We thank the reviewer for her/his comments and recommendations.

In the context of an inversion of atmospheric CO2 data to estimate CO2 fluxes in Europe, Broquet and colleagues assess the uncertainty estimates from the Bayesian inversion by comparing them to misfits between the fluxes and independent flux measurements from eddy covariance sites. As the meaning of Bayesian uncertainty estimates is controversially discussed in the inversion community, this is a relevant and interesting study. Broquet and colleagues present evidence that their Bayesian uncertainty estimates are compatible with the independent data. Method and findings are well described. I would like to recommend publication of this study.

My only concern is the conclusions about the interannual variations (Broquet and colleagues conclude that IAV cannot be estimated reliably in Europe). In contrast to the assessment of the seasonal cycle, I found the IAV discussion much less convincing, either because of the less clear formulations which I may have misunderstood, or because the evidence does not support the conclusion as general as stated (see below for details).

This concern is in line with some comments from the first reviewer and **we will clarify and strengthen the discussion on IAV**. See our answer to the following comments on the IAV but also all the discussions regarding the IAV in our answer to the reviewer #1.

Though the paper reads nicely, a few places may profit from slight reformulation for clarity (see some suggestions below).

Specific comments:

p5775 l 11: re-entering?

we will apply this correction

p5775 l 24: "Bayesian formula" should be briefly explained.

we will modify the sentence to make it easier to understand

p5777 l 3-4: Not fully clear - the temporal correlation refers to corresponding 6hr intervals in consecutive days?

yes, we will clarify it

p5777 l 15-16: Not clear what index i refers to.

it refers to the ith observation, we will clarify it

p5777 l 19-24: Is the same offset used in all yearly inversions? If so, mention explicitly.

yes, we will clarify it

p5777 l 26: Does "synthetic" refer to pseudo-random? If so, it would be important to mention.

yes, we will clarify it

p5779 l 4: Mention here over what the averaging is done (even though it becomes clear later).

We do not speak about the spatial and temporal averaging over Europe and 30-day periods here. **The text will be improved to clarify the fact that we speak about the hourly averaging of the continuous EC measurements.**

p5781 l 4-8: Not sure I understand correctly - is it about the disaggregation of correlated errors?

No, on the opposite, we try to derive an uncertainty for monthly averages "aggregated" over the years,

Consider reformulating.

we will clarify it

p5781 l 13: Not fully clear what "quadratic mean" refers to.

we will clarify the fact that this corresponds to RMS (however, in mathematics, the term quadratic explicitly refers to RMS)

p5781 l 17-21: I did not understand this part (maybe a grammar issue?).

we will clarify it

p5782 l 21: "Consequently" rather than "Subsequently"?

yes, we will apply this correction

p5783 l 27: Does "regular increase" mean "rising trend"?

No, because a trend does not prevent from having "peaks" while we want to point out the fact that the NEE increases systematically from year to year. **We will replace "regular increase" by "increase […] each year".**

p5784 l 25: "positive result" for "passing the test" may be misunderstandable.

ok, we will apply this correction

p5786 l 10-25: The arguments are a bit hard to follow because the numbers are given as Table rather than as Figure. As the IAV topic is important, I would strongly suggest to replace Table 3 by a time series plot.

ok, we will replace the table by a figure with timeseries of annual anomalies

p5787 l 23 - p 5788 l 4: I did not understand at all this paragraph, can you reformulate?

yes, we will clarify it

sect 5.2:

- Is is unclear to me how the monthly uncertainties can be used to judge about IAV, due to possible temporal correlations. (what is the obstacle in directly looking at uncertainties for yearly fluxes?)

In section 5.2, monthly uncertainties are used to judge about the IAV of monthly estimates, not about the IAV of 1-year mean fluxes. **The text will better introduce this distinction.** As explained in section 2 and reminded in section 4.2, we cannot derive posterior uncertainties in 1-year estimate of the fluxes, but the final paragraph of section 5.2 discusses the IAV of 1-year mean fluxes considering the prior uncertainty in 1-year mean fluxes derived from the **B** matrix.

The text needed to be clarified regarding the discussion on the IAV of monthly fluxes based on the analysis of uncertainties at monthly scale (since we cannot rigorously derive an uncertainty in the IAV without information about time correlations for 1-year lags, but since our analysis helps characterize the fact that these correlations are not close to 1), **which will be done in the new manuscript.**

- I did not understand the point raised in lines 23-25.

It will be rephrased for clarification.

- I am puzzled about the finding for 2003, as other inversions do see the reduced uptake during the heat wave.

To our knowledge, state of the art atmospheric inversions do not necessarily point out a positive anomaly in summer 2003 in the part of Europe considered in this study. We could even say that they do not necessarily point it out in the "larger European domain" defined by the TRANSCOM inter-comparison experiment but some of the inversions that identify a positive anomaly in the "TRANSCOM Europe" during summer 2003 do not locate it in the area of our study (by locating it in Russia for example). Note also in *Peylin et al 2013, BGD* (now cited in the text) fig8 that, on average, the state of the art global inversions do not clearly see a positive anomaly for the whole year 2003 in Europe and that the uncertainty in the IAV of these inversions is such that it is hard to derive a robust conclusion regarding the 1-year mean anomaly in 2003 based on these systems (this relates to annual means rather than summer means but this can give an interesting insight about uncertainties in the IAV in summer).

As this signal is directly seen in the data (both concentrations and eddy fluxes, Ciais et al 2005), I find it hard to believe that it was insignificant within the uncertainties.

Actually, Ciais et al 2005 do not analyze concentration measurements. Few low altitude station delivered concentrations on years before 2003 in our domain, which makes the derivation of typical bias to a background station like MHD difficult. Furthermore, interpreting the data without considering the complexity of the atmospheric transport (e.g. of the anomaly in PBLH during summer 2003) may be problematic. Looking at the afternoon (12:00-20:00) mean mixing ratio data for the period June-September, we can see that the gradient between HUN and MHD increased to -0.9ppm in 2003 while ranging between -3.2 and -1.3 ppm for the other years (2002 and 2004 to 2007). However, the gradient at HEI was larger in 2005 (5.5ppm) than in 2003 (5.3ppm) and it ranged from 4 to 5.1ppm for the other years of the period 2002-2007. Therefore, we feel that it is not so easy to detect a positive anomaly in NEE during summer 2003 (which should have mainly impacted the area around France and Germany where the heat wave was the most intense) by looking directly at the mixing ratio data.

Inter-annual anomalies in the mean gradients of atmospheric concentrations are relatively low

compared to the typical prior and posterior misfits between modeled and observed concentrations. This could illustrate, in the observation space, our point on the fact that the IAV of the fluxes is smaller than the posterior uncertainty in these fluxes.

I feel that the conclusion that /no/ IAV can be estimated reliably (as stated in the abstract) is not supported by the findings, and should be formulated much weaker.

We agree about the point about formulating it in a way that is quite weaker than "IAV cannot be derived reliably". **We will rephrase the abstract for moderation. The text will also strengthen (according to the discussions above) the assumption that the confidence in the IAV from existing and state of the art inversions should be low.**

Please, see also all the discussions regarding the IAV in our answer to the reviewer #1.

p5789 l 21: "uncertainty bounds"

we will apply this correction