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Interactive comment on "Greenhouse gas network design using backward Lagrangian particle dispersion modelling – Part 2: Sensitivity analyses and South African test case" *by* A. Nickless et al.

Anonymous Referee #1

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This paper presents a study for the optimal design of a CO2 atmospheric network dedicated to CO2 flux inversion in South Africa. It is the second part of an analysis of the results from a network design tool (that is applied to Australia in the first part). This tool is based on an atmospheric inversion system using a Lagrangian model to simulate the atmospheric transport and on algorithms for the minimization of the uncertainty in the fluxes from the inversion as a function of the site locations. In addition to applying the system to a different region and with a different constraint regarding the number of sites to be installed (5 in this study, in agreement with the number of instruments that have actually been purchased), this study also complements the part I by exploring the sensitivity of the network optimization to various parameters of the inversion system and of the optimization algorithm.

The network design tool described and applied here is a very interesting and powerful tool and I would be very interested in a study exploiting its full potential. The actual need to locate 5 stations that have been purchased also provides a nice context for such a study. However, from my point of view, this part II should be rejected and potentially merged with the part I for several critical issues:

- First, the quality of the text in this manuscript is very low. There is hardly any paragraph (even in the abstract) that is not messy, disorganized, confusing, that does not contain mistakes (some are quite embarrassing), redundancies, abusive shortcuts or discussions that sound absurd. This is quite surprising since the part I (written by same co-authors) seems to be well written. Listing few examples of the sentences that are problematic would hide the fact that their number is actually discouraging, and prevent from reading this paper easily. Even the structure of the sections is sometimes problematic (see the section "prior covariance matrix", where the descriptions of the prior error covariance matrix and the observation error covariance matrix alternate 5 or 6 times; see also the very confusing spread of the pieces of information regarding the exact variables that are controlled by the inversion throughout the text).

- Contrary to what the authors say at lines 22-23 p11305, the major part of this paper is actually dedicated to detailing again the system such as in part I, with the duplication of nearly all the equations, and with attempts at rephrasing some of the explanations that turn into confusion and some useless lengthening.

- The additional content of part II compared to part I is relatively weak quantitatively and in terms of scientific relevance. The analysis of the results is relatively poor. I think that the two parts should have been merged 1) The application of the system in South Africa does not bring any particular scientific analysis regarding the set-up of the system, since, actually, the author do not really pay attention to the realism of this set-up. They

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wait for the conclusion to indicate that it would be good to use the network design tool with an inversion set-up that looks like the one that would be used when assimilating real data (see lines 28-29 p11327 and 1-2 p11328). They often say that their default set-up has many parameters that are less realistic than the one used for the sensitivity studies (see l13-15 p11318). This is even more problematic that this study is supposed to support the set-up of new stations with existing instruments. Network design studies have often yielded far too optimistic results regarding the capabilities of the inversion. But the papers detailing such studies have generally made their possible to justify the configuration and thus the results of the underlying inversion system. Here, on the opposite, the authors nearly claim that the spatial resolution of the inversion and the values for the prior error covariances should be tuned so that the system give a predefined station distribution that they would like to justify (see lines 1-4 p 11303 and l 22 to 26 page 11327) ! In inversion systems, computational costs prevent from working at very high resolution over the whole domain, which explains why there have been some attempts at optimizing the horizontal grid for the fluxes to be controlled as a function of the station locations. But if there was no computational or technical limitation, one should use a very high resolution over the whole domain in order to derive results as realistic as possible (bearing aggregation errors that are as small as possible). The authors ignore the aggregation error and do not modify the value of the observation error when changing the spatial resolution of the flux to be controlled. Consequently, the text does not give any confidence in the optimal location of the stations derived by the system. 2) The selection of the sensitivity tests and of the corresponding changes in the parameter values sound quite random according to the manuscript. Additionnally, the discussions regarding whether one should minimize the mean uncertainty in fluxes at pixel scale (called "a more detailed-focused solution" l10 p 11327) rather than the uncertainty for the mean fluxes which drive to a "sensitivity test" sound absurd. These discussions and the mixing of fossil fuel fluxes and natural fluxes in the corresponding "cost functions" highlight the absence of "physical" target for the network and for this study. Consequently, the analysis of the results is rather poor. The

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results of the sensitivity studies sound too specific to the experimental framework to draw general conclusions such as in lines 3-5 p11326.

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