

## ***Interactive comment on* “Evaluation of various observing systems for the global monitoring of CO<sub>2</sub> surface fluxes” by K. Hungershoefer et al.**

### **Anonymous Referee #2**

Received and published: 3 September 2010

The manuscript "Evaluation of various observing systems for the global monitoring of CO<sub>2</sub> surface fluxes" by K. Hungershoefer et al. presents an analysis of the benefit of various observing systems (ground-based and space-borne) in reducing uncertainties on the inferred surface fluxes. It turns out that an active space-borne CO<sub>2</sub> monitoring mission provides the highest constraint on the surface fluxes, however, with the same funding a theoretical ground-based network could be built with a similar constraint. The achieved uncertainty reductions are set in relation to the actual needs of the scientific community to improve models and of the policy stakeholders for emissions verification.

The manuscript addresses an important issue in the design of future CO<sub>2</sub> observation systems and sets the different concepts in relation to each other. The manuscript is well written and structured. I can recommend the manuscript for publication pending some

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minor modifications. As a general comment, a lot of important details are not mentioned explicitly (for instance for satellite measurements why are CO<sub>2</sub> fluxes inferred from small variations in the column averaged mixing ratio, it is never mentioned before that satellites only measure a CO<sub>2</sub> column mixing ratio), I would think the manuscript could be improved by explaining these details more explicitly.

Detailed comments: Page 18563, line 1: "rising" instead of "raising"

Page 18564, line 12/13: This is currently debated, see for instance Knorr, GRL, 2010.

Page 18565, line 2-4: You mentioned FLUXNET here but you do not use these data in your study at all and you also do not explain how direct carbon flux measurements could be used in inversions.

Page 18565/18566: The potential of the A-SCOPE mission has already been assessed in another study (Kaminski et al, Tellus, 2010), which should be mentioned here as well.

Page 18568, last line and page 18569, first line: Could you explain the justification for using an exponentially decreasing error correlation with a decay time of four weeks? How sensitive are the results to this assumptions?

Page 18570, last line: You should explain why night time measurements are not useable.

Page 18581, line 7-16: I think it would be good to display the extended ground-based networks in a figure.

Page 18572, line 1: Why do you assume that there are large error correlations among the errors?

Page 18572, line 3: How do you define "best" measurement? How do you know that one particular "pseudo" measurement is better than the others? How sensitive are you results against this choice of the best measurement?

Page 18573, line 23-25: I don't think that the measurement error should contain sim-

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ulation errors as well. Usually, measurement error as referred to as the mismatch between the measured value and the "real" value! Consider the case with the satellite observation and the transport simulation having the same, non-zero error, the result would, however, be a measurement error of zero.

Page 18575, line 20: Why do you not consider transport model errors? I think this is an important contribution to the overall error assessment.

Page 18577, line 21: "analytical" instead of "analytic"

Section 4.3: The prior errors are missing here. I think it would be good to set the posterior errors in relation to the prior error structure.

Page 18584, line 21 and in the following: "Representativeness error" sounds awkward; I think the common terminology is "representation error".

Page 18587, line 24 to Page 18588 line 20: This "Land-Vegetation dynamics" section is a bit misleading. Vegetation dynamics is usually understood as the slowly changing components of the terrestrial biosphere such as mortality, succession, and competition and not processes, which operate on a weekly time scale. In fact, it is not clear to me which processes you actually refer to here. Processes regulating the terrestrial CO<sub>2</sub> exchange fluxes operate on a much higher temporal time scale. They vary on a synoptic time scale.

Page 18588, line 25/26: Could you provide a reference for this.

Page 18590: Kyoto protocol verification is very demanding indeed. But how about verifying UNFCCC reporting which considers total emissions and not anthropogenic and natural emissions separately.

Page 18590, line 25-27: Could you elaborate on what kind of systems you are thinking of here.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 18561, 2010.

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