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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) Top-down/inverse estimation of CO2 surface fluxes largely depend on the quality

of forward model transport and density of atmospheric measurement network. This study present idealistic tests of CO2 measurement network over Europe. They have used various scenarios of ICOS infrastructure. The topic of this is study is appropriate for ACP(D). However, I not sure how much of an impact this study will leave in the mind of scientists who are developing the ICOS network. Some of the ICOS project members are probably involved as coauthor.

A) Three of the co-authors of this paper have been active members of the ICOS consortium. This study has been encouraged by the scientists developing the ICOS network and its authors were invited to present it at several ICOS meetings. As detailed in the conclusion, and better demonstrated through the analysis of posterior uncertainties in the new version of the manuscript, this study supports extending rather than increasing the density of the current network. This extension of the network towards Eastern Europe is also now strongly pushed by the European commission. The study analyses the current potential of the network based on state-of-the-art or improved models. This type of tools are required to assess the influence of new stations, because of the dependence of this influence to the meteorological conditions and to its complex combination with the influence of the already existing networks.

C) We have added the sentence "In this context, the developers of the ICOS atmospheric network have encouraged network assessment studies such as the one conducted in this paper." in Sect. 1. See also the sentences added in Sect. 4 in answer to the following general comment.

Q.2) I am not convinced that this manuscript brings significant knowledge on how to optimally extend a regional measurement network. I get a feeling that the best network optimisation policy is to fill up the gaps (Section 3.2).

A) Our study opens the path (by demonstrating the capability for computing uncertainty reduction at the 6-hour / 0.5° resolution) to network optimization systems for the more precise location of specific sites. However, it does not attempt itself at deriving an optimized map for the extension of the network. Still, it gives a strong recommendation regarding the extension of the network in the East rather than increasing its density in the West. It does not just mean that we should “fill up the gaps” since the size of the gaps was unclear before such a study. It was not obvious that the network was already dense enough in the West so that one can already get useful improvement of the knowledge on the fluxes in this area using regional inversion systems and so that the impact of adding new sites there would be relatively weak. If the study had revealed that the uncertainty reduction at the national scale to the 0.5° resolution was very low with the existing network in the West, it would have been more sensible to increase its density there instead of setting up new sites in the East and failing to derive robust estimates at a relatively high resolution anywhere in Europe. This is now better discussed in the text based on a new analysis of the prior and posterior uncertainties in the estimates of NEE at the national scale.

C) In Sect. 4, we have added/modified the following sentences: “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×500 km and one 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.”

“The fact, in Western Europe, that notional targets for the posterior uncertainty in national scale NEE are already reached in Western Europe when using ICOS23, that the sensitivity of the posterior uncertainties at the national to 0.5° scale to increase in the network is relatively low, and the fact that results in Eastern Europe are highly impacted by the increase of the network encourage a spread of the ICOS network to poorly monitored areas rather than a densification of the core of this network in Western Europe. This recommendation sounds natural but this study would have rather supported a densification of the network in Western Europe if revealing that the density of the ICOS23 network was not high enough there, so that spreading the network in the East would have resulted in preventing from getting useful information about the NEE anywhere in Europe.”

“By demonstrating the capability for deriving scores of uncertainty reductions for NEE at 6-hour and 0.5° resolution, it supports the development of operational inversion systems deriving the optimal location for new sites to be installed in the European network.”

Q.3) However, I am not against publication of this manuscript in ACP. The authors have put large amount of resources to come up with reasonable conclusions.

A) We thank the reviewer for sharing this opinion.

Q.4) Some of the common problem remains in the manuscript.

In my opinion bias is the major in CO2 inverse modelling, even for the surface measurement sites. For example you choose to select data differently for inversion, depending in the site location, i.e., mountain vs valley. Actually, this introduced an “unknown” bias. This could be checked, say, by using data for all day vs afternoon or nighttime only.

A) In the OSSE framework which follows the assumptions of the theory underlying atmospheric inversions, assimilating both nighttime and daytime data at all sites would yield higher uncertainty reductions, but no deviation of the mean estimate compared to that when selecting night-time or daytime data only depending on the sites (this can be easily demonstrated mathematically). The term bias is not adapted to the impact of such a time selection. The principal impact of such a time selection is to have a smaller uncertainty reduction during periods of time when there is less data that are assimilated. If prior errors and model errors are not biased, the inversion cannot introduce any bias in the posterior estimates.

One could argue that actual ecosystem (prior) or transport model errors are biased over restricted periods of time such as nighttime or afternoon. In this case, it would generate biases in the posterior estimates of the NEE. However, this would have been detected in the analysis of Broquet et al. (2013).

As stated in answer to a similar comment during the review of the manuscript for publication in ACPD "The atmospheric signature of the natural fluxes over a given area and time period can be detected (before it vanishes through atmospheric diffusion) after several days in Europe. According to the OSSEs, even though the uncertainty reduction for nighttime fluxes is weaker than that for daytime fluxes, it is still high (Broquet et al., 2011). This does not seem to generate serious biases in the inversions with real data that are analyzed in Broquet et al. 2013. And this type of time selection procedure is used by nearly all the CO2 inverse modeling community (Peylin et al., 2013; see the full references given in the manuscript)."

C) We have added / modified the following sentences in Sect. 1: "Indeed, the distributions of the misfits between 1 month and continental scale averages of the flux measurements and of the NEE estimates sampled at the flux measurement locations revealed to be unbiased and consistent with the estimate of the uncertainties from the inversion system. 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 performance of the ICOS

network."

and in Sect. 2:

"This generally yields larger uncertainty reduction during daytime than during nighttime (Broquet et al. 2011). However, this does not raise a potential bias related to a better constrain on daytime inverted NEE (when the ecosystems are generally a sink of CO2) than on nighttime inverted NEE (when the ecosystems are generally a source of CO2) since uncertainties in both nighttime and daytime prior NEE, transport and measurements are assumed to be unbiased, as supported by the results from Broquet et al. (2013)."

Q.5) Although this paper is mostly about 'surface network' optimisation, it doesn't cite early works in the field going back to 1990s. It would be interesting to get a review of how this paper is different from the earlier optimisation tools, methods, and results. I understand that this paper is regional and the earlier papers did global analysis.

A) Yes, this manuscript focuses on dense networks using regional atmospheric inversion at relatively high resolution, while earlier studies before the years 2010-2015 targeted estimates for large latitudinal bands using very large scale systems and assimilating data from the sparse global networks of that time. Therefore it is difficult to compare its results and techniques with such studies, and we think that reviewing such studies would be out of the scope of this paper.

Furthermore, we cited OSSEs with such large scale systems from the years 2000s. We do not think that it is an advantage to cite even older paper, especially since atmospheric inversions were just emerging in the late 1990s. Still, we have added a reference from 1996 (Rayner et al., 1996) on OSSEs for atmospheric inversion in the revised manuscript and better highlighted the fact that our manuscript tackles a new and different problem.

C) In Sect. 1, we have added / modified the sentences: "Using synthetic data in an

OSSE framework has been a common way to assess the utility of new GHG observing systems for the monitoring of the GHG sources and sinks at large scales based on global inversion systems with coarse resolution transport models (e.g., Rayner et al., 1996, Houweling et al., 2004, Chevallier et al., 2007, Kadygrov et al., 2009, Hungershoefer et al., 2010). This approach now plays a critical role in the recent emergence of regional inversion systems supporting strategies for the deployment of regional observation networks and assessing the potential of regional inversion for assessing the GHG fluxes at a relatively high resolution (Tolk et al., 2011, Ziehn et al., 2014)."

Q.6) There are many other claims, I did not feel comfortable with (a couple are listed): -Furthermore, its complex terrain also requires a high resolution of the topography when modeling the atmospheric transport (Peters et al., 2010). there are older regional modelling paper papers more appropriate here.

A) We now cite another paper which is more appropriate to discuss this point and which still relates to the transport of CO2 (in line with the topic of the paper): Ahmadov et al. (2009).

Q.7) Broquet et al. (2013) have demonstrated, based on comparisons to independent flux tower measurements, that there is a high confidence in the Bayesian estimate of the European NEE

in statistical sense, yes may be, but not at the level of carbon budget. check out the results of European CO2 fluxes estimated by three of the papers you cite (Roedenbeck, Peters, Chevallier).

A) The statistical consistency found by Broquet et al. (2013) demonstrated the robustness of the monthly mean carbon budgets for Europe that they derived using real data. Therefore, we are not sure to understand this comment.

C) We hope that the new sentence "Indeed, the distributions of the misfits between 1-month and continental scale averages of the flux measurements and of the NEE

estimates sampled at the flux measurement locations revealed to be unbiased and consistent with the estimate of the uncertainties from the inversion system." in Sect. 1 brings a clarification on this topic for the reviewer.

Divergences between the systems of Roedenbeck, Peters, Chevallier are highly connected to the coarse resolution of at least two of these systems which prevents them from targeting the typical scales discussed in our study, using as many site as in Broquet et al. (2013). These systems generally diagnose higher posterior uncertainties in their estimates for Europe than Broquet et al. (2013). So there is a consistency between the confidence the estimates from global / large scale inversions and the spread between them. This is explained in the second paragraph of the introduction in which we had the parenthesis "(which is confirmed when it is diagnosed by the inversion study)".

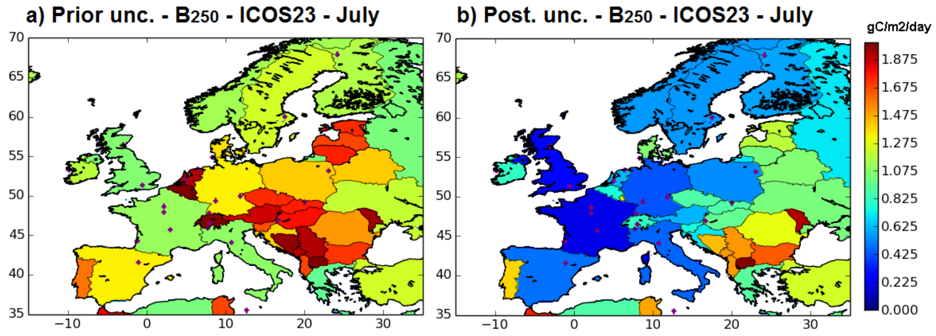
References:

Ahmadov, R., Gerbig, C., Kretschmer, R., Körner, S., Rödenbeck, C., Bousquet, P., and Ramonet, M.: Comparing high resolution WRF-VPRM simulations and two global CO2 transport models with coastal tower measurements of CO2, Biogeosciences, 6, 807-817, doi:10.5194/bg-6-807-2009, 2009.

ESA, Report for Mission Selection: CarbonSat, ESA SP-1330/1, (2 volume series), European Space Agency, Noordwijk, The Netherlands, 2015.

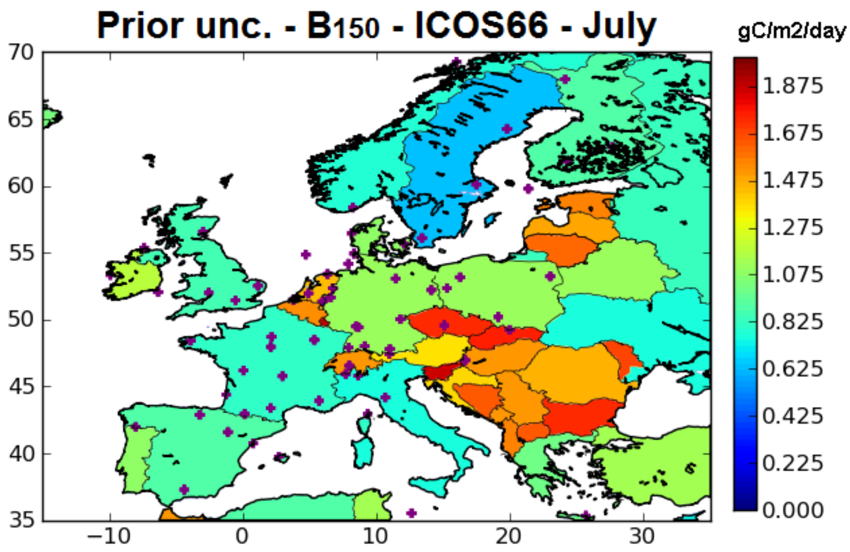
Rayner, P. J., Enting, I.G., and Trudinger, C. M.,: Optimizing the CO2 observing network for constraining sources and sinks, Tellus B, 48(4), 433-444, 1996.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 14221, 2015.



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