

Interactive comment on “Design and evaluation of CO₂ observation network to optimize surface CO₂ fluxes in Asia using observation system simulation experiments” by Jun Park and Hyun Mee Kim

Anonymous Referee #1

Received and published: 9 September 2019

This paper describes an evaluation of different observing locations in Asia to infer Asian surface CO₂ fluxes. The authors use the Carbontracker inversion system, with model-generated pseudo-data, to assess different observing networks. They compare fluxes estimated with the existing network, with alternative networks based on random addition or relocation of sites, and the choice of sites using sensitivities from the inversion system. This contrasts with previous network design studies that use optimisation to locate the best observing sites, with higher computational cost.

Some aspects of the methodology need improved description, as described below. I

C1

have some concerns with the methodology, also described below, however it is possible that I have misunderstood what was done and improved description would give me a better understanding of the methodology and address some of my concerns. There is a need for minor improvements to the English throughout, but this would be addressed with copy-editing and I don't believe it has contributed to any difficulty in my understanding of the methodology or results.

Specific comments:

1. Self-sensitivity (page 4, line 6 and section 2.2) - it is not completely clear to me what sensitivity is used in this paper. On page 4, line 8 'the relative impact of each CO₂ observation for the optimized surface carbon flux can be calculated ... and used as a strategy for selecting potential sites of CO₂ mole fraction observations', however on page 8, line 8 'contribution of the observation vector (y^o) to the analysis at the observation space (y^a)'. From the description in the paper, I understand y^a to be the model equivalent of the CO₂ mole fraction in air, also described as the predicted observation in Liu et al (2009) that the authors refer to. These are two different quantities (i.e. sensitivity of fluxes or sensitivity of surface mole fraction). Which was used in this study? I can see value in considering the sensitivity of the optimised flux (or perhaps the scale factor in this study) to each observation, but I am not as clear on the value of the sensitivity of the predicted observation. Of course they are related, but not the same. I am also not clear about how time affects the sensitivities. For example, some information comes from distant sites but with a lag. When is the analysis sensitivity calculated - before lagged information has had a chance to improve an analysis estimate? If so, that would downweight information from other gridcells that arrive after a lag. Thus I have concerns about the methodology, but I admit that it is not clear to me exactly what was done.

2. Simulated hypothetical observations (section 2.3) - there are no details or references given about the EXTASI experiment - are the EXTASI fluxes based on the same flux modules as used in this study but with different scale factors? Therefore, are there

C2

differences in the spatial distribution of fluxes within the ecoregions that are used to generate the simulated hypothetical observations compared to the flux modules, as there would be between modelled fluxes and real world fluxes in an inversion of real observations? This is perhaps most relevant for the two regions that each account for close to 20% of the domain. If the distribution is the same, that's probably ok, but it should be mentioned, as model error in the spatial distribution within each region is not considered.

3. Average of random redistribution (page 11, line 1) - My understanding from the text is that REDIST is created by averaging the fluxes from three random redistribution experiments of 7 sites. Firstly, is this correct? And if it is, I am concerned that this may lead to a better solution than you would expect from just 7 sites, as $7 \times 3 = 21$ sites were actually used to generate the average. Errors in the individual results may cancel in the average. The statistics of the average may not reflect the statistics of individual experiments, and therefore it would be an unfair comparison. The ADD case is also an average of three experiments, so would potentially have the same issue. Perhaps it would be a fairer comparison to instead calculate the PC, BIAS, RMSD and UR statistics for the individual experiments then average these statistics?

4. What affects the self-sensitivity of an individual gridcell in the ALL case? In Fig 6, most of the gridcells with high self-sensitivity are near the boundaries of the regions used for this calculation. Presumably this is because they contain some information not available from neighboring gridcells in the ALL case. But for gridcells with many neighbors that contain similar information to each other, the information from any one of those gridcells may not be needed when all of the others are available, as in the ALL case. But that does not mean that at least some of these gridcells that rank low in the ALL case are unimportant in a case with a much lower number of observing sites. The authors point out on page 17, line 18 that self-sensitivity is generally inversely proportional to the number of assimilated observations in an ecoregion, and that makes sense, but within a region, does the self-sensitivity pick out some sites that will give

C3

most value in a network with few sites, or just those with most sensitivity in a case with many sites (ALL)? In network design studies that use optimisation, the value of observation sites is determined for a network that is closer to the expected size of the potential network. I am not yet convinced of the value of determining the worth of any single site from the self-sensitivity in the ALL case when many more sites than would be practical are included. This is my greatest concern about the methodology, and I believe this would need to be addressed for the paper to be published. Of course, exactly what the self-sensitivity is (sensitivity of fluxes or surface mole fraction) is also important here (see above comment).

Minor points:

5. Page 1, line 10 - "Inverse modeling derives estimated CO₂ mole fractions in the air from calculated surface carbon fluxes using model and observed CO₂ mole fraction data" - No, forward modelling derives CO₂ mole fractions in the air from surface fluxes. Inverse modeling derives surface fluxes from CO₂ mole fractions in the air.

6. Page 2, line 7 - "Inverse modeling uses observation data and transport models to estimate the sources and sinks of surface carbon flux and associated atmospheric CO₂ mole fractions" - better than the previous description, but doesn't specify what observation data are used (should be CO₂ mole fractions in air). The associated modelled atmospheric CO₂ mole fractions can be estimated from the inferred fluxes (or perhaps during the inversion), but I don't consider that part of the inverse calculation.

7. Page 3, line 14 - Add 'alone' after 'data' i.e. Assimilating XCO₂ data alone ...

8. Page 3, line 22 and many other locations - OSSEs (with an 's' at the end) is often used for the plural of OSSE. I.e. We conducted one OSSE, and they conducted many OSSEs.

9. Page 4, line 12 - "which does not seem feasible in the near future" - what is meant here? Is the 43 site network not feasible? Or the 233 site network (is this not like

C4

the ALL case considered here, to see what would be possible with observations everywhere)? Or are the authors referring to the computation of the network design calculation for many sites?

10. Page 4, line 24 - I would add at the end of the sentence ' , as an alternative to optimisation that has been used in previous studies' to make it clear that optimisation is not used in this study. Alternatively (or perhaps in addition), point out clearly elsewhere in the introduction that optimisation of the network is not part of this study, as that point was initially not clear to me. (At page 4, line 3, problems with IO and GA are discussed, but that doesn't mean another optimisation method wasn't going to be used).

11. Page 5, section 2.2 - There are many details of the inversion that are not clear: Does the inversion run globally with a focus on Asia, or just run over Asia as a regional inversion (i.e. are fluxes outside the Asian domain estimated)? How many ecoregions are used in this study? (Is 156 regions a global number or for Asia? What are the 240 ecoregions? There are 40 lines in Table 3, is that the number for Asia? Could say 'We estimate x scale factors for y times'.) Is it possible to include a map of the ecoregions for Asia? How contiguous are the ecoregions?

12. Page 5, line 9 - I would mention up front that the fluxes from the flux modules are scaled, and not wait until line 19. e.g. at line 9 'The estimated surface CO₂ fluxes are mainly calculated by scaling fluxes from the flux modules composed ...'

13. Page 5, line 28 - the sentence that begins 'In addition, also ...' is not clear. It does not say what the model counterparts are. I would replace that sentence with something like 'From this spatiotemporal CO₂ distribution, the model equivalents of atmospheric CO₂ at the times and locations of the observation data can be calculated, and these are used in the data assimilation process.'

14. Page 7, line 17 - I would say 'A statistical method' rather than 'The statistical method', otherwise a reader would wonder which method is 'the' method. I would replace 'feasible' with 'meaningful'.

C5

15. Page 8, line 8 - define y^a (e.g. = Hx^a) and give some information about what it is (e.g. model equivalent of observations, or predicted observation).

16. Page 8, line 12 - replace 'size of observation' with either 'size of the observation vector, n' or 'number of observations, n'. Is that the number of observations at only one time or all times?

17. Page 8, line 25 - do you (and should you) assume no correlations between observation errors? It seems to me that the errors in your simulated data would be correlated, and also likely in the real world.

18. Page 9, line 6 - please give more explanation of what S^o is, e.g. 'In our case, this would be the contribution of a CO₂ observation to the inferred CO₂ at that model gridcell/time' - is that the correct explanation?

19. Page 10, line 2 - please explain 'On the basis of the nautical time zone'. Also explain '13 LST'.

20. Page 10, line 9 - 'Model-data mismatch (MDM) was set to 3' - what does the setting of 3 mean? Is it a setting within Carbontracker, in which case it should be explained.

21. Page 11, line 25 - add 'for observation j' to 'The normalized self-sensitivity for observation j is defined...'

22. Page 12, section 2.4 - define n for equations 16-18.

23. Page 15, line 10 - 'the three experiments show increasing trends' - be careful that this is not misinterpreted as a trend with time. I assume you mean that for RMSD in the summer, CTRL>ADD>ALL? Please clarify what is meant here.

24. Page 16, line 9 - what does 'enabled in the CT2013B framework' mean? There may be a better way to express this.

25. Page 16, line 16 - 'showing the impact of each observation site for the model simulation results' - could you be more specific here about what quantity the impact of

C6

the observation sites is calculated for.

26. Page 17, line 13 and Table 3 - could the ecoregions be described in terms of vegetation types rather than just as a number which may not mean anything to the reader?

27. Page 18, line 8-9 - These sentences are difficult to follow, consider rephrasing without the 'this is in contrast' beginning to each new sentence.

28. Page 19, line 5 - 'because they were derived from an uneven distribution of observation sites' - do you mean an uneven number of sites for each ecoregion?

29. Page 19, line 13 - add 'each' after 'one observation site'

30. Page 20, line 5 - I don't think 'and this is in contrast' is the appropriate wording here.

31. Page 21, line 18 - replace 'below 50°N' with 'north of 50°N'

32. Page 21, line 20 - replace 'slight increases in UR' with 'slightly more UR'

33. Page 21, line 21 - add 'than REDIST' after 'including China and India'.

34. Pable 6 - Bias in Fig 7 looks like it is lower for ADD than SS and ECOSS - is this consistent with the numbers in Table 2? Is the signed biased averaged, or the magnitude?

35. Figs 3m and 6 - the gap in observing sites in Figs 3m and 6 over the Himalayas is presumably due to elevation and therefore practicality of an observing site? Is this worth mentioning?

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-241>, 2019.