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***Interactive comment on “A comparison of
different inverse carbon flux estimation
approaches for application on a regional domain”
by L. F. Tolk et al.***

Anonymous Referee #1

Received and published: 30 March 2011

This paper addresses an important aspect of inverse estimation of carbon fluxes at regional scales. The authors assess the impact of the choice of the state space on the resulting flux estimates. It is known from global inversion studies that this choice is important, but at the regional scale this has so far been somewhat neglected in the literature. The paper uses a pseudo data experiment, in which pseudo observations have been simulated using a transport model with a biosphere model that is different from the one used for a priori fluxes in the inversion. This is an important aspect, as it takes into account structural differences between modeled and true fluxes.

One minor issue that I have with the paper is that it is not clear what the primary

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intention of the inverse estimate is. Is it for estimating regional budgets in the sense of a “reanalysis”, or is the focus also on predictive capabilities? This should be clearly stated. I find the paper acceptable for publication after addressing this and the more technical issues below.

Detailed comments:

P3361 L19 I would suggest to replace “new biosphere model constants” by “new biosphere model parameter values”

P3364 L3 “a” instead of “an”

P3364 L24 why was a different model-data mismatch used for control and other inversions? This should be explained.

P3365 L19: the term “innovation chi-square” is unknown to the reviewer, probably also to other readers. This term should be introduced.

P3368 L2 the metric RMSD should be introduced (it is introduced a bit late on page 3370)

P3368 L15 it would be helpful if some additional information on the results of the control inversion would be given, for example the range of RMSDs for the best and worst method.

P3369 L14 again some more information on the control inversion results would be helpful

P3371 L6-9 It might not be that simple: the two inversions have an inflated variance, but also more degrees of freedom. It depends on how those different degrees of freedom are “aggregated” when turning fluxes into mixing ratios.

P3371 L17 May be include the control inversion results in Table 4

P3371 L21 move the bracket “(Tables 3 and 4)” to directly after “temporal structure of

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NEE”

P3373 L26 I do not see the necessity to have all three requirements at the same time. A good first guess combined with a small uncertainty could well work in a linearized version

P3374 L3 if the authors have some specific “other observation types” in mind, those should be mentioned

P3374 L15 add “estimation” after “biosphere model parameter”

P3394 Figure 2: letters for different parts of figure missing

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 3355, 2011.

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