EGUsphere, referee comment RC1
https://doi.org/10.5194/egusphere-2022-1019-RC1, 2023

Comment on egusphere-2022-1019

Anonymous Referee #1

Referee comment on "Analysis of high gas concentration and flux measurements at Swiss Beromünster tall tower" by Andreas Plach et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1019-RC1, 2023

General comments

The submitted study presents an analysis of tall-tower concentration and flux measurements in Switzerland. Such towers measuring both trace gas molar fractions and fluxes for several years are quite unique and could indeed provide important additional data to improve regional inversions aiming to constrain regional and national level emissions/removals of GHGs.

The authors furthermore implement an impressive, and in my view defensible, statistical approach to identify "high" concentration and flux values, taking into account seasonal and diurnal variations. After identifying the seasonal-diurnal thresholds for high values, an analysis was conducted explain test whether local, regional or distant sources were responsible for values exceeding 90th percentiles, by splitting the dataset into local, regional and distant subsets, by inferring transport distances from the interplay between mixing height (relative to tower height) and vertical potential temperature gradient. If the subset contains more than 10% of values classified as high, the authors inferred that significant trace gas sources are found within this transport scale.

The paper is well-written and results are well presented; however, I have substantial concerns over the methods used to classify the transport scales and whether such a common approach can be applied to both concentrations and fluxes. Furthermore, while the writing is good and the graphs are sharp and well rendered, I was disappointed that the study was not put into context were a more thorough review of literature in the introduction and then a discussion of what this tower and these results mean within this context. I elaborate on these and other major concerns in the section Specific comments.

Specific Comments

Characterization of transport scales. This constitutes the biggest weakness of the study, which is critical given that it is fundamental to the goals of the analysis. ABL height above the tower height coupled with moderately stable to unstable stratification (Pot T gradient > - 10 K km-1) was considered Regional. Relative to a Distant scale, I can imagine this would be valid for concentrations but not for fluxes. I think within the range of ABL heights and stratification considered here as regional, one would actually have flux footprint ranging over 2 or more orders of magnitude (shorter footprints with high ABLs and/or very unstable conditions; longer footprints with ABLs just above the tower height and/or stable stratification.

Furthermore, for the concentrations, I believe it is not possible to then split the remaining consitions when ABL < Tower height between local and distant scales. I think with ABL below the tower, the scale is distant. In this study, an ABL < tower height coupled with a very strong inversion is considered local. I think here the assumption is that the ABL is in fact below the tower base, so that measurements are only sensitive to sinks/sources on the elevated local terrain around the tower. I can imagine under such a scenario, the concentration oberservations are proportionally more sensitive to local sources than regional ones trapped under the inversion; however, surely this scenario would constitute a local + distant scenario. Moreover, I think the ABL height being below the tower base would be decisive rather than the Pot T gradient at the tower.

Finally, I think the applied scheme ignores important factors affecting fluxes in that low ABL/stable stratification leads to decoupling between the turbulent fluxes and the sources/sinks in the footprint and an associated increase in storage and advection fluxes below receptor height. For example, under the distant subset, one could potentially see more high fluxes at midday not because of distant sources but because the 90th percentile is negative flux value closer to zero, and under such conditions absolute exchange is lower because of the dominating non-turbulent fluxes within the volume below the receptor.

I therefore must express considerable doubts about the validity of this scheme for defining the three transport scales. If the authors were to revise and resubmit the analysis, I would strongly recommend basing this analysis on concentration and flux footprints

The authors mention that follow-up work is planned to model flux and concentration footprints with FLEXPART, which I think would have been very helpful for the aims of the submitted study. Assuming that this work may take time to emerge, perhaps the authors could nonetheless simpler e.g. analytical footprint models to split the data into different transport scales. I think presentation of footprints, even if coarser approximations, of both concentration and flux footprints would be useful to this study.

Lack of context. The Introduction focused too much on previous studies at the Beromunster tower and failed to introduce the context and rationale for the measurements and the analysis. I would have expected the introduction to touch e.g.:

- Need for national and regional networks of in situ trace gas measurements for improved inverse modelling of regional/national fluxes and emissions
  - CH (and UK) as examples of countries establishing national in situ networks for this purpose
- Particular global/regional need for tall-towers…
  - ICOS aims at certain ratio between mountain stations and tall-towers
- Novelty of Beromünster, operationally measuring both trace gas concentrations and fluxes. There are not many others… there are however, some and would be good to name a couple of these examples

After setting the scene, it would then be good to justify why this particular analysis was undertaken.

Additionally, the literature cited in the introduction and discussion was heavily dominated by previous studies of data from Beromünster, with almost no reference to relevant studies from elsewhere.

Reflecting that this information was missing/lacking, the paper therefore does not communicate what authors wanted to do with this analysis and why, and what new insights have been gained. For example, what new insights were gained from correlations between trace gases compared to other studies elsewhere and previous analysis at the tower.

Other specific comments

In addition to the above, I would also highlight the following issues.

- Use of gap-filled EC data. I think such an analysis should be using only measured fluxes and not measured and gap-filled. For a gap-filled data point, the data will not reflect e.g. the transport scale at the time but an average of those data points used to gap-fill
- Show the in the supplements the monthly diurnal curves for trace gas concentrations and fluxes and perhaps show the stats per hour e.g. percentiles and means
- Consider removing H2O conc/fluxes from the analysis or at least make adequate reference to these results

Technical corrections

Given fundamental scientific concerns and my recommendation that the manuscript and the analysis itself be substantially revised, I think it inappropriate to list technical issues

with the manuscript. Furthermore, it is worth repeating that, despite my scientific concerns, the MS is well-written and with good graphical presentation of the results. I did not therefore come across many technical issues such as typos, spelling and grammar mistakes.