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Discussions

Interactive comment on "Estimates of CO_2 fluxes over the City of Cape Town, South Africa, through Bayesian inverse modelling" *by* Alecia Nickless et al.

Anonymous Referee #1

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The manuscript "Estimates of CO2 fluxes over the City of Cape Town, South Africa, through Bayesian inverse modelling" presents an atmospheric observation and modeling system dedicated to the monitoring of the CO2 emissions from Cape Town. In particular, it presents series of CO2 measurements at two new stations in the vicinity of the city, a high resolution atmospheric inverse modeling system and results of emission inversions over more than 1 year. One the main conclusion of the manuscript is that the inversions hardly manages to distinguish between the biogenic fluxes from the ecosystems in the region and the city emissions.

I appreciate that the authors installed and maintained the two new measurement sta-

tions, made an inventory analysis and developed and applied a dedicated inverse modeling framework. To this date, the number of attempts at developing city scale inverse modeling systems is still small. The design of the measurement network in view to monitor Cape Town emissions seem highly relevant and promising. In such a context, I would like to support the publication of a good presentation and analysis of the authors' work.

However, as detailed below, I think that there are serious issues in this manuscript, which should undergo a major revision.

First, while the text could appear to be well structured at first sight, it is actually strongly hampered by a critical lack of rigor in the details. The pieces of text written too fast, the shortcuts, the theoretical mistakes and the approximations narrow the scope of this study to a community of experts well used to the theoretical material of this paper. They also often make the text really confusing and difficult to read in details. Redundancies and the detailed account of diagnostics that are sometimes uselessly complicated also participate to the difficulty to read this manuscript. I provide a list of examples below to demonstrate it. These examples represent only a small part of the issues I have met when reading this manuscript.

Second, I have concerns regarding some of the main results of this manuscript and regarding the lack of material that could help evaluate them.

A) Figures 9a,c and 10a,c indicate very large misfits between the model and the measurements when using the prior fluxes while, as highlighted by the manuscript itself, one does not expect a very large variability of the CO2 in the area. How is it possible to get such an amount of misfits larger than 30 ppm (and in the range 100 to 200 ppm) while the data nearly never reach an excess of 30 ppm over their baseline, and while the measurement stations seem to be quite distant from the city major point sources ? The manuscript ignores that such prior misfits strongly question the reliability of the atmospheric transport modeling framework, and thus of the inversion system.

The authors say that the model "shows ability to track local events at the sites" but it is impossible to assess on Figures 9 and 10. Furthermore, given the very large size of the control vector (and thus the very large number of degrees of freedom in the inversion system), it is not really surprising to see that the inversion manages to fit the data to a far better extent. I find it difficult to take it as a demonstration that the atmospheric transport model is reliable. In particular, opposed to what is said on page 1 of the supplementary material ("some evidence to provide confidence in the modelled meteorology is provided in this section"), the figures 1 and 2 of this supplementary material strongly question this reliability, displaying very large misfits between the modeled and measured timeseries of wind speed, with a weak correlation between them. The difference between the height of the measurement sites and the model vertical representativeness can hardly explain such misfits and even less such a low correlation. This requires a deeper analysis or better insights regarding the skill of the 1 km resolution CCAM model at city scale from studies like Engelbrecht et al. (2009, 2011).

By gathering the night-time and daytime data both in their analysis and in the inversion system, the authors do not help investigating this issue. We can assume that the largest misfits are obtained at night. However, opposed to what is said on page 1 line 12 ("Night-time observations were included, but allocated much larger errors compared to the daytime observations"), figure 5 indicates that the increase of the observation error at night is likely far from sufficient to cope with the increase of model errors at night. I think that the authors have overestimated their ability to assimilate night-time data. Analysis of the misfits between the modeled and measured CO2 and of the corrections to the fluxes applied by the inversion at night vs day could help investigate this topic.

B) One of the main discussion of the manuscript is related to the lack of distinction between the anthropogenic and natural fluxes in the inversion results. What puzzles me is that the analysis of the posterior error covariance matrices should be a very helpful

tool to feed such a discussion. The authors display correlations between uncertainties in the emissions at a given pixel and the NEE field in Figure 16 but ignore them when conducting this discussion.

Actually, Figure 16 shows correlations that are very close to 0, which undermines the assumption of the lack of distinction. When looking at the station locations and at the situation of the city vs. the areas of high NEE (which are also the areas of high prior uncertainties in the NEE and of high corrections to the NEE in the inversion), it is difficult to understand why the separation between the NEE and the anthropogenic emissions should be so problematic for the inversion. The authors have used very large prior uncertainties in the NEE, and the NEE dominates the mean diurnal cycles of the stations. This explains why, on the first order, the inversion focuses on the NEE rather than on the anthropogenic emissions (from that point of view, it would not be a problem of distinction between NEE and anthropogenic emissions, but rather a problem of detection of the emissions despite the dominating signal from NEE). But, according to figures 3 and 4, this should not prevent the inversion from getting a signal that is dominated by the anthropogenic emissions when the wind blows roughly from one station to the other one through Cape town. Paradoxically, the type of "gradient approach" that the author assume to be useless for their study case (p13 line18 p61lines7-9) may help them to cope with the NEE signal. All of this needs to be better analyzed. The analysis of the variations of the modeled contribution from the NEE vs that from Cape Town at the measurement sites could be very useful.

C) Regarding the prior uncertainties in the NEE, the relative values discussed in section 2.9.2 can be very large. They deserve some justification based on the CABLE validation studies, especially since they will be amplified by the multiplication of the prior uncertainties by a factor of 2 in section 2.11 (Figure 4 being misleading). I understand that when aggregating them over the modeling domain, we get much smaller relative values due to using a small spatial correlation length scale. However, could not it be an issue for the results at the pixel scale and, implicitly, for the control of the highly localized anthropogenic emissions (see the strange correction patterns in figure 13 and 14) ?

D) I am not sure to understand the distribution of the emissions from Cape Town according to the author's inventory. p11 says "But of the carbon emissions due to energy usage, only 27% were attributed to the transport sector as a result of the carbon intensive usage of coal for electricity generation to provide almost all of the energy to the residential and commercial sectors in South Africa, which emit approximately 29% and 28%, respectively, of the total carbon emissions of CT (City of Cape Town, 2011)." Paying much attention to the terms "electricity generation" and "almost all of the energy", my understanding of this sentence is that there is a large number of coal power plants within the city bounds (otherwise the part of the emissions within the city due to the transport would be very high), which represent almost 60% of the city CO2 emissions, while the direct emissions from the residential and commercial areas should be very low. However, this seems strongly at odd with the figures and discussions of this manuscript and this would be highly problematic for the atmospheric inversion. Could the authors clarify this point ?

It would be difficult and useless to discuss the secondary scientific issues at this stage. The authors should first improve the presentation of this study. The following illustrations of the lack of rigor and clarity of the text are mainly picked up from the abstract and sections 1 and 2. Actually, the text of these following sections is often more problematic than that of these first ones. Since I could have listed a far larger amount of issues, the author should not limit themselves to correct for the ones given below. They should rather consider a full and deep rewriting of the manuscript.

- Section 2.1 makes a rough account of the traditional theoretical framework of the inversions (e.g. sentences like "If we assume a Gaussian error distribution for the surface fluxes and concentrations we obtain the following cost function for our least squares problem" on page 6). Throughout the manuscript, the covariances between uncertainties in fluxes are often called covariances between fluxes.

- in the abstract and first sections, the text introduces the concept of "boundary concentrations" without specifying that the boundaries relate to the modeling framework. This becomes problematic when explicitly speaking about the sensitivities of the measurements to the boundary concentrations (e.g. l23-24 p5)

- the text often uses the terms "sources" and "emissions" while speaking about (natural) fluxes that can be negative

- p10 line17-19 the sensitivity is not the influence, H is not HTtranspose

- p11 "and allowed for small scale transport features to be maintained in H": using a coarse resolution control vector does not remove the small scale transport features in H

- Section 2.11 states that the error covariance matrix that is underestimated in the first configuration according to the chi test is necessarily B ("and values greater than one indicate that the variance prescribed is lower than it should be and therefore the posterior estimates will be over-constrained by the prior fluxes") while it could be R (and actually some of the results favor the assumption that it is R).

- sample of awkward, meaningless or confusing sentences:

p1: "The inversion solved for the actual concentration measurements at each site, which was made possible by the use of the Cape Point background site to provide information on the boundaries, and was necessary due to the effect of topography on the atmospheric transport, affecting particularly the sensitivity of the Robben Island site to the surface fluxes."

p1: "The mean bias in the modelled concentrations was reduced from -2.9 ppm, with interquartile range -9.1 to 3.7. . ."

p2 "The inversion was also allowed to solve for each of the four boundary concentrations (north, east, south and west), but these were provided with tight constraints provided by the background site."

p2: "Model assessment by means of the chi2 statistic indicated that the mean statistic was 1.48 over all months"

p2: "prior values for the model errors or the uncertainty in the fluxes"

p2: "By mitigating the CO_2 impact of cities, cities play a pivotal role in decreasing their own climate vulnerability."

p3: "Originally implemented to determine global, large scale sources and sinks of CO_2, regional or mesoscale scale atmospheric inversions are becoming more common."

p3: "All emissions are observed as an aggregated total, therefore all emission sources are accounted for, but it is challenging to separate out these CO_2 emissions into different components of the total CO_2 budget without additional measurements or confidence about spatial and temporal patterns of emission"

p3: "At the moment background conditions are not sufficiently characterised in order to use isotope tracers to differentiate between fossil fuel and biogenic sources, as these measurements are far rarer than atmospheric measurements of CO_2 mole fractions" (all the last paragraph of page 3 follows a very loose reasoning which is a bit difficult to follow)

p4: "This analysis underpins the assumptions of human behaviour driving the anthropogenic emissions"

p4: "Previous studies on estimating CO_2 emission for cities have found that errors in atmospheric transport modelling are a significant contributor to the overall uncertainty of emission estimates and therefore more work is required to refine these models so that they can perform more reliably during these periods of high uncertainty before they can be used to infer emission estimates at all times of the day."

p4: "To be able to verify emissions from underlying processes, higher resolution inverse modelling systems are needed to better understand and quantify emissions from different sectors."

p4: "This model at a slightly lower resolution was previously used for a regional network design study, making use of he same Bayesian inverse methodology and has been verified over southern Africa at relatively low resolutions through to ultra high resolution (1 kmâĞě1 km)"

p5 "A linear relationship can be used to describe the relationship"

p5 "The vector of the modelled concentrations cmod is a result of the contribution from the sources s, described by the transport or sensitivity matrix H ."

p11 "In the case of the boundary sources which are given as concentrations, their contributions to the concentration at the measurement site are expressed as a proportion of their concentration, dependent on their influence at the receptor site."

- Redundancies: p10: lines 6, 10-11 and 13 p10 lines 29-30 with all other parts of the text explaining it before and with p11 line1-2 + many details of the analysis in section 3

- The design of the figures should be improved. The location and name of the sites are hardly visible in figure 1. Labels are too small and the fields are fuzzy (mainly due to the choice of the colorbars) in figures like Figure 3. The choice of the colors in figures like Figure 8 is poor: on my screen, it is really hard to distinguish between the different curves. In figures like Figure 9, it is impossible to analyze the different timeseries since they are compressed along the x axis (with nighttime and daytime data mixed together) and most of the measurements are hidden behind the model patches. In most of the figures, there is a lack of subtitle and legends to help the reader while the captions are sometimes quite complex (e.g. the legend of figure 15). Therefore, in general, the figures are very difficult to read. In Figure 15: it is difficult to see the pixel against which covariances are computed.

- The notations used in several equations are not really optimal. At least, they do not help understand the meaning of the variables, e.g. Etrans in equation 11 which refers to a subcomponent of the transport error, while Eobs refers to another part of the transport error, and not to the observation error (which is the sum of the transport and measurement errors). Eq 8 is not really adapted to equations 1 and 6. Eq 9 and 11 are informal.

- There are too many significant digits in tables 3 and 4 which makes these tables difficult to read. Would not it be better to show the content of these tables using plots ? I do not understand why the authors produce distinct sections (3.2.3 and 3.2.4) and tables (3 and 4) for the variations of the 1-week mean and 1-month mean flux budgets. This is a source of redundancies and I don not think that they manage to bring specific insights for each of the two timescales.

- The acronyms CT and CBD are not defined explicitely.

- Section 2.2. and Equation (6) are confusing regarding the composition of the control vector (regarding the fact that the inversion solves for the fluxes at the transport model spatial resolution and regarding the control of the average conditions for each of the 4 lateral boundaries). We need to guess it from the numerical derivation of the size of the control vector or wait for sections 2.5 to get clearer details. The situation is similar regarding the fact that 1-month inversions are conducted to cover the 13 month period.

- there is a problem with the order of the citations (see Tarantola (2005) and Enting (2002) and Lauvaux et al., 2016; Bréon et al., 2015 on page 5)

- the percentile filtering technique at Cape point and its impact on the timeseries at this site is not well detailed (e.g. on which time windows, at which timescales is it applied ?), while the station can be influenced by Cape Town, and by the NEE in the region covered by the modeling domain. This is perturbing since the system controls the North and East boundary conditions that are inland (and thus separated from the Cape point station by large areas of NEE and potentially influenced by even larger areas of NEE outside the modeling domain) and since it uses the data filtered at Cape point to provide a prior value with a low prior uncertainty to these conditions.

- p20: the discussion on the representation error ignores the part of this error due to the difference of spatial representativeness between the measurements and the model ("We did not account any further for aggregation or representation errors as we did in the network design, as we were running the inversion at the same spatial scale as the transport model.")

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