Response to interactive comment by Anonymous Referee 1 on "Estimates of CO2 fluxes over the City of Cape Town, South Africa, through Bayesian inverse modelling" by Alecia Nickless et al.

Alecia Nickless, Peter J. Rayner, Francois Engelbrecht, Ernst-Günther Brunke, Birgit Erni, and Robert J. Scholes

We would like to thank the referee for their time and thorough consideration of the paper. We would like to thank the referee for their support of this work. With regards to the issues raised concerning the flow of the manuscript and redundancies highlighted by the referee, we have taken these comments on board, and performed a thorough rewriting of the manuscript which presents a more cohesive and focused presentation of the results of this reference inversion. The main purpose of this paper is to introduce the inversion framework used for the Cape Town inversion and to present the initial results for the reference inversion. The manuscript was rewritten to reinforce this focus. The sensitivity analyses will be presented in a related paper, and will rely on this introduction of the methodology. This paper focuses on the specification of the covariance matrices. Originally we had intended to include this work together with the reference inversion, but it soon became apparent that it would be too much content. Some of the discontinuities in the original manuscript are likely due to this change in focus of the paper. This was corrected in the revised manuscript.

As the manuscript has largely been rewritten to improve clarity, we will not focus on individual sentences identified by the referee, as many of these will be entirely changed or dropped from the manuscript.

Firstly we will address the main scientific concerns (labelled A to D by the referee).

The referee states *"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."

The issues raised here are concerning the poor match between the prior modelled concentrations and the observed concentrations, as well as the results of the attempt at validating the wind information from CCAM in the supplementary material. Lastly the justification of the allocated uncertainties for the night-time observations is questioned.

The referee is concerned with the reliability of the atmospheric transport modelling framework. In retrospect, presenting the adhoc validation of the CCAM wind fields was unwise. Unfortunately the wind measurements we have available to us were not adequate to compare to the modelled winds from CCAM. The locations of the weather stations were far from the sites, except for Cape Point. All the locations for the weather station sites would not have been representative of the 1 km by 1 km pixel from which the modelled winds would have been extracted, or were situated in locations that would have been strongly influenced by the local topography or built environment. The anemometer located at the top of the Cape Point measurement tower is subject to winds influenced by the surrounding topography (Figure 1). As stated in Whittlestone et al. (2009), the single point measurements at Cape Point were strongly influenced by local topography and measured wind directions showed little correlation with the true source of the air mass: "Two superficially attractive selection criteria proved to be ineffective. One, wind direction, was so perturbed by local topography that there was no correlation of the measured wind direction with the bearing of the source of more distant trace gases from the critical north to east sector. The other, back trajectories, were effective in determining if contact with the southern African continent had occurred, but the indicated time and location of land contact was highly inaccurate". We make use of the Cape Point site for background concentration information only, and the inversion does not rely on correctly modelling transport to this particular site. Our two sites are slightly less extreme in the surrounding topography, but nonetheless, still pose significant challenges for obtaining wind speed and direction measurements that would be representative of a grid square produced by a regional climate model. All the weather station sites are located near the shoreline, therefore subject to sea-breeze variations.

Figure 1. Image of topography around Cape Point GAW station. http://www.imk-ifu.kit.edu/319.php

To justify the use of CCAM to provide modelled winds, we rely on previous studies which have used this model for atmospheric transport modelling in our target area (Whittlestone et al., 2009), and studies which have validated CCAM at different spatial resolutions (Engelbrecht et al., 2009; Roux , 2009; Engelbrecht et al., 2011, 2013, 2015). In particular, CCAM has been able to satisfactorily recreate present-day rainfall totals and the rainfall seasonal cycle, as well as circulation patterns over South Africa (Engelbrecht et al., 2009), and has been able to simulate with some success mid-tropospheric closed-lows and extreme rainfall events (Engelbrecht et al., 2015). CCAM has been validated over the Stellenbosch wine producing area, which falls within the domain of this inversion, with respect to temperature, relative humidity and wind speed at six different stations within this region (Roux , 2009). Validating the wind product from CCAM in a rigorous manner is beyond the scope of this paper. Our interest is in the impact of the estimates of the uncertainties on the inversion results, and we have focussed our sensitivity analyses on this question. The discussion in the manuscript reviewing CCAM's capabilities has been expanded to give a more thorough examination of this literature.

Whittlestone et al. (2009) used CCAM to generate the wind data as well as to perform the transport modelling over Cape Point for their investigation. We instead relied only on the wind and other climate variables from CCAM, such as temperature which is well validated (Engelbrecht et al., 2009), and used the well known Lagrangian Particle Dispersion Model (LPDM) (Uliasz, 1994; Lauvaux et al., 2009; Lauvaux et al., 2016) for the transport modelling. This model has been used for several inversion exercises at various spatial scales.

The more likely candidate for the poor agreement between the observed and prior modelled concentrations is the specification of the prior fossil fuel and net ecosystem exchange (NEE) fluxes. The information available to perform a spatially and temporally disaggregated fossil fuel emission inventory for the City of Cape Town is limited. In addition, Cape Town is a city with stark inequalities between the population subgroups. For cities in developed countries it can generally be assumed that almost all heating and lighting will be generated through electricity consumption, but for many of the communities in Cape Town, this is not the case. These communities will rely heavily on raw fossil fuel burning for heating and lighting. Accessing this information and converting this into emissions based on the assumed behaviour of people is not factored into standard inventory analyses and is beyond our scope. Therefore, the residential emissions are a large contributor to the fossil fuel emission budget as well as one of the largest contributors to the uncertainties in the fossil fuel flux. The relevant question is whether we have successfully captured these uncertainties in specification of the prior uncertainty.

Another significant uncertainty is the simulation of the Fynbos biome. This biome is biodiverse, with many endemic species, but covers a relatively small area in South Africa, but a significant area within the domain of the inversion. The fynbos biome is poorly represented by dynamic vegetation models. The land atmosphere exchange model CABLE was selected to couple with CCAM due to its use and development in regions of Australia which share similar characteristics to the savanna biome in South Africa, which has a coverage of over 50%. Its ability to simulate respiration and photosynthesis in the fynbos region is largely untested.

We can test whether our assumed uncertainties are consistent with the prior misfit in concentration (see Michalak et al. (2005) for details). The magnitude of the discrepancies that we obtained between the observed concentrations and the modelled concentrations are expected. If we calculate the matrix $\mathbf{H}\mathbf{C}_{\mathbf{s_0}}\mathbf{H}^T + \mathbf{C}_\mathbf{c}$, and assume $\mathbf{H}$ is correct, the uncertainty in prior fluxes is projected into uncertainty in modelled concentrations. The square root of the diagonal elements have a similar distribution to the absolute mismatches between the observed and prior modelled concentrations.

To confirm that the prior information is the major cause of the disagreement (although not having perfect transport modelling is to blame as well), we performed a sensitivity analysis where the NEE estimates were averaged over the domain, and the prior estimates for NEE set as the overall average for all pixels. Similarly, the uncertainty of the NEE estimates was set as the overall average of the net primary productivity (NPP) estimates. At the Hangklip site, which is dominated by the NEE contributions, this led to far smaller discrepancies between modelled and observed concentrations (compare Figures 2 to 3). Therefore it appears that CABLE may be overestimating the amount of photosynthesis or respiration (or both) that are taking place in the region. On the other hand, Robben Island, which is far more influenced by the Cape Town fossil fuel emissions, did not show any improvement in the discrepancies between the prior and observed concentrations (compare Figures 4 to 5). In the sensitivity analysis paper, we present further tests based on the specification of the fossil fuel emissions and NEE fluxes and their uncertainties.

The plots of the residuals between the modelled and observed concentrations are not very different between day and night (see the Figures 2 and 4). Therefore it does not appear that the difficulty in modelling transport at night is leading to significantly larger discrepancies between the observed and modelled concentrations, as suggested by the referee. Specifying uncertainties that are too small for the night time model errors does not appear to be leading to the large discrepancies we are observing. The time series plots have been given for only a short period in order to avoid squashing the time axis, and to better demonstrate the inversions ability to obtain posterior modelled concentrations that are much closer to the observations. Although this is entirely expected, we wanted to demonstrate that as far as optimising the flux estimates to better match the observed concentrations, the inversion was at least achieving this. The expanded time series plots better show the ability of the inversion to track local "pollution" events.

The revised manuscript now includes more discussion on the discrepancy between the observed and prior modelled concentrations. To deal with the problems related to viewing the observed and modelled concentration data in Figures 9a,c and 10a,c, the time series plots have been altered. They are expanded over several panels in order to stretch out the time axis, allowing the reader to better assess the discrepancies and similarities between the observed and modelled concentrations, as shown in Figures 2 to 5. The diurnal plots in the supplementary material will also be altered to show additional information about the modelled concentrations, rather than only summaries of the observed concentrations at the sites. The purpose of this is to demonstrate the average differences between observed and modelled day and night-time concentrations.

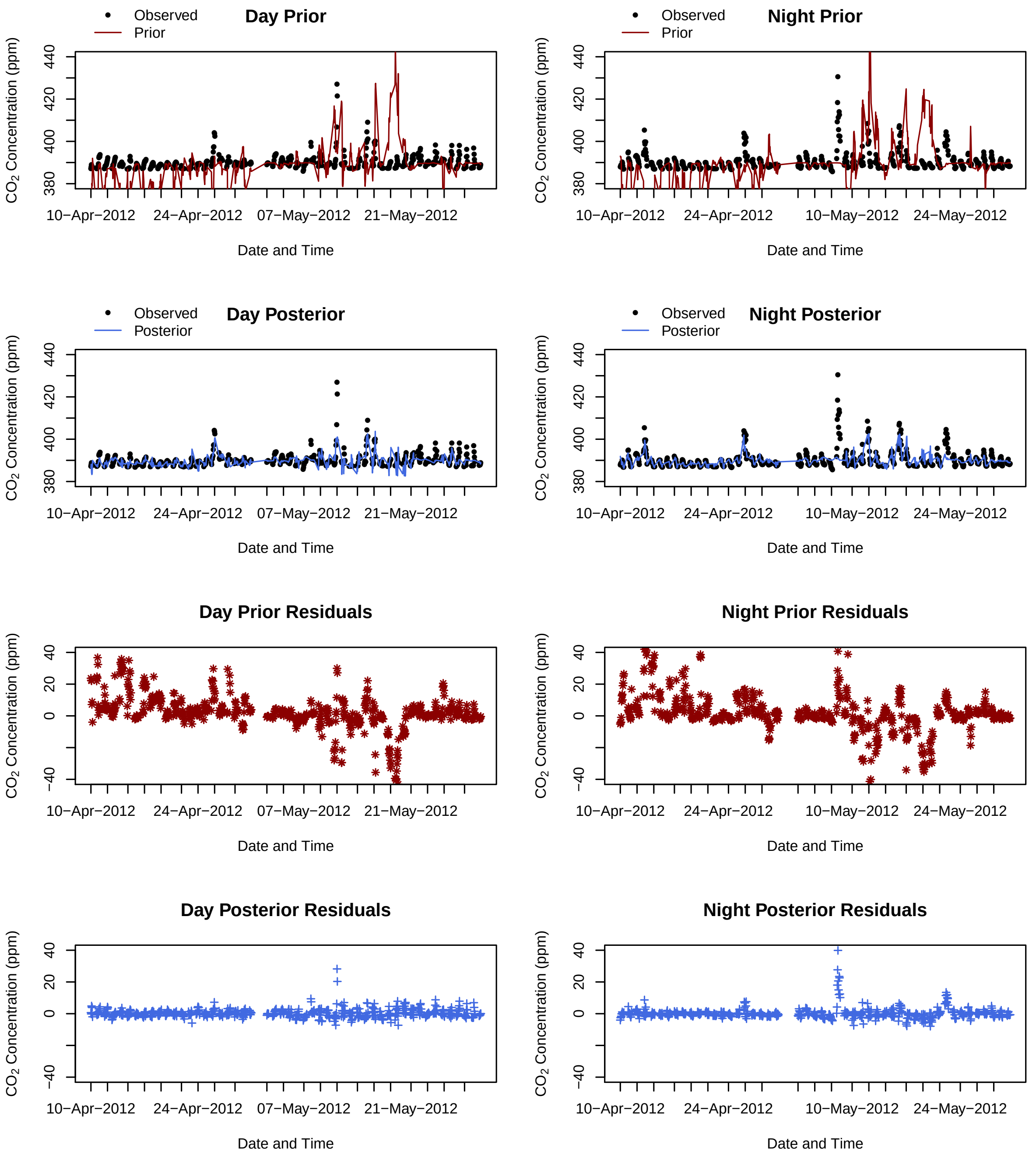


Figure 2. A time series of the observed and modelled CO_2 concentrations for April and May 2012 at Hangklip separated by day and night

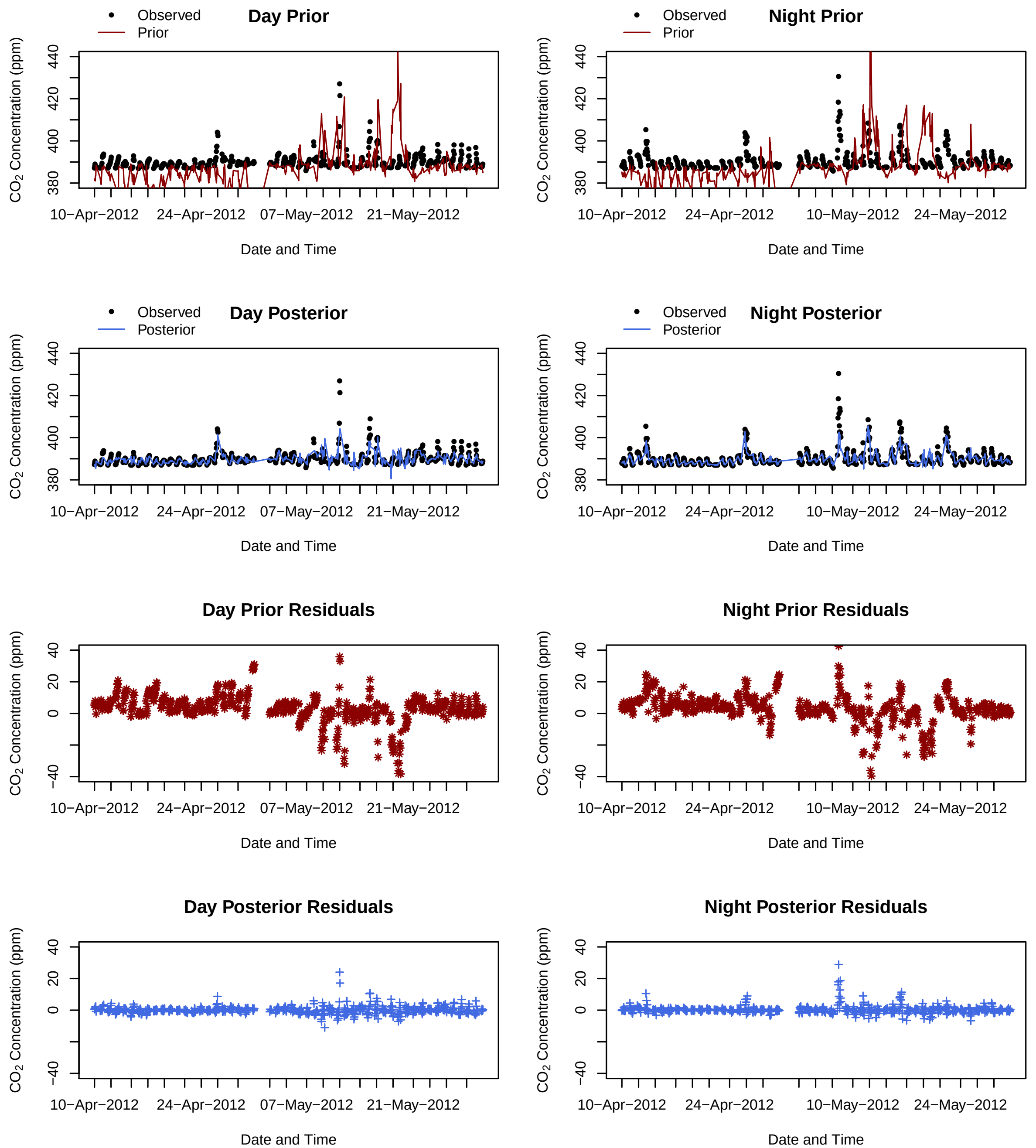


Figure 3. A time series of the observed and modelled CO_2 concentrations for April and May 2012, where the prior estimates of NEE have been set as the average NEE over all pixels, at Hangklip separated by day and night

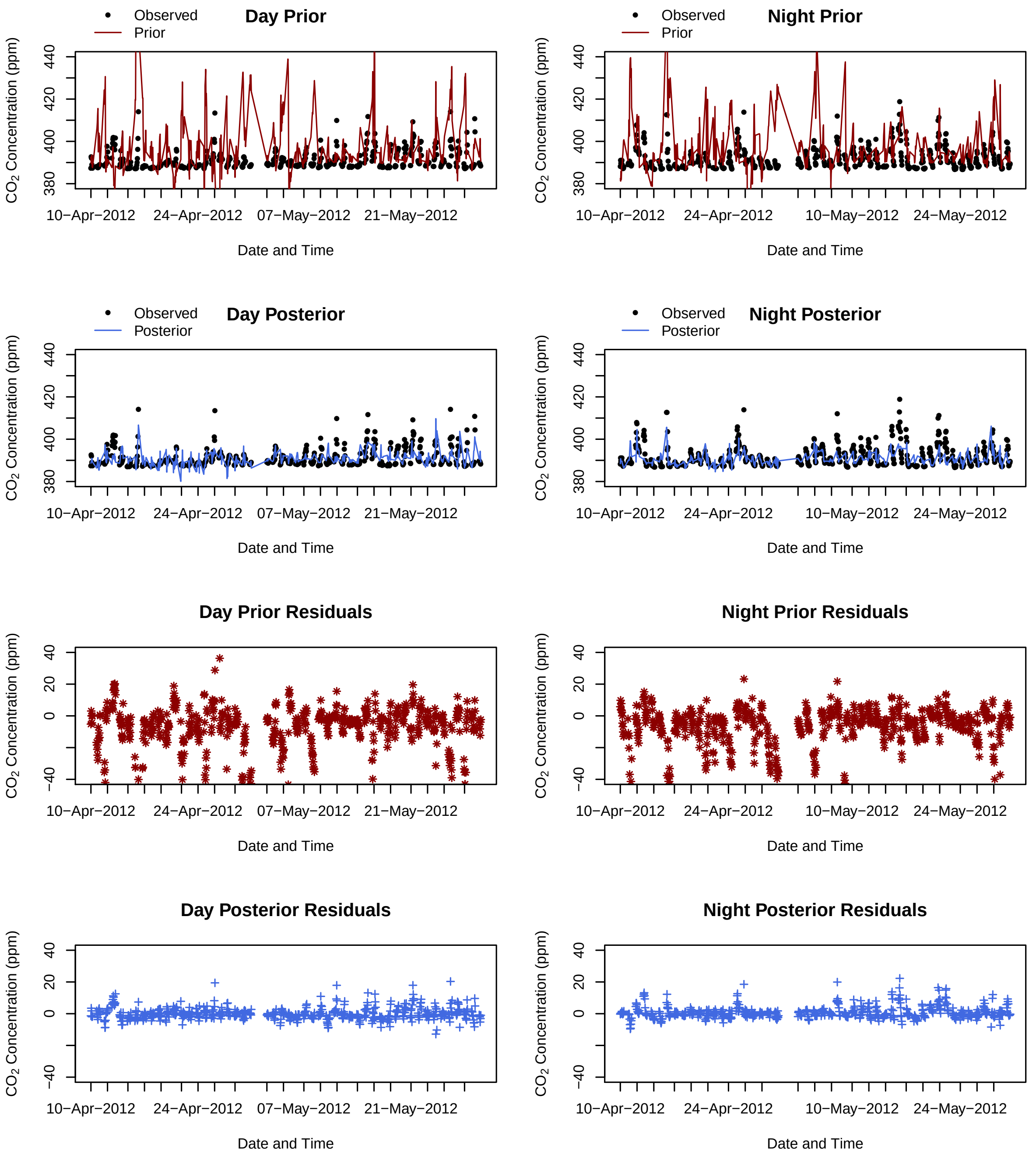


Figure 4. A time series of the observed and modelled CO_2 concentrations for April and May 2012 at Robben Island separated by day and night

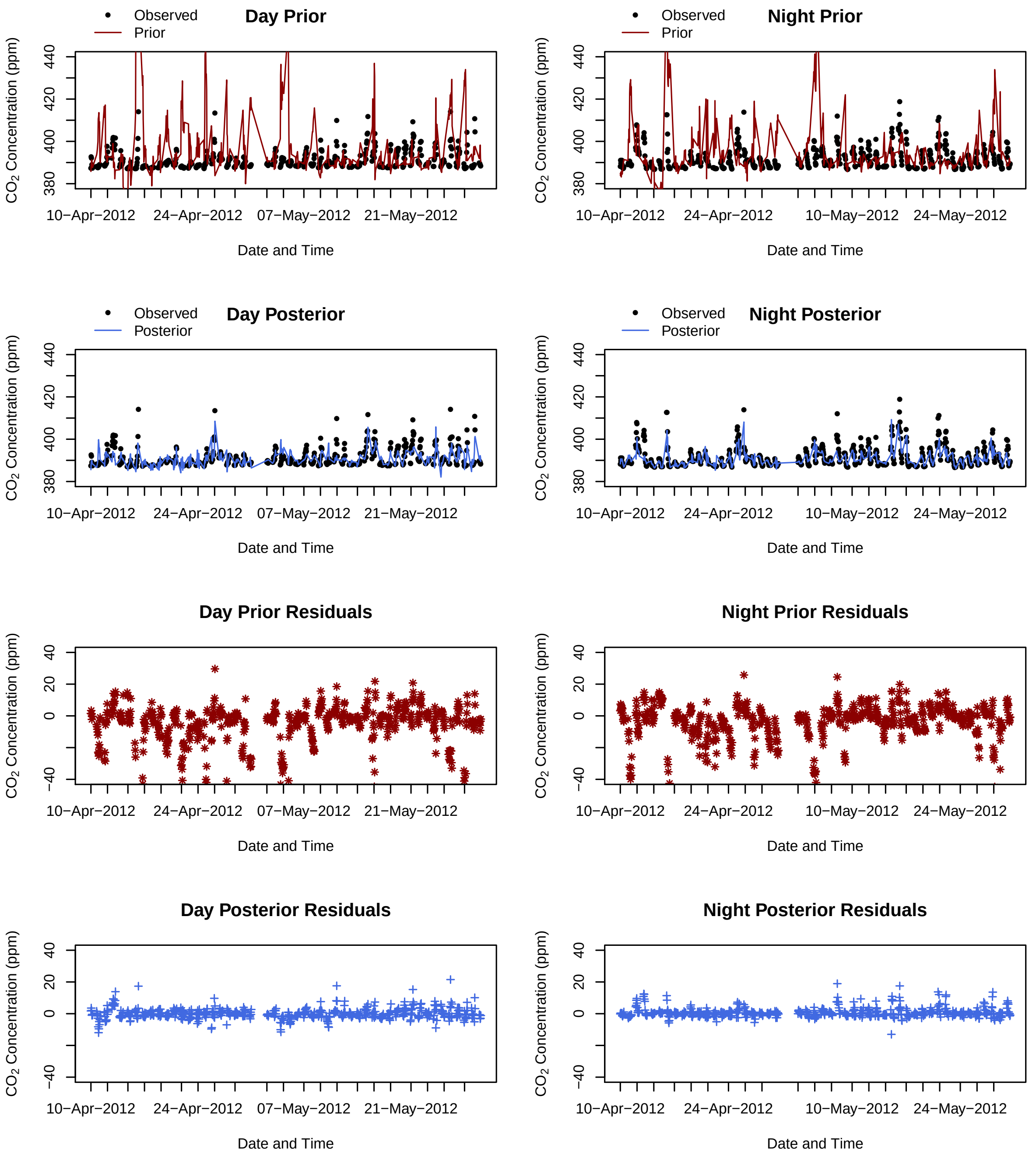


Figure 5. A time series of the observed and modelled CO_2 concentrations for April and May 2012, where the prior estimates of NEE have been set as the average NEE over all pixels, at Robben Island separated by day and night

The second major issue that the referee raised was:

"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 p61 lines7-9) 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."

The referee is concerned about the conclusions regarding the difficulty of separating NEE and fossil fuel fluxes correctly in the inversion. This is an important concern although we do not agree with the metric the referee chooses. We believe that the correct metric is the uncertainty of the difference between the two flux components, not the correlation. To see this, imagine the case where two flux components from the same pixel are given independent priors (no prior correlation) and are not all visible to the observation network. Nothing will have been learned about the fluxes, including their difference, but the posterior correlation will be zero. Unfortunately, this is close to our present case for many pixels. There is not enough information available to the inversion for it to correctly assign contributions from NEE and fossil fuel fluxes. We solve for these contributions separately in the inversion so the flexibility exists for adjustments to be made to both of these fluxes while attempting to obtain sensible adjustments to the overall flux within the pixel. As the referee also mentions, this is made more difficult by the large uncertainty that we have assigned to the NEE estimates (more on this in the third issue raised).

The covariance between fossil fuel and NEE flux uncertainties are small because the uncertainties in the prior modelled concentrations that are attributed to the flux contributions ($\mathbf{H}\mathbf{C}_{\mathbf{s_0}}\mathbf{H}^T$) are small relative to the uncertainties specified for the modelled concentration errors ($\mathbf{C_c}$). This is not because our prior uncertainty is small but because the transport Jacobian only projects fluxes from individual pixels weakly into modelled concentrations. As the uncertainty in the modelled concentration errors is decreased, the size of the posterior off-diagonal covariance elements between the fossil fuel and NEE flux uncertainties from the same pixel increases. This can easily be confirmed through the use of a toy inversion system using typical values for $\mathbf{H}$, $\mathbf{C}_{\mathbf{s_0}}$ and $\mathbf{C_c}$ from our inversion system. The posterior variance of any linear combination of terms from the source vector of the fluxes (including the difference between two fluxes from the same pixel) will always be reduced or (at worst) left unchanged relative to the prior variance of the same linear combination of elements (Jackson , 1979; Jackson and Matsu'ura , 1985). Observations may very well introduce correlations between flux components but this does not mean a reduction in the ability to distinguish between them.

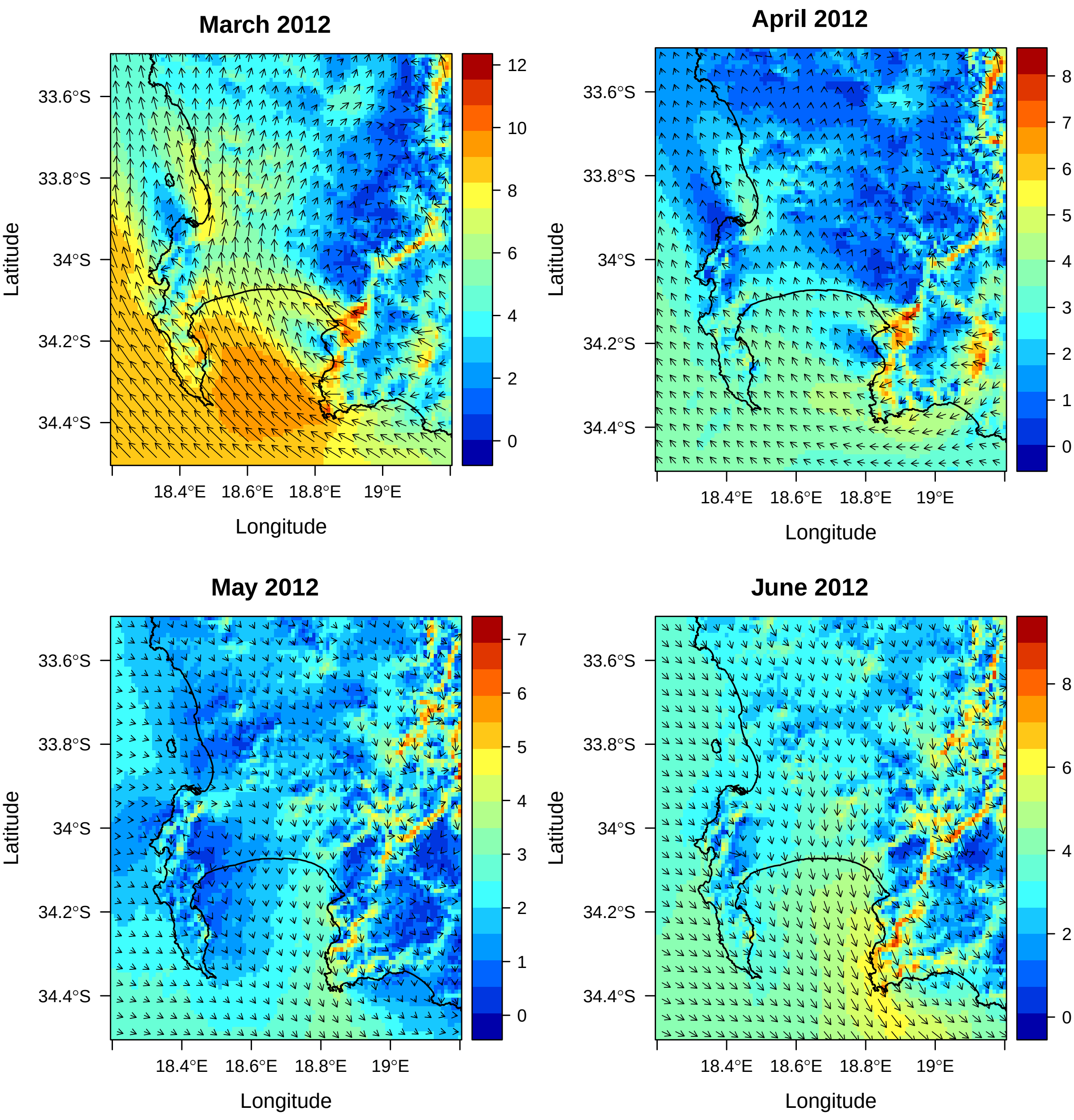
If we define the distinction between the fossil fuel flux and NEE flux within the same pixel *i* as the variance of the difference between the fossil fuel and NEE fluxes $s_{f,i} - s_{NEE,i}$, this will be equal to the sum of the variances of these two fluxes minus twice the covariance between them: $C_{s(f,i;f,i)} + C_{s(NEE,i;NEE,i)} - 2 \times C_{s(f,i;NEE,i)}$ where $C_{s(f,i;NEE,i)}$ will be negative. Therefore the posterior uncertainty of the difference in these fluxes is always going to be larger than the sum of the individual posterior flux uncertainties, but smaller than the prior uncertainty of this linear combination of terms. Therefore, although the off-diagonal terms may be small due to large prior variances, the ability for the inversion under the current framework to detect between NEE and fossil fuel fluxes is limited as the posterior uncertainties are still large, and therefore the uncertainty of $s_{f,i} - s_{NEE,i}$ is large. If the covariance terms are small because, relative to the errors in the modelled concentrations, the contribution of the uncertainty in the fluxes to the uncertainty in the modelled concentration is small, then the variance of $s_{f,i} - s_{NEE,i}$ is still going to be large due to the dominance of uncertainty in $s_{NEE,i}$.

On the other hand, when we aggregate these fluxes from the same pixel to get the total flux within a pixel $s_{f,i} + s_{NEE,i}$, the uncertainty of this term is equal to $C_{s(f,i;f,i)} + C_{s(NEE,i;NEE,i)} + 2 \times C_{s(f,i;NEE,i)}$ where $C_{s(f,i;NEE,i)}$ is negative. We already know that the sum of any linear combination of sources will have a smaller uncertainty after the inversion, but when we aggregate fluxes from the same pixel, the uncertainty of this total is smaller due to the smaller posterior variances and also because the covariances are negative. This demonstrates that the value of the inversion is to reduce the uncertainty on each of the individual fluxes and to additionally reduce the uncertainty of the aggregation of the NEE and fossil fuel flux within the same pixel. The reduction in the uncertainty of the sum of fluxes within the same pixel is going to depend on the size of the uncertainty of the NEE flux, which is usually the larger uncertainty. To improve the ability of the inversion to estimate the total flux within a pixel, we need to improve the skill of the atmospheric transport and we need to reduce the uncertainty in the estimates of the NEE. As it stands, with a large prior uncertainty in the estimation of the NEE fluxes from the CABLE model which remains a large posterior estimate after the inversion, the distinction between the fossil fuel and NEE flux from the same pixel is not very different from the prior estimate of the variance of the difference between the fossil fuel and NEE flux, $C_{s_0(f,i;f,i)} + C_{s_0(NEE,i;NEE,i)}$.

We would like to thank the referee for the suggestion of adding the investigation of what contributions NEE and fossil fuel fluxes make to the modelled concentrations at each site. This analysis has now been included into the manuscript.

We still have a long way to go before we can reliably estimate the fossil fuel fluxes from the City of Cape Town using this inversion framework, but our intention is to provide the building blocks for such an inversion system which would allow better products for fossil fuel emissions and NEE fluxes to be slotted in as they become available, while we work towards a reduction in the misfit between the prior and posterior modelled concentrations. For example, we could replace the bespoke inventory analysis used here for the global 1×1 km ODIAC fossil fuel product (Oda et al. , 2017) which has recently been used for Indianapolis.

The referee mentions that the gradient approach may have been more useful in the Cape Town inversion than implied in the original manuscript. The main concern we had about using the gradient method in these circumstances was that the direction of travel of an air parcel between the two sites would not necessarily be in a straight line due to the topography of the Cape Town region. Therefore selecting observation pairs based on the wind direction would not necessarily have given the true gradient in concentration between the sites. We supplied a map of the sensitivities of the observations sites to the surface fluxes to show this in the original manuscript. To further add to this argument, Figure 6 provides the average wind speed and direction for the domain for each month, which shows that, in general, the wind direction would not be in our favour, and with only two sites, that would leave very little information to constraint the surface fluxes. When the wind is blowing from the Hangklip site, it curves northwards towards the interior and away from Cape Town. When the Robben Island site is observing clean air before coming into the Cape Town area from the Atlantic side, such as June 2013, the wind changes for the north westerly direction once it passes over Cape Town to a more northerly direction, missing the Hangklip site. These wind plots will be included in the supplementary material instead of the original attempt at validating the CCAM wind data.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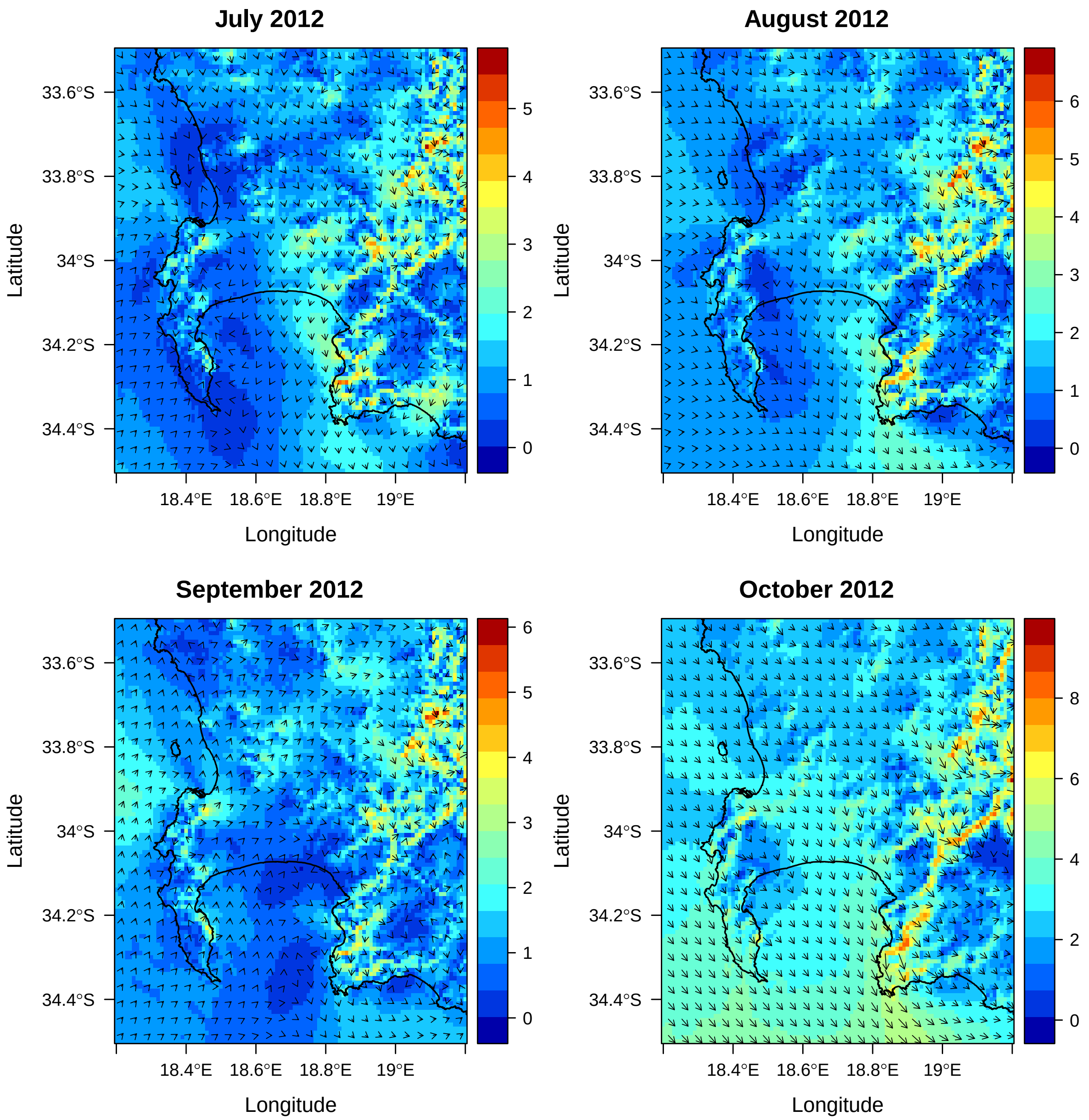
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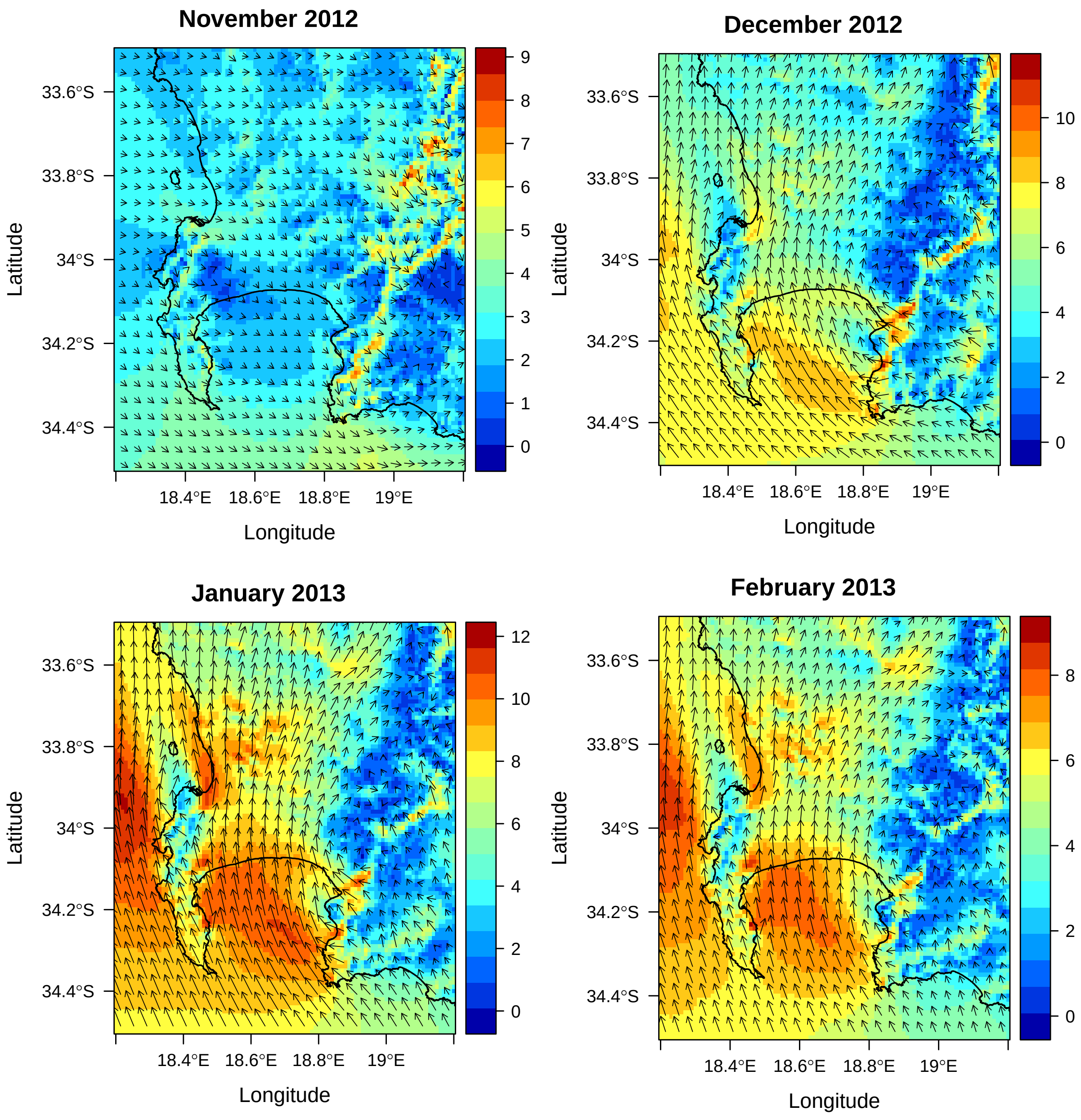
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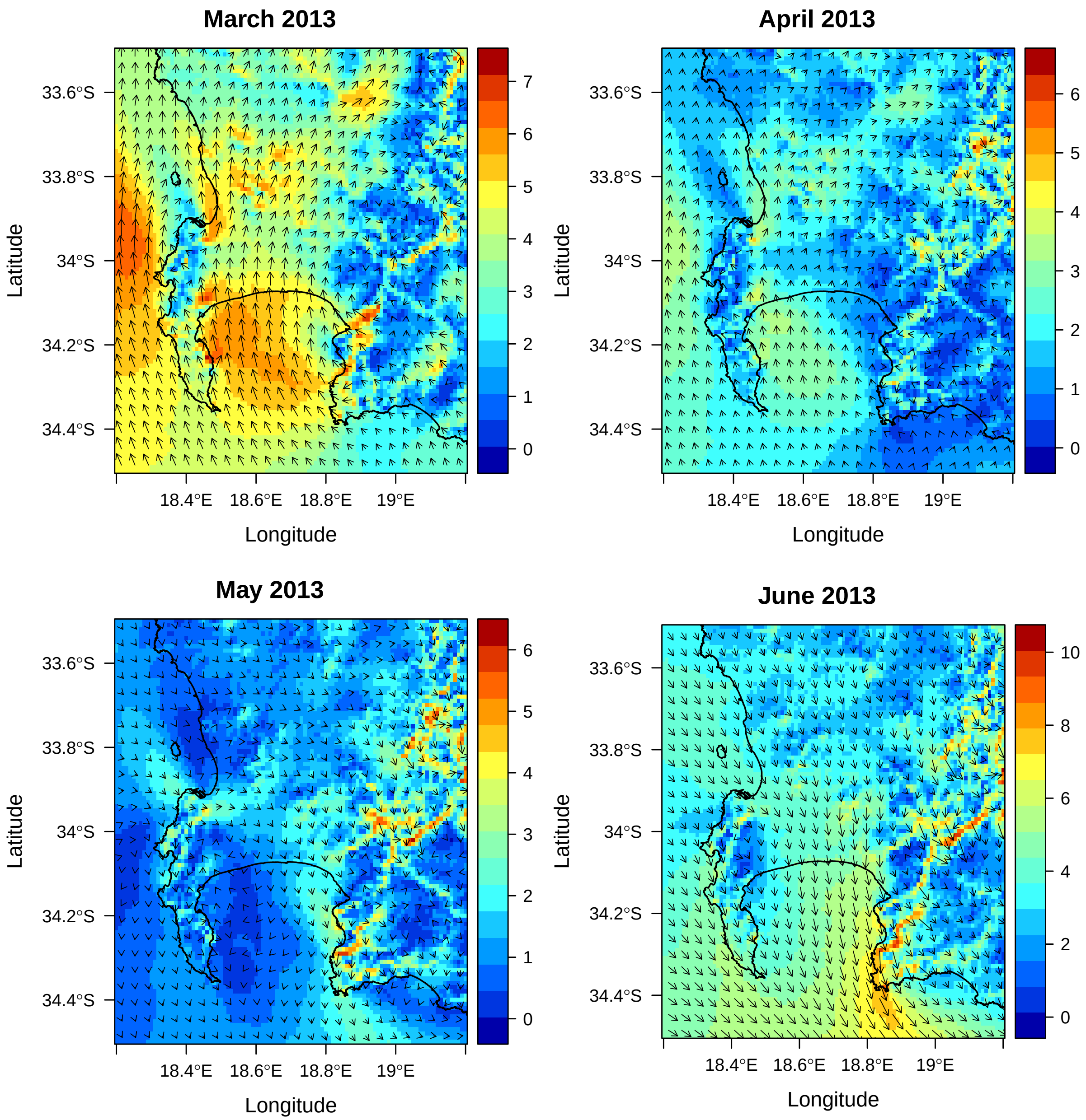

Figure 6. Mean modelled wind speed and direction in the Cape Town domain for each month. The colourbar represent the mean wind speed (m/s).

The third major issued related to the NEE uncertainty estimates. These were set to be large (relative to the NEE estimate), and we used the estimates of the productivity associated with the NEE estimate as the uncertainty. More precisely, which is made clearer in the manuscript, as the uncertainties were scaled so that the Chi-squared goodness-of-fit statistic was closer to 1, the productivity values (NPP) were used to define the relative uncertainties between the NEE estimates. There is a typo in the original manuscript which states we used Net Ecosystem Productivity for the uncertainties where it should be Net Primary Productivity. The NPP fluxes were squared to give initial estimates of the variances. The fossil fuel and NEE variance estimates were then doubled so that the Chi-squared goodness-of-fit statistic was closer to 1, and these scaled variances provided the final uncertainties assigned to the prior fluxes.

"*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) ?*"

We certainly agree that the large uncertainty we have in the NEE estimates produced by CABLE is a limitation and inhibits the ability of the inversion to apply corrections to the fossil fuel fluxes, in which we are the most interested. Modelling NEE in this region is uncertain. Wang et al. (2011) have shown that unless CABLE is closely calibrated for a specific system, it can lead to significant errors in the estimation of NEE. There are many land types in the region, which include the endemic fynbos biota, different types of agriculture, as well as fallow land. Although a great deal of work is being carried out to validate CABLE over the savanna biome, which covers much of South Africa, the information available for how well CABLE behaves for fynbos is limited. In the sensitivity analysis we will consider other models of NEE for this region, but for the reference inversion, we think that a conservative estimate of the error is best. Sensitivity analyses will consider the impact of reducing the uncertainty estimates to a smaller fraction of the NPP estimates from CABLE, particularly on the inversion's corrections to the fossil fuel fluxes.

The final major scientific issue that the referee raises concerns the inventory analysis:

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 "*

The referee is concerned about contributions to the fossil fuel budget from the different sectors of the city. The percentages that are quoted here come from a report on the energy consumption of the city, and do not entirely relate to the direct fossil fuel emissions from the city. Emissions from coal are small because most of the power generation capacity through coal occurs in the north eastern provinces of South Africa. Residential emissions are not negligible in Cape Town because many of the communities still rely on burning raw fossil fuels for heating, cooking and lighting. This discussion has been made clearer in the revised manuscript. Instead of discussing these energy statistics which are already discussed in the inventory paper, the discussion in the revised manuscript now reflects the percentage contributions as reflected in the inventory data available for this inversion. Using these statistics, the emissions from industry (based on the available fuel usage data) are 7.2%, 36.5% from vehicle road transport, 53.8% from the residential sector, and 2.5% from the airport and harbour.

The referee lists a number of additional issues which have been corrected in the revised 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.*"

The description of the Bayesian inversion framework has been rewritten in the revised manuscript. With regards to the covariances, the wording has been clarified.

"- *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)*"

We have made this description clearer in the manuscript to reflect that we are referring to the concentrations at the boundaries of the modelling domain when we refer to "boundary concentrations".

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

This has been corrected in the manuscript.

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

This has been corrected and the description of the methodology made clearer".

"- *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*"

This has been removed from the sentence.

""- *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)."*

As discussed above in response to the referees concerns regarding the atmospheric transport modelling, we think that the prior information is the largest source of uncertainty in our inversion system. We consider other configurations of the covariance matrix for the errors in the modelled concentrations in the sensitivity analysis paper.

The individual sentences identified by the referee on pages C6 to C8, and similarly problematic sentences elsewhere in the manuscript, have been amended or cut from the revised version.

"- *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.*"

Figures in the manuscript have been replotted to address the concerns highlighted above. The time series have been expanded to multiple panels as explained early, and the day and night-time concentration and residuals separated. Better use has been made of legends on the plots, to avoid including too much information in the caption. The maps have been improved so that labels are easier to read. The colour scheme has been slightly amended, but the purpose of deliberately not using a smooth colour scheme is to allow very large fluxes, like those from point sources, to be distinct from smaller fluxes, while still allowing differences in these smaller fluxes to be distinguished, such as subtle differences in the residential and transport fossil fuel fluxes. The pixel against which covariances are computed in Figures 15 and 16 has been clearly identified.

"- *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.*"

The notation for these equations has been amended to be more consistent with the rest of the manuscript.

"- *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.*"

Table 3 has been converted into a time series of box plots to show the prior and posterior distribution of pixel-level fluxes over the domain. These fluxes are also presented as maps in the supplementary material already. Table 3 was produced to deliberately consider the summary statistics of the pixel-level flux estimates within the domain, and to show how these summary statistics differ between months. The objective of section 3.2.2 was to address how the inversion was updating fluxes at the pixel level, and from month to month. In this section we were comparing the prior estimates $\mathbf{s_0}$ with the posterior estimates $\mathbf{s}$. Therefore the unit of the fluxes are $\mathrm{kg\ CO_2 m^{-2}\,week^{-1}}$, which is unit of the fluxes solved for by the inversion. Sections 3.2.3 and 3.2.4 were aimed at addressing what impact the inversion had on the total flux of $\mathrm{CO_2}$ over the whole domain. These two sections have been merged. The purpose of the month estimates was originally for the sensitivity analysis to compare between configurations.

As discussed earlier, the inversion has the most impact in reducing uncertainty when aggregating over fluxes in the domain, and the main objective of Table 4 is to show this uncertainty reduction. When aggregating fossil fuel and NEE fluxes, and aggregating over all pixels, the reduction in the posterior variances and the negative posterior covariances brought about by the inversion can be taken advantage of to produce posterior total estimates which have associated uncertainties which are much smaller than those of the prior total estimates.

"- *The acronyms CT and CBD are not defined explicitly.*"

This has been corrected.

"- *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.*"

This has been made clearer in the revised manuscript.

"- *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)*"

This has been corrected.

"- *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.*"

More discussion has been added on the percentile filtering technique. A detailed description of this technique is provided in (Brunke et al., 2004): "For baseline data it is necessary to remove continental and anthropogenic effects,which can be either an increase or a decrease from the baseline level. Baseline data will therefore lie in a band between a lower and an upper threshold. Similarly to the background cut-off described above, two 11-day moving percentiles (e.g. 5th and 95th percentile), each one adjustable by an additional factor, established the smooth curves for the lower and upper thresholds, respectively. In this way a narrow concentration band was obtained, where the greater part of unwanted continental and anthropogenic effects had been removed, retaining concentrations close to those of maritime air.".

The purpose of the percentile filtering technique is to remove those measurements which are strongly influenced by either anthropogenic emissions from Cape Town (which are observed very seldom by the tower) or those measurements strongly influenced by biospheric uptake of CO_2. This discussion has been expanded in the manuscript. We have deliberately made the margin of the domain around the City of Cape Town large, which allows the inversion system to solve for fluxes at large distances from the City, rather than relying on estimates of the concentrations at the boundary. Therefore when the air arriving at the measurement site originates from the north the inversion can account for uptake by correcting the far-field fluxes and leave the concentration at the far North or East boundaries relatively unchanged. In this way, the boundary CO_2 concentrations act more like baseline concentrations.

"- *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.")*"

The representation error is accounted for in the 2 ppm and 4 ppm assigned to daytime and night-time concentrations respectively. The discussion in the manuscript has been altered to better reflect this. The representation error occurs due to errors in the transport modelling. The distinction between aggregation error and representation error has been corrected in the manuscript.

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