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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "On the potential of ICOS atmospheric CO_2 measurement network for the estimation of the biogenic CO_2 budget of Europe" *by* N. Kadygrov et al.

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We thank the referee for his analysis of our manuscript, which helped improve our study. We hope that our answers and the new discussions in the manuscript will satisfy his queries.

Questions/comments from the Referee, answers to the comments and changes to the manuscript are presented according with the following notation: Q) Questions and comments A) Answers to the comments C) Changes to the manuscript

Q.1) This paper describes efforts to assess the impact of an expanded European in

situ GHG network (under ICOS) on our ability to determine net terrestrial biospheric CO2 fluxes over Europe. In particular, so-called Observation System Simulation Experiments (OSSEs) are used, in which atmospheric CO2 inversions are performed on pseudo-data, under various model and data configurations. Overall, the paper is well written, the figures are clear, and the analysis, for the most part, is sound. The topic and quality are appropriate for ACP.

A) We thank the reviewer for sharing this opinion.

Q.2) However, there are some significant assumptions and/or missing elements that make me doubt that the experiments conducted are sufficient to answer the question of how well the eventual ICOS network will be able to determine annual NEE over the ICOS European domain.

A) We hope that the new details we provide have helped clarifying the relevance of our tests.

Q.3) To my mind, the main issues that are not dealt with fully, but that can have a major impact on retrieving CO2 fluxes from CO2 data in a regional inversion are: 1) The CO2 (and secondarily, meteorological) lateral boundary conditions, especially how uncertainty (both bias and 'noise') in the boundary CO2 fields propagates into the flux solution; and 2) The time-dependent fossil fuel emissions inside the domain, and how uncertainties (noise and bias) will propagate into NEE flux retrieval. For both boundary CO2 and fossil fluxes the issue is not simply one where the uncer- tainty of the NEE will increase as a result of propagating errors. But there is the major issue that biases in these fixed parameter fields will alias into NEE biases. In other words, the results of the study, at present, need to caveated by saying that "In the limit of perfectly known fossil fuel emissions and lateral boundary conditions the proposed ICOS network will be able to solve for NEE with such and such resolution."

A) We have extended the discussions in Sect. 2.2.1 and Sect. 4 on the weight of uncertainties in the boundary conditions and anthropogenic (fossil fuel) emissions. These discussions were primarily based on that of Broquet et al. (2011). Previous (e.g. Peylin et al. 2011) and on-going (but not published yet) experiments tend to indicate that the amplitude of the signature of such uncertainties at European ICOS-like stations is well smaller than that of uncertainties in the NEE and in the atmospheric transport. The impact of uncertainties in the boundary conditions during the inversion is further decreased by the fact that the inversion, on the first order, exploits gradients between the measurement sites to constrain the NEE. Since the spatial scale of the signature of the boundary conditions is relatively large compared to the distance between neighbor sites, especially under west wind conditions, their signature is often similar and does not impact much the retrieval of the NEE between these sites. Through the statistical consistency between actual differences between the inverted NEE and averages of eddy covariance NEE measurements, Broquet et al. (2013) indirectly confirmed the robustness of the budget of uncertainties in the inversion configuration and the fact that this inversion was not biased even though they assumed that uncertainties in the boundary conditions and anthropogenic emissions are negligible for their experimental framework.

Section 4 acknowledged that with the extension of the network, the sensitivity to uncertainties in the fossil fuel emissions could increase. We have now added further caution regarding this topic in this last section.

C) In Sect. 1, we have modified the sentence: "This gives confidence in the configuration of this system, described in Broquet et al. (2011, 2013), and in the underlying assumptions (e.g. on the unbiased and Gaussian distribution of the uncertainties, or regarding the weak impact of the uncertainties in the CO2 modeling domain boundary conditions at the edges of Europe, or in the CO2 fossil fuel emissions) for the estimation of the performances of the ICOS network."

In Sect. 2.2.1, we add / modify the sentences: "Peylin et al. (2011) indicate that uncertainties in anthropogenic fluxes yield errors when simulating CO2 mixing ratios at ICOS stations that are smaller than atmospheric model errors. Furthermore, the

relative uncertainty in anthropogenic emissions is smaller than that in NEE, while on short timescales, the anthropogenic signal is generally smaller than the signature of the NEE at sites that are not very close (typically at less than 40km) to strong anthropogenic sources such as cities (see the analysis for the Trainou ICOS station near Orléans, in France by Bréon et al. 2015). Relying on such indications, we assume that the errors due to uncertainties in anthropogenic emissions are negligible compared to errors from NEE and atmospheric model errors. This is a fair assumption as long as most ICOS stations are relatively far from large urban areas, which should be the case since the ICOS atmospheric station specification document (https://icos-atc.lsce.ipsl.fr/?q=doc_public) recommends that the measurements sites are located at more than 40km from the strong anthropogenic sources (such as the cities). Zhang et al. (2015) yield conclusions from their transport experiments at 1° resolution which contradict this assumption and this clearly raises an open debate. However, the evaluation of the inversion configuration from Broquet et al. (2013) supports our use of this assumption for our study." "Again such an assumption is supported by the evaluation of the inversion configuration by Broquet et al. (2013). The relatively weak impact of uncertainties in the boundary conditions in Europe (while studies in other regions such as that of Gockede et al. (2010) indicate a high influence of such uncertainties) can be explained by the fact that the spatial scale of the incoming CO2 patterns at the ICOS sites from remote sources and sinks outside the European domain boundaries is relatively large due to the atmospheric diffusion (especially under west wind conditions, when the air comes from the Atlantic ocean) compared to the typical distances between the ICOS sites. In principle, the inversion mainly exploits the smaller scale signal of the gradients between the sites to constrain the NEE, and it is thus weakly influenced by the large scale signature of the uncertainty in the boundary conditions."

Finally, in Sect. 4, we add "The assumption that uncertainties in the boundary conditions and in the anthropogenic emissions have a weak impact on the inversion is also supported on average by the results of Broquet et al. (2013). But when assessing results for specific areas such as in this study, this assumption may be weakened in highly industrialized countries or close to the model domain boundaries."

Q.4) Another issue that is never addressed in the paper is that of whether the absolute uncertainties produced by this system might be useful enough to meet the ICOS/EU/national objectives. All of the figures in the main text, for example, deal with relative uncertainty reduction. It is only in the Appendix (Fig. A2) that absolute uncertainties are shown at the country scale. Moreover, it's not clear to the reader whether these values, say 0.25 gC/m2/day, would be useful policy-wise. I don't mean to say that the paper needs to include a C policy analysis, but some guidance or reference point needs to be provided to interpret the absolute uncertainties.

A) We fully agree with this comment. The previous discussion compared the posterior uncertainties to typical estimates from the ORCHIDEE vegetation model only.

C) Figure A1a) and the plot of posterior uncertainties at the national scale when using ICOS23 are now merged and put in the main text (as Fig. 12), and it is discussed, along with fig A1 (which used to be Fig. A1b)) and Fig. A2.

However, to our knowledge, no notional target for the uncertainties in NEE at the national scale have been reported by the ICOS community.

C) In section 4, we have added: "These numbers can be compared to the uncertainty targets defined for the CarbonSat satellite mission (ESA, 2015): 0.5 gC m-2 day-1 at the 500 km$\times$500 km and 1 month scale. Figures 12, A1 and A2 shows that at the 2-week and national scale, the prior uncertainties are systematically well larger than this target, but that the posterior uncertainties in Western and Northern Europe are generally close or smaller than this target even when using ICOS23. Since the temporal correlations in the prior uncertainty have a 1 month timescale and since the temporal correlations in the posterior uncertainty should be smaller, these uncertainties at the 2-week scale can be considered to be equal or lower than the corresponding uncertainties at the 1 month scale. Therefore, this indicates that the inversion is required to reach the target from the CarbonSat report for mission selection. It also indicates

that this target is likely not reached in a large part of South Eastern Europe even when using ICOS66 but that for countries like the Czech Republic and Poland, extending the network from ICOS23 to ICOS66 allows reaching it. Finally, it indicated that the ICOS23 network is sufficient to reach this target in Western Europe."

Q5.) Specific comments: P14222, 12: Given my concerns on the absence of boundary CO2 and FFCO2 in the OSSEs, I don't think this is 'robust'. Also, strike final 's' from Experiments.

A) See our answer regarding the assumptions on the uncertainties in boundary CO2 and FFCO2 emissions above. However, we have removed the term "robust" since it is difficult to explain the value of this term in the abstract. We have also removed the s from Experiments.

Q.6) P14222, 25: Strike 'resp.' in two instances. Not necessary and makes one erroneously think 'respiration'. P14233, 18: Strike 'The' at the start ;of the paragraph.

A) Done; it was meant P14223, 18 instead of P14233, 18

Q.7) P14225, 3: Insert 'are' at the beginning of the line. P14225, 9: Strike 's' in performances. P14225, 28: strike 's' in Experiments.âĂÍP14226, 15: change 'built' to 'build'

A) Done

Q.8) P14227, 6: Earlier, the study is described as 'state of the art', yet using 50 km resolution for meteorology for a regional European inversion hardly seems so. (I understand the need, however, to solve for fluxes at 50 km to reduce the dimension of the problem.)

A) One or two systems have been recently developed for the inverse modeling of CO2 fluxes at the European scale using higher resolution meteorological forcing and Lagrangian transport modeling, which, in theory, allows for representing the transport at the meteorological forcing resolution. To our knowledge, the application of the inversion using such models over a several-year period (such as in Broquet et al., 2013) would be highly expensive and has not been attempted yet. These systems have not been applied for assimilating real data yet. Finally, they solve for the fluxes at a resolution similar to that of our system as indicated by the reviewer. First publications using such models will arise but to our knowledge this is not yet the case (which is why we do not complement the text to discuss about this). Similar systems may have already been applied over areas whose size is similar to Europe on other continents, but we do not think that the spatial resolution of the transport modeling is the only important criteria to define the level of advancement of an inversion system. Inverse modeling is complex enough so that one could use high-resolution systems at the cost of a poor representation of uncertainties. In this context, the use of a variational data assimilation approach, the inversion of NEE at 6-hour/ 0.5° resolution and the level of evaluation lead by Broquet et al. (2013) justify, for us, applying the term "state-of-the-art" to our system.

C) We complement the two sentences mentioning that this system is "state-of-the-art": in the abstract we add "variational" to "state-of-the-art mesoscale variational atmospheric inversion system assimilating hourly averages of atmospheric data to solve for NEE at 6 hour and 0.5° resolution" and at the end of the introduction, we modify the sentence "The manuscript first documents the potential for constraining NEE, through the use of a state-of-the-art (i.e. which solves the NEE at high spatial and temporal resolution, and which has been submitted to a high level of evaluation) variational atmospheric inversion system, and of the ICOS23 network containing existing sites and other stations that could be installed on tall towers over Europe in the coming years."

Q.9) P14228, 17: When using 'hourly averages', it's not clear if these are night and day or only daytime (or as in Broquet, 2011, do they change by site class/altitude).

A) This line corresponded to the very beginning of the description of the method. A specific subsection called "Time selection of the data to be assimilated" is dedicated to this topic later. And this sentence indicated that we use the method of Broquet, 2011,

which implicitly indicates that we use their observation selection.

C) However, in the updated manuscript, we have added "(over restricted time windows everyday depending on the type of sites that are considered, see Sect. 2.2.2.)" here to anticipate the description of the time selection here.

Q.10) If using nighttime data, are the corresponding 'data' error values in R inflated to account for the likely inability of the model to accurately simulate nighttime boundary layer structure?

A) We do not use nighttime data at low altitude sites. And this problem does not impact high altitude sites (see Broquet et al. 2011).

Q.11) Moreover, if using consecutive hourly data, although off-diagonal elements are not included in R to account for hour-to-hour correlated errors in the meteorology, are the diagonal elements inflated to account for this effect? This issue is important, because if the effective number of independent observations in the analysis is too high (i.e. uncorrelated errors for consecutive hourly averages), then the uncertainty reduction produced will also be too high (according to eq. 2 which defines posterior covariance). Some, but not all, of this information is available from Broquet, 2011. More explanation is deserved here.

A) From our point of view, all this information is contained in Broquet et al. (2011) and / or reminded from Broquet et al. (2013) and discussed in this manuscript. There is no simple evidence that the temporal autocorrelations of transport errors should be significant in the analysis led by Broquet et al. (2011, 2013). Ignoring them leads to better agreement between the inversion and the averages of eddy covariance flux measurements in Broquet et al. (2013) than when including them (ignoring them might already be balanced by an overestimate of the standard deviation of the errors for individual hourly concentrations). This was stated at the end of the subsection "Observation error covariance matrix" which explicitly discussed the potential increase of the standard deviation of the observation error in order to account for potential temporal autocorrelation of this error.

C) We have tried to better emphasize these discussions in Sect. 2.2.2 by modifying/adding the sentences: “Indeed, there is no evidence that such autocorrelations could be significant in the analysis of Broquet et al. (2011). The resulting budget of observation errors at daily to monthly resolution seems reliable (Broquet et al. 2011, 2013). It could be due either to a compensation of ignoring the temporal autocorrelations by an overestimate of errors for hourly data, or to the fact that the temporal auto-correlations of actual observation are negligible (Broquet et al. 2013). However, in both cases, the assumption that the temporal autocorrelations of the observation error are negligible does not seem to need to be balanced by an artificial increase of the observation errors for hourly averages.”

Q.12) P14228, 25: As mentioned earlier, assuming that errors in fossil fuel emissions are “negligible” compared to transport errors is a big assumption, and one I doubt without good evidence to the contrary, which is not provided here.

A) We actually cite the study by Peylin et al. (2011) at the beginning of this sentence to support this. However our other indications arise from on on-going experiments by some of the co-authors of this manuscript that have not been published yet. See the corresponding addition to Sect. 2.2.1 that is stated above in answer to the general comment of the reviewer on this topic.

Q.13) The paragraph goes on to say that ICOS sites are “relatively far from large urban centers”, but it’s not clear what “relatively” means in this case.

A) The ICOS atmospheric station specification document states: ”Avoid short distance (usually less than 40 km) from strong anthropogenic sources (e.g. city) especially if located upstream of the prevailing wind. This is to ensure that observations can be represented in atmospheric transport models with spatial resolution of around of 10-20 km. In case of proximity to strong anthropogenic sources, a footprint and representativeness analysis should be performed.” (https://icos-atc.lsce.ipsl.fr/?q=doc_public)

C) We now provide some of this more precise information in section 2.2.1: “This is a fair assumption as long as most of ICOS stations are relatively far from large urban areas, which should be the case since the ICOS atmospheric station specification document (https://icos-atc.lsce.ipsl.fr/?q=doc_public) recommends that the measurements sites are located at more than 40km from the strong anthropogenic sources (such as the cities).”

Q.14) Even if “relatively” here means that ICOS sites have in their 50x50 km cells one or two orders of magnitude less emissions than urban grid cells, the “local background” levels of FFCO2 will still be impacted.

A) There is a critical difference between the level of FFCO2 and the level of uncertainty in FFCO2. At the annual scale, the anthropogenic signal is high compared to the natural one. However, the natural signal includes a seasonal oscillation whose amplitude is very high compared to its annual mean. Furthermore, the relative uncertainty in FFCO2 emissions is well lower than that in NEE. This explains why, at the temporal scales analyzed in such a study, for stations that are not very close to strong anthropogenic sources, the signature of uncertainties in the NEE is larger than that of uncertainties in anthropogenic emissions.

C) This answer explains the addition to the beginning of Sect. 2.1: “Furthermore, the relative uncertainty in anthropogenic emissions is smaller than that in NEE, while on short timescales, the anthropogenic signal is generally smaller than the signature of the NEE at sites that are not very close (typically at less than 40km) to strong anthropogenic sources such as cities (see the analysis for the Trainou ICOS station near Orléans, in France by Bréon et al. 2015).”

Q.15) In short, there may well be bias in the FF product used, including potential (missing) covariances between the temporal FF patterns and transport (see e.g., http://www.atmos-chem-phys-discuss.net/15/20679/2015/acpd-15-20679-2015.html). The bottom line for me is that especially in Europe with high emissions density, there needs to be a careful analysis of how these errors propagate into NEE estimates. If the error in NEE due to fossil fuel emissions is low, this would be a great result, but I think it needs to be demonstrated, not assumed.

A) The weakness of the signature of uncertainties in FFCO2 emissions at ICOS-like CO2 measurement sites is demonstrated by Peylin et al. (2011). Our own experiments using CHIMERE at 0.5° and transporting differences between existing inventories yield even smaller signal at such sites. City scale (Bréon et al. 2015, see the ref in the new manuscript) or 14C analysis (Levin, I., Munnich, K.O. and Weiss, W.: The effect of anthropogenic CO2 and 14C sources on the distribution of 14CO2 in the atmosphere, Radiocarbon 22, 379-391) approaches are presently developed to track uncertainties in anthropogenic emissions because of this. This can be viewed as an open debate and we now acknowledge and cite Zhang et al. 2015. But we feel that Broquet et al. (2013) demonstrate that our inversion of NEE is not biased by ignoring uncertainties in the anthropogenic emissions. See our corrections to the text which are stated in answer to previous comments corresponding to this topic.

Q.16) P14229, 8: While I agree that it would be possible to correct much of the boundary condition bias through careful examination of 3D global model CO2 fields and upwind CO2 observations, I still think it is very important to propagate the random uncertainty from the boundary into the posterior flux estimates. This could be done in a number of ways mathematically, all the way from solving for one boundary value per observation in the state vector x (along with uncertainty), to simply inflating elements of R.

A) It is definitely possible to add some terms in the inversion system to account for some types of uncertainties in the boundary conditions if we anticipate that their impact on the inversion of NEE is high. However, results from Broquet et al. (2013) do not support this assumption. This may be related to the specific configuration of Europe with dominant winds from the Atlantic Ocean. We agree that this is an open debate and we will more emphasize this point. See our corrections to the text that are stated

in answer to previous comments corresponding to this topic.

Q.17) Because the distance between the western boundary and the majority of the sites is of order 1-3 days PBL travel time, the boundary CO2 uncertainty, if taken into account could substantially inflate the NEE uncertainty.

A) This western boundary is located in the ocean where the patterns in the CO2 concentrations should have a relatively large scale due to horizontal diffusion on the path from North America to Europe and to the large scale of the ocean fluxes. 1-3 more days of transport should further increase the spatial scale of the signature of remote fluxes outside the domain and thus it should not impact the gradients of CO2 within Europe. See our corrections to the text that are stated in answer to previous comments corresponding to this topic.

Q.18) P14230, 7: 'image' is confusing and unusual terminology here. Please clarify.

A) We now use the term signature. However, in mathematics, "image" is a basic terminology for the output of a function.

Q.19) P14230, 23: This view of eq. 2 (i.e. posterior cov. A) is overly optimistic. Sure, the equation tells you that there's no sensitivity to fossil fluxes or the boundary, but that's a limitation of the equation, not a reflection of reality.

A) This part is purely mathematical and does not raise any optimism regarding the different sources of uncertainties. Fossil fuel emissions or boundary conditions could be included in the control vector or, by mathematical definition of the inversion problem, they have to be part of the observation operator. In both cases, uncertainties in fossil fuel emissions or boundary conditions would have appeared mathematically in the error covariance matrices (either the prior or the observation error covariance matrix) and the equation and this sentence would have been exactly the same.

Q.20) P14231, 13: It's not true that the dimension of the problem precludes an analytical solution (thus requiring 4DVar and the like). The system of Yadav and Michalak

(GMD, 2013), allows for the relatively easy inversion of large matrices, with no loss of accuracy.

A) We have slightly modified the sentence. The inversion of large matrices is not the only limitation for analytical computations. The main one often appears to be the building of the full matrix corresponding to the observation operator. It is still feasible if being able to spend a huge amount of computing resources over a long time period, but such resources were not available for this study.

C) In section 2.2.1 we now write: "we could not afford the analytical computation of Eq. (2) based on the full computation of the H matrix (using a very large number of CHIMERE simulations; Hungershoefer et al., 2010)."

Q.21) P14231, 28: change 'these' to 'the'.

A) Done

Q.22) P14232, 9: What are the potential impacts of a 500 mb (âĹij 5 km) ceiling for the model? For example, what if vertical transport (storms in the winter and convective lifting in summer) were to transfer surface signal into the upper troposphere? Is all this ok as long as there are no observations above this height? I'm not sure of the implications, but I would be more confident of the study if this issue was addressed.

A) Yes, in principle, as long as there are no observations close to the top of the model, there is no direct implication of this ceiling. The issue could be that this ceiling deteriorates the quality of the transport modeling near the ground. But for such regional applications it does not seem to have significant impact. And it would have been accounted for in our diagnosis of the model error based on radon model – data comparison. We do not feel at ease with introducing such a digression in the text since a ceiling of the regional transport models is routinely used for regional tracer transport modeling (e.g. Marecal et al. 2015).

C) We still add the parenthesis "(such a ceiling being usual for regional transport mod-

eling when focusing on mole fractions close to the ground, e.g. Marécal et al. 2015)" here.

Q. 23) P14232, 10: Fill in the missing section number after 'section'.

A) Done, actually the corresponding paragraph "Spatial and temporal domains" is not numbered as a subsection.

Q. 24) P14233, 3: Regarding edge effects, is a three day buffer at the end of the inversion period sufficient to capture all upwind fluxes ending on day 14 of the main period? Consider observations on the eastern part of the domain: fluxes from the western side of the domain may not have travelled all the way across (assuming westerly flow). Thus these fluxes may not be as well constrained as fluxes during the middle of the study period.

A) We acknowledge that the advection of tracers throughout Europe can last more than 3 days. However, atmospheric diffusion makes the amplitude of the signature of NEE generally quite low and negligible after 3 days. This is now better commented in the text.

C) We add the sentence "Indeed, the advection of CO2 throughout Europe can last more than three days, but the atmospheric diffusion ensures that the signature at ICOS sites of the NEE during a 6-hour window is generally negligible after three days of transport (not shown)." in Sect. 2.2.2.

Q.25) P14233, 16: Strike 'months' at the end of the paragraph.

A) Done

Q.26) P14234, 7: Please specify what the range of the scaling factors on Rh is?

A) Done. Actually, the scaling factors depend on the 6-hour window of the day and we give the value (i.e. $\sim$2) for the resulting factor to convert daily mean Rh into daily mean uncertainties.

Q.27) P14236, 5: (see also final comment p14248): The authors may also want to cite Bousserez et al, 2015, Quarterly. J. Royal Met. Soc. concerning the number of ensemble members required for a given degree of accuracy of the posterior covariance matrix.

A) The results of Bousserez et al. (2015, their Sect. 2,1) are not easily applicable to our case because our problem differ a lot and we prefer avoiding to open a complex digression here.

C) We still modify the corresponding sentence: "Similarly to Broquet et al. (2011), 60 members are used in each Monte Carlo ensemble experiment (this is also the typical number of members that Bousserez et al. 2015 use for their Monte Carlo simulations)."

Q.28) P14238, 16: Insert 'the' before 'south', otherwise this refers to Africa!

A) Done

Q.29) P14238, 25: It is not clear why 'there is generally a larger uncertainty reduction in July'. Please explain more.

C) The sentence is rewritten: "Because the prior uncertainties are larger and the observation errors are smaller in July than in December, there is generally a larger uncertainty reduction in July".

Q.30) P14239, 22: Change 'shows' to 'show'.

A) Done

Q.31) P14240, 9: Please explain the last sentence more. Why does this occur?

A) The reasons are similar to those for the same phenomena at the grid scale.

C) We now mention it by modifying the sentence: "In particular the uncertainty reduction is higher in July for western countries but higher in December for eastern countries for the same reasons as that given when analyzing the same behavior at the pixel

scale."

Q.32) P14240, 28: This comparison with CT-EU is hard for me to understand. First, how are annual scale uncertainties from CT being compared with uncertainties just for two weeks from the present system?

A) This was explained few lines later (see below). Still, we have removed this paragraph since it would have been difficult to clarify it without a long digression, since it was not a critical result from the paper, and since there were some assumptions regarding the conversion from annual to monthly uncertainties.

Q.33) Second, CT uses a five week window in its ensemble Kalman smoother and only produces covariances at these time scales. Any annual covariance from a system like this is not reliable in the first place.

A) The CT website acknowledges the low reliability of their estimates of uncertainty at the annual scale (http://www.carbontracker.eu/version.html). These estimates are based on a simple conversion of the estimates that they get at the 1 week or 1 month scale (http://www.carbontracker.eu/version.html). We just attempted at getting such estimates at the 1 week / 1 month scale (which can be directly compared to our 2-week mean estimates) back by doing the "revert" conversion, following similar assumptions that there is no correlation of uncertainties from month to month. The robustness of such assumptions was not an issue here. Still the explanation regarding this conversion in http://www.carbontracker.eu/fluxmaps.php?type=eur#imagetable or http://www.carbontracker.eu/version.html was not clear enough and we could have been wrong in applying our own assumptions for recovering the uncertainties at short temporal scales The corresponding text has thus been removed.

Q.34) P14241, 3: Change 'error temporal correlations. . .' to 'temporal correlations between uncertainties'.

A) Done

Q.35) P14241, 21: Figure 5 seems to have more spatial considered than just the 5 grid scales listed in the text.

A) Figure 5 shows the scales in km2 while the spatial scales are given in degrees squared in the text. When checking each curve separately, one better sees that they are based on 5 values only.

C) A comment is added to the legend of figure 5: “(in km2; for each curve values are derived for 1.5°x1.5°, 2.5°x2.5°, 3.5°x3.5°, 4.5°x4.5° and 10.5°x10.5° areas which correspond to different values in terms of km2 depending on their location in Europe)”

Q.36) P14247, 11: Delete ‘the’ before ‘wind speed’

A) Done

Q.37) P14247, 17: Change ‘results’ to ‘result’ A) Done

Q.38) P14248, 27: I understand that more iterations may be required for convergence with more observations, but would more ensemble members be necessary for accurate Monte Carlo uncertainties? Please see Bousserez, 2015.

A) We have now removed our assumption that the requirement in terms of the size of the ensemble should increase with the size of the problem.

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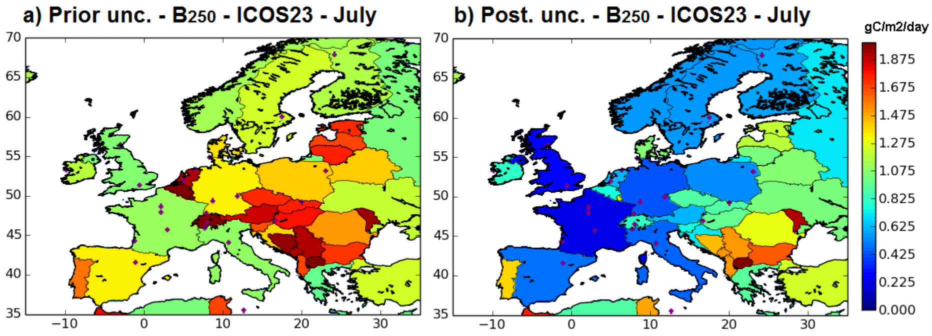
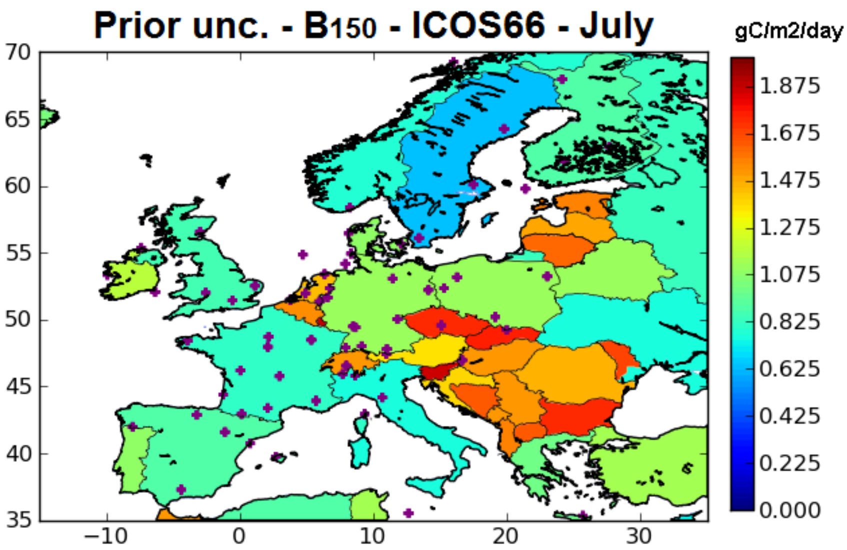


Standard deviations ($gCm^{-2}day^{-1}$) of the prior **(a)** and posterior **(b)** flux uncertainties at country scale. Posterior uncertainties are given for inversions using ICOS23 (red dots) and the reference inversion setup. Red/blue colors indicate relatively high/low uncertainties (with min = 0 $gCm^{-2}day^{-1}$, max = 1.975 $gCm^{-2}day^{-1}$ in the color scale).

Fig. 1. New Figure 12



Standard deviations ($gCm^{-2}day^{-1}$) of the prior flux uncertainties at country scale for July when considering $\mathbf{B}_{150}$. Red dots: ICOS66. Red/blue colors indicate relatively high/low uncertainties (with min = 0 $gCm^{-2}day^{-1}$, max = 1.975 $gCm^{-2}day^{-1}$ in the color scale).

Fig. 2. Modified Figure A1