

***Interactive comment on* “Spatial and temporal resolution of carbon flux estimates for 1983–2002” by L. M. P. Bruhwiler et al.**

Anonymous Referee #2

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The paper considers the question how well the CO₂ fluxes in different spatial regions, when estimated by an inversion of atmospheric transport, are distinguishable from each other given the data information. This is a very relevant and yet unresolved question. The authors tackle it by looking at the resolution matrix, which is a well-known quantity from theory but, to my knowledge, has not yet been systematically investigated before for the CO₂ inversion problem. The paper in particular considers the important changes in the resolution over a 20-year’s time due to the increasing number of measurement sites. The authors consider this for different levels of spatial aggregation, which allows them to highlight interesting points. The presentation of the resolution matrix in ‘slices’ (Figs 3, 5, 7, 8, 10) is a good way to summarize this large amount of information, especially for comparing the networks.

The major problem of the paper, in my view, however is that the quantification of ‘distinguishability’ remains vague. The authors use judgements like ‘well resolved’ or ‘not very well resolved’ for high(er) or low(er) values of the resolution matrix, but it remains entirely open to which thresholds they compare these values. What do values of, e.g., 0.8 really mean in practice? According to Eq 3, I’d rather guess that 0.8 means a 20% misplacement error - is this well resolved or not? As far as I see, the only thing directly backed up by the resolution matrix as such is statements like ‘better resolved’ or ‘less well resolved’. However, unless more absolute information can be obtained, the discussion of the resolution matrix seems to me of limited utility in practice.

For example, the statement that land total and ocean total can be distinguished from each other is very interesting (in particular because it would contradict common experience) - but at present, I would tend to take it with strong caution. Also, why does the finding for the global scale contradict the finding from the resolution matrix at regional scale (poor resolution there)? In this context I was wondering how exactly the resolution matrix for aggregated regions was calculated. This is not explained in the methods section, but only alluded to in the caption of Fig 3. From what may be guessed from this caption, I would not be sure whether it is actually the correct way. Therefore, please give an explicit formula.

Given the relevance of the topic, I’d like to encourage the authors to resubmit the study for BG, but I feel that considerable revision is necessary. This also refers to the quite long list of smaller errors in writing and reasoning (as detailed below).

Specific comments:

p 4701, line 1: ‘but the inversions are sensitive to the prior estimates of these errors’: it remains ambiguous what you mean here - can you reformulate?

p 4703 line 5: if a site cannot be adequately simulated, how can it nevertheless be used in a monthly inversion?

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p 4705 line 24: ‘nominal uncertainty of 100% of the monthly NEP’: does this imply strong seasonality in uncertainty (proportional to NEP)? What happens then in months with near-neutral flux?

p 4707 line 2: which sites are these difficult ones?

Method section: Which are the source regions for which fluxes are estimated? Are there a-priori correlations between them, or between consecutive month? (this is only said later in Sect. 5, but should be given explicitly in Methods)

sect 4.1: The equation $Q^*H^*(H^*Q^*H')^{-1} * H = I$ would only hold if H was square and non-singular. That is however not the case, due to atmospheric mixing and incomplete sampling by the available sites. For example, if there would be just a single (albeit perfect) measurement, then $R \rightarrow 0$ but still almost nothing would be constrained.

p 4811 line 7ff: If I understand this right, then exchanging n by m in Eq (8) rather shows that the asymmetry only follows from the ratio of the diagonal elements in the prior covariance matrix, instead of giving additional information as claimed. This also means that the conclusion in page 4717 lines 16-21 are actually meaningless.

p 4714 line 1f: ‘this suggests that ... land and ocean cannot be distinguished’ - I did not understand how this would follow from the overlapping sigma-intervals. (also in page 4716 line 26f)

p 4714 line 7 ‘are very small’ - what does small/large mean quantitatively here? What should it be compared to?

p 4715 line 10f: The comparison of global fluxes cannot be done in this way: You also need to add the fossil fuel emissions which are probably different between the studies. I would be extremely surprised if any inversion study would have a significantly different global total flux than others (if time period is the same). The misleading conclusions need to be removed.

p 4715 line 27: Rödenbeck et al actually use an ocean prior with a similar long-term

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flux based on ocean-interior inversions as Jacobson et al.

p 4717 lines 1-6: These are very important points, but only put as possibilities here - what can be done to find out which of these possibilities is the case?

p 4718 lines 14-18: Something seems to be missing in the first half sentence - it is not clear to me what is meant here.

p 4724 lines 20-24: As discussed here, the statement that land and ocean can be distinguished from each other contradicts previous findings. It would be interesting to actually resolve this contradiction.

Technical corrections:

p 4698, line 5: ‘... and we the ...’: something missing here?

p 4703 line 1 ‘...us to *do* many ...’

p 4707 line 5 ‘due *to* ...’

p 4709 Eq 2: ‘(R++HQH)’ - additional ‘+’

Eq 4 and 5: ‘R’ within parantheses should probably not be the curly one (as for the kernel)

p 4714 line 12: ‘*be* seen’

p 4718 line 23: ‘update’ - probably ‘uptake’

p 4721 line 16: ‘are are’

Fig 5, 7: colored bars indicating the month seem to be shifted with respect to the numbers of the x-axis. Also, the numbers are shifted with respect to the tics, and presumably do not match the caption (because the highest bars do not occur at the tic indicated as diagonal element).

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