nickless et al. (2014)).

*Sect 3.3: You later only use Eq (18). I think a rationale needs to be given for this choice. How different would the results be when using Eq (17)?*

Response: The reason for using Eq(18) instead of Eq(17) is that we are interested in the uncertainty reduction of the total flux estimate instead of considering individual fluxes independently from one another. This will be clarified in a revised version of the manuscript. Both cost functions result in an optimal network and the difference of using one cost function over the other is investigated in a companion paper (Nickless et al., 2014).

*p 7572 | 14-17: Put to appendix as well.*

Response: We decided to remove those lines from the manuscript.

*p7573 | 27 .. p7574 | 4: Put to Methods.*

Response: The corresponding paragraph will be removed and merged into the methods section in a revised version of the manuscript.

*p7580 | 2: "estimates" probably means "uncertainties".*

Response: Yes. This will be corrected in a revised version of the manuscript.

*Typos:*

*p 7565 | 1: "overbar"*

*p7580 | 24: "modelled"*

Response: The spelling mistakes will be corrected in a revised version of the manuscript.

References:

Nickless, A., Ziehn, T., Rayner, P. J., Scholes, R. J., and Engelbrecht, F.: Greenhouse gas network design using backward Lagrangian particle dispersion modelling – Part 2: Sensitivity analyses and South African test case, *Atmos. Chem. Phys. Discuss.*, 14, 11301-11342, doi:10.5194/acpd-14-11301-2014, 2014.

Chevallier, F.: Impact of correlated observation errors on inverted CO<sub>2</sub> surface fluxes from OCO measurements, *Geophys. Res. Lett.*, 34, L24804, doi:10.1029/2007GL030463, 2007.