

Interactive comment on “Impact of Siberian observations on the optimization of surface CO₂ flux” by J. Kim et al.

Anonymous Referee #1

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Review Kim et al. ACPD 2016

General comments

Kim et al.'s paper discusses the additional constraints on net biogenic CO₂ fluxes in Siberia brought by adding specific regional CO₂ observation. The authors add on top of a global ('control') data set (NOAA, WDCGG) additional data that is the Japanese Russian ('JR') network of station in Siberia. The control data set lacks stations over Siberia, a gap that the JR network fills successfully. The inversion set up is the well-established CarbonTracker (CT), and inversions of the control and control+JR sites are analysed comparatively. This paper discuss that adding JR observations in the setup, in the vast, poorly-sampled region of Siberia, brings additional information when estimating top-down fluxes. The CT inversion set up used in the present paper was

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described in two other recent papers (Kim et al. 2014a, b), where the authors applied CarbonTracker with a focus in Asia (including Siberia) to the ‘control’ data set. Previously, Saeki et al. (2013) already evaluated the impact of adding this same JR data set on the mean biogenic CO₂ flux, albeit with a different inversion set-up. Kim et al. find in relative terms a similar reduction in flux uncertainty when adding the JR network in the inversion. The paper lacks sufficient discussion on the ability of their model to reproduce JR and independent observations in Siberia, and rely on high level statistical analysis instead, which limits a deeper understanding of the problem. The paper also lacks distance to the opportunities and limitations associated to inverse modelling in an ‘under-documented’ region such as Siberia. However the material at hand is very valuable and provides potentially a basis for an in-depth discussion of CO₂ fluxes over Siberia. Therefore I suggest rejecting the manuscript to allow its authors to improve it and eventually resubmit.

Detailed comments

Abstract Please specify in the abstract the time period over which the study is done. The abstract should be more quantitative about the fluxes to illustrate the improvement brought by the additional data, in terms of control, updated fluxes, and uncertainty reductions. The abstract could state the improvements obtained over Saeki et al. (2013).
P2 L6 ‘useful information’: please provide a quantitative estimate for this statement.
Last sentence: please also mention the contribution to the estimation of European fluxes.

Introduction The first paragraph should mention also comparisons with inversion results, e.g. Dolman et al., Biogeosciences, 9, 5323-5340, 2012 The second paragraph (p2 l20-21) should discuss other factors leading to error in inverse modelling results, e.g. model error, representation error etc.: data sparseness is not the only one. P3 l5 after last sentence please discuss results from previous research, including Saeki et al. , 2013 here, as well as Dolman et al. 2012 (see above), and Berchet et al. (Biogeosciences, 12, 5393–5414, 2015).

P4 I22: 'CO₂ uptake (...) slightly increase': compared to what and by how much? It could be useful for the reader to be reminded if Zhang et al. used (landing/take off) vertical profiles or (cruise altitude) tropopause data.

Methodology Here the section is written for readers already initiated in CarbonTracker (CT). Many items are not explained or assuming a detailed prior knowledge of CT.

P5 I9: 'emissions': could these F's be defined as the 'a priori' emissions?

P5 I12 What is an 'ecoregion'?

P5 I13: Gurney et al (2002) uses 11 land regions and this research uses 126 land regions. Therefore the reference to Gurney 2002 might not be appropriate, or explanation lacking. Please explain the difference in region definition.

P5 I14-15: scaling factor: how are these (5 weeks, 1 week) durations chosen?

P5 I15: What is an assimilation cycle in this context?

P5 I15-17 the two sentences ('In each assimilation... assimilation cycle.') are unclear, please revise the explanation.

P7 I7 is EDGAR corrected for interannual growth of CO₂?

P7 I22-24: Is there a correction applied to account for the difference of the NIES scale in the inversion? The paper should mention how does this bias translates into uncertainty (especially when inversion correction modifies the balances Siberia vs Europe)

P8 I4: regarding the notion of having 'the same' MDM: it is not consistent with the fact that MDM is specified above to be determined by Eq. 6 in the paper. How can it be equal to 3 ppm? The authors should also explain why the same confidence is given to the JR-STATION network MDM (3 ppm) as for the US network, given their different tower design (e.g. sampling height) . The ability of models to reproduce the observations needs to be discussed to comfort the chosen value.

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P9 I4 The difference between the inversions should also include a comparison to the prior fluxes for the sake of further discussion. ‘The difference in fluxes . . . distinctive in . . . Siberia’: Here it seems from Fig 2 that the fluxes are modified because the CNTL inversion puts an anomalous large sink in Siberia in the absence of local measurements. Therefore the JR run brings back fluxes to a realistic value. The difference is reflecting a particular approach (be it prior fluxes or optimization process) chosen by the inversion.

P9 I13: How is the 1 sigma standard deviation determined (on which basis: ensembles, . . .)? Does error on prior fluxes intervene in the inversion uncertainty?

P9 I27: ‘uncertainty of the . . . uptake . . . is reduced’: this is quite expected and I suggest adding the word ‘is expectedly reduced’. However in terms of relative uncertainty (error ratio to estimated fluxes) no progress has been made. This reflects the fact that the CNTL inversion tends to allocate strangely large fluxes to Siberia in the absence of atmospheric measurement able to constrain this region

P10 I2-3: Since the drought affects Europe to a large extent, and the dataset is not different over Europe for the two runs JR and CNTL, it is not plausible that the drought can be used as an explanation for differences between JR and CNTL runs. Please revise this part.

P10 I5: This difference of number of observation and its impact on the fluxes may partly explain the time pattern of Fig. 4. Please provide some quantified information on the impact of the evolution of observations on the time pattern (e.g. by removing some sites or maintaining a sparse network and comparing with the long term run).

Explanations for the trends observed in Fig 4 and 5 should be discussed, see e.g. Sitch et al (2015, Biogeosciences, 12, 653–679)

P11 I16: what are background surface co2 fluxes?

P11 I18-20: How are the results of this study sensitive to MDM? could the authors test

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this assumption (well prescribed MDM) with different MDM values? Inversely, do poor (different from 1) values of this chi-2 parameter for other stations imply that the MDM is poorly prescribed? (e.g. BAL, MNM, ...). Is MDM not dependent on sampling height in the JR station network?

To support this statement the authors should show and discuss comparison of JR-station CO₂ observations and model (prior and optimized, in the JR experiment context).

P11 I29. Sites with 7.5 ppm MDM are presented as afflicted by poor model simulation of their observed CO₂. However no confidence is given about the JR-STATION sites in terms of the accuracy of model representation of CO₂ at these sites, only a mean bias which is a very limited measure of model–observation fit improvement. This should be presented and extensively discussed prior to discussing the result of inverse modelling with a blind approach to the forward simulated CO₂ (this is also directly related to the comment above).

Overall section 3.2 is too limited and lacks a conclusion to support the subsequent analyses, especially in view of demonstrating the value of additional observations offered by JR-STATION.

In section 3.3. Fig 7 8 and 9 are valuable but not sufficient in themselves to allow the reader to appreciate the contribution of the additional JR observations. a mapping of prior uncertainties, CNTL UR and JR UR would be required to support the discussion.

P12 I15: ‘additional observations sometimes have a great impact’ : Please be more explicit and quantitative about ‘sometimes’ and ‘great impact’.

P12 I16: The author find stronger UR in summer than in winter. Is this due to a higher uncertainty related to larger net fluxes in summer relative to winter? How is this seasonal UR difference explained over Siberia?

P13 I7-8: please give more details about why high 1-week RMSD and self sensitivities

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of JR STATION sites is consistent.

P13 I9: 'it takes 5 weeks to affect the surface CO₂ fluxes in Siberia by the transport of CO₂ concentrations' : this statement is not supported by the demonstration in Kim et al. (2014, see their Fig 13), who only compared 1 week and 5 weeks, but not other time intervals. Therefore this incorrect interpretation needs to be reformulated. I could suggest a sentence such as 'it takes more than 1 week to affect the surface CO₂. . .', which is better supported by the elements provided.

However this observation by the authors is important. If the correction of Siberian surface fluxes, in CNTL, is only performed based on air masses between 1 and 5 weeks, it means that Siberia is an underconstrained region in terms of CO₂ fluxes (and this is a conservative statement). As a result, comparisons between CNTL and JR should take into account the large 'weakness' of fluxes allocated to Siberia by the CNTL inversion. This is illustrated in Table 2: EB is the region with the strongest increment between a priori and CNTL (from -0.07 to -1.17 PgC/y), but at the same time it is the only region with the least in situ observations. The other regions have smaller increment, and at the same time more 'local' observations are available. This bias needs to be explicated and discussed. The sentence of the abstract (p2, l1) comparing the fluxes calculated with the additional observations to the fluxes calculated without, suggests as such that the two flux values can be compared directly ('uptake. . . decreased'). On the contrary, Siberian fluxes calculated without the additional JR observations are highly sensitive to many assumptions within the inversion, and therefore any direct comparison requires a clear statement that this comparative approach is dependent on the inversion set up. This is also true for the sentence concluding this section (p13 l14-16).

Section 3.4. This section should also propose comparison with inversions intercomparison exercises, such as the TRANSCOM intercomparisons, see Gurney et al. (2002) or RECCAP Peylin et al (2013 Biogeosciences, 10, 6699-6720, doi:10.5194/bg-10-6699-2013). Please also compare with the synthesis work of Dolman et al. (2012)

P13 I26 the paragraph is concluded by the importance of the inversion framework used. Therefore the difference with the results of Saeki et al. (2013) needs to be examined in more detail. This paragraph leads the reader to the obvious conclusion that the choice of the inversion setup has as much impact on the posterior fluxes uncertainty of Siberia than the addition of a novel observation network such as JR-STATION.

Comparing the study reviewed and Saeki et al. (2013) from the numbers provided in the papers, it is striking that the two studies consistently conclude, in relative terms, both to a Siberian flux that is lower by about one third when adding JR-STATION (-37.5% for Saeki et al., -34.1% for this study), and decrease their uncertainty by about one-quarter (-22.7% and -24.7% respectively). At the same times, when comparing the two studies (being based on similar observation data sets), Kim et al.'s find fluxes consistently two times higher than Saeki et al. with or without the JR-STATION dataset, and report uncertainty that are 15% higher, with or without the JR STATION dataset.

Therefore the reported numbers lead to the observations that a change in inversion setup has more impact on the estimated fluxes in Siberia than using or not the only existing dataset in the region. This is stimulating because it means that more research is needed before CO₂ budgets calculated using inverse methods over Siberia can be reliably used. It supports the suggestion for extensive comparison of the simulated and measured CO₂ at the JR STATION sites in this study.

The numbers above also imply that the current set up questions the constraints on CO₂ fluxes (in terms of range of likely flux value) over Siberia reported by Saeki et al. 2013, and this requires further comparison with other studies, possibly bottom up allometric or modelling studies. It should be noted also that Maki et al. 2010 reported smaller uncertainty (0.41 PgC/y), even without the JR-STATION network. This requires a detailed comparison of these studies for the sake of coherence.

P14 I2 and 3: what is the uncertainty range of single-year flux values (here 2008 and 2009 are discussed)?

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Section 4 Please revise the conclusions according to the discussion above. What are the key challenges identified by the authors before estimating robust CO₂ fluxes over Siberia from atmospheric inverse modelling?

P15 I9 is the longitudinal redistribution toward Europe a conclusion shared by Saeki et al., 2013?

P32 fig 5 further discussion of the trend in European fluxes and its possible drivers would be valuable.

P33 fig 6 please add the prior fluxes seasonal cycles to each panel

Editorial comments

There are several occurrences of unexplained acronyms; the authors should check this carefully.

P2 I13: typo: Schulze et al, not Schuleze et al

P3 I10: 'useful information': please be more specific on the usefulness of this information.

P3 I18: due to the difference in time period the word 'accompany' is not correct, should be e.g. 'preceded'

P3 I21: typo: YAK-AEROBO -> YAK-AEROSIB.

P3 I25 : multi-year Zotto measurements can certainly be used for inverse modelling.

P4 I11: 'increasing the sites': should be 'increasing the number of sites'

P5 I19: expand EnKF acronym

P5 I22 (eq. 2) and I26: please be consistent on notation: x (superscript) b or x (subscript) b

P6 I7-13: ensembles, perturbation, background error, localization, physical distance:

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these notions have not been introduced before, please provide guidance for the reader to understand

P6 I15-17: please revise this sentence for clarity and syntax

P7 I7 please give references for CDIAC and EDGAR.

P7 I12: please provide link for ESRL data set, and give credit consistently to organization operating the measurements (e.g. in Europe).

P7 I29: Is the MDM 'determined' or incremented? It is unclear with this formulation. Please revise accordingly. What is intended by 'innovation' chi-2?

P8 I17: typo: exists -> exist

P8 I25: 'from two experiments': I suggest to change to a determined form 'from the two experiments'

P9 I4 'greater': please quantify in the text

P9 I14: global total optimized CO₂ fluxes': the wording is problematic because this does not include fossil fuel and forest fire. Should be e.g. 'total biogenic'.

P9 I20: typo 'between two the experiments' -> 'between the two experiments'

P10 I2 please revise and clarify: 'is reduced all years', I suggest 'is reduced in JR for all years'

P10 I9 readability would be improve to write Siberia instead of 'EB'. What is 'ET'?

P10 I14 the figure is not a histogram (binned distribution) but a time series. Please correct accordingly.

P11 I1 'Additional Siberian data": very indeterminate formulation. Please add e.g. 'These additional JR Siberian data'

P11 I17 what are background scaling factor? This seemingly important notion should

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be explained in the section on experimental framework.

P11 I22 ET: explain acronym

Section 3.3. : Sect. 3.3. or part thereof should be before 3.1 and 3.2 as these sections 3.1 and 3.2 discuss already on the basis of CNTL vs JR runs. The structure of the paper could be reassessed for the benefit of readability.

P12 I10: Conifer: typo (confer). Why are only Conifer Forest of EB mentioned with no discussion of other ecosystems? What do they represent vs the rest of the Siberian ecosystems?

P12 I11 'which has additional information'; I suggest to use a determinate form 'which has the additional information'

P12 I14: 'the magnitude of the maximum uncertainty reduction is higher than the average value': this is certainly trivial. Please remove this sentence. I don't see the value of maximum weekly UR at all and I suggest to remove this panel 7b

P12 I26 please relate the definition of self sensitivity to the

P12 I26 what are 'Continuous site category observations'?

P13 I6-7: 'fluxes. . . are analysed by direct observations at the first cycle': this sentence might not be clear. Please rephrase.

P13 I21 What is CT2013B?

P13 I22 Please be careful when reporting numbers. The uncertainty of 0.41 PgC/y is wrong , the correct number is given in your Table 5 (0.61).

P15 I20. Please add acknowledgments for other observational data providers.

P23 Table 1 the table needs to differentiate altitude and sampling height, which is a potential indicator of how difficult it is to simulate a particular site. Also please make sure the proper credits are given to the providing Laboratories (last 7 lines, in Europe).

P24 Table 2 I suggest to add total Northern hemisphere

P27 CT2013B and CTE2014: please give reference.

P32 Fig 5 Panel (a): please correct typo (Euraisan -> Eurasian)

P32 l3 there are no 'ocean' in this figure, please correct Fig 5 caption accordingly.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2015-875, 2016.

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