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Interactive comment on “The potential for regional-scale bias in top-down CO₂ flux estimates due to atmospheric transport errors” by S. M. Miller et al.

Anonymous Referee #3

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General Comments (GC):

Imperfect of knowledge of atmospheric transport and related mechanisms introduces large uncertainties in the CO₂ flux estimation (via top-down approach), but are often difficult to characterize. This paper investigates on this important aspect and attempts to quantify the impact of transport error on (inverse) flux retrievals globally. The study utilizes a global meteorology model ensemble (CAM) together with a data-assimilation system (LETKF) for estimating CO₂ transport uncertainty. A statistical approach is followed to assess the impact of this transport uncertainty on retrieved fluxes by designing a case study (Case study 1). The second case study is mainly designed to character-

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ize different meteorological parameters/conditions that can influence CO₂ transport at longer time-scales and hence have the potential to bias CO₂ flux estimation derived by the inverse modeling approach. The paper is organized very well and clearly written by linking the concept behind and the outcome. The introduction provides a concise, general background of the topic that nicely gives the appreciation of this work to the wide audience. The chosen methodology, at least a consider part of it, is quite standard and described substantially well. The results obtained are significant and scientifically relevant to the inverse modeling community, although those shown in sec. 3.3 are somewhat based on the detailed characteristics of the followed methodology described in sec(s). 2.3 and 2.4 (see below for the detailed comments). Overall, I find this paper very interesting and useful. However, I find a few major points that need to be clarified or addressed adequately. Therefore, I recommend the paper to be published only after addressing my major concerns as given below.

Major Comments:

About the extent of meteorological uncertainty and the associated CO₂ transport uncertainty:

These uncertainties are calculated based on a coarse resolution meteorological model, which has a spatial resolution of 2.5o longitude \times 1.9o latitude. In the reality, there are other additional error terms introduced due to fine-scale variations that cannot be captured by the coarse model. These additional terms will be more significant depending on the regions and/or periods you sample. The mentioned model ensemble method cannot account for these fine-scale spatial variations, given that the weights (to match the meteorological observations) are estimated for each grid box using observations within a radius about 1500 km. I am not sure how nugget variance (R) is constructed and whether it necessarily represents all errors due to these fine-scale variations. Moreover, I am not much convinced how a single inflation factor for each model grid box works fine for all model parameters. Hence I fear that the values reported for CO₂ transport uncertainty (globally) can be far away from reality. This could

be one of the reasons why Fig.2 does not generally show high transport related uncertainties in the coastal sides (sea/land breeze effects?). The authors may wish to provide more detailed discussion regarding this aspect and it is worthwhile to mention explicitly the significant limitations of this approach.

About the detection limit of the flux biases above the transport uncertainties:

In the given design and set up, I would certainly consider that the flux bias estimations in the case study 1 are overestimated values, because of unrealistically “too strict” constraints. The current inversion approaches followed by many modeling groups take into account the transport uncertainties to some extent and the method is not as simplified as the approach given here. In this aspect, I completely agree with Referee 1 and authors may need to re-design the experiment or rephrase the interpretations.

Differences between monthly mean and 6-horly mean:

I am a bit surprised to see totally different patterns between these two mean values. I could not find very direct and convincing reasons for these differences from the manuscript. Perhaps I missed some details. In that case, the authors may wish to bring this point clearly in the discussion part.

Specific Comments (SC): p.23692, line 13: “.. from surface sources is strong” – “.. from surface sources and sinks is strong”

p. 23696, line 9: “At marine sites, in contrast, the minimum detectable bias is far larger”. Why? transport uncertainties are comparatively shown lower over coastal areas!?

p. 23696, line 11: “.. large sources are better ..” – “.. large sources and sinks are better ..”

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 23681, 2014.

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