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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The CarboCount CH sites: characterization of a dense greenhouse gas observation network" *by* B. Oney et al.

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Anonymous Referee #2
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Review of the manuscript: "The CarboCount CH sites: characterization of a dense greenhouse gas observation network" by B.Oney et al. The paper is describing the new regional network in Swiss Alps of four atmospheric stations for the measurement of CO2 and CH4 mole fractions. It is focused mostly on the characterization of the footprint of each site, using local observations of CO2, CH4 and meteorological parameters, and a high resolution particle dispersion model. The paper is well written with clear figures, even though there are sev-

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eral elements repeated throughout when scanning the four stations. The main limitation of the study is the fact that authors develop only the sensitivity to the atmospheric transport, and not to the surface emissions variabilities. This limitation is mentioned once in the paper but I think the authors should remind it, or discuss it, in the presentation of the results. With this clarification and few minor revisions detailed hereafter I do recommend the manuscript for publication in ACP.

The authors would like to thank anonymous referee #2 for the supportive review and the helpful comments. The reviewer is certainly correct that it is important for the reader of this paper to understand that we investigate (generic) surface sensitivities and not concentration sensitivities to a particular surface flux pattern. We thus aimed to clarify this in the text. At the same time, we would like to point out that the study of surface sensitivity has many merits in its own right, so we don't regard our focus so much as a limitation, but rather actually as a strength of this paper. First, the surface sensitivity is a property of the measurement site (determined by atm. transport and mixing) and is thus independent of the tracer that is being investigated. Second, by avoiding the uncertainties associated with any surface flux data product, we focus on the most robust aspect of the site characterization. Third, by discussing primarily the differences in surface sensitivity across the four sites, we are also less affected by potential biases in the transport model, especially those associated with vertical transport out of the surface layer. In the following, the reviewers comments will be in bold font, and the responses will be in plain font, with suggested new text being quoted.

Abstract: I suggest to add the period of the measurements considered for the paper. I found the period only in Figure 4. I may have missed this information earlier.

The period of measurements considered is described on page 12917, line 8, and figures 4, 9, & 10. We will include the period considered in the abstract as suggested:

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"We describe a new rural network of four densely placed (< 100 km apart), continuous atmospheric carbon (CO_2, CH_4, and CO) measurement sites in north-central Switzerland and analyze its suitability for regional-scale ($\sim$100 to 500 km) carbon flux studies. We characterize each site for the period from March 2013 to February 2014 by analyzing surrounding land cover, observed local meteorology, and sensitivity to surface fluxes, as simulated with the Lagrangian particle dispersion model FLEXPART-COSMO."

Introduction: (p.12914): "It aims to quantify terrestrial carbon fluxes": is the separation of different sources contributions considered in the project ?

The project's goal is to understand terrestrial carbon fluxes; therefore, yes, separation is critical but not the focus of this study. The goal of this study is to consider the trace gas measurements and spatial aspects of surface sensitivity, without relying on the accuracy of surface flux inventories. We will discuss this further on.

(p.12915): "use only afternoon measurements...": this is true for flat terrain, but not for mountain sites for which only nighttime values are generally used (e.g. Schauinsland, Mauna Loa, ...)

Yes, that is true; it will be changed. New paragraph: "Currently, most surface flux estimation studies use only afternoon measurements from sites in flat terrain when the ABL is thickest and well-mixed to reduce the sensitivity to errors in the representation of the ABL by the atmospheric transport models (Gerbig et al., 2009; Pillai et al., 2012; Kretschmer et al., 2014). At mountain sites, conversely, nighttime or early morning measurements above the stable ABL are preferentially used due to the difficulties in correctly representing the daytime convective ABL above steep terrain. In inverse modeling studies using coarse resolution models, measurements from mountain sites are usually discarded all together."

Data methods: (p.12916): Measurement data: please add the date of installation for each station in table 1/ I do not understand why the 'Local site characteristics'

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(par. 3.1) are not included in this description of the sites and instruments. I suggest moving the whole paragraph here.

Yes, that information is missing, and will be included in Table 1 as suggested. Please consider the message of "Local site characteristics" as a description of potential local (< 1 km) influences on greenhouse gas measurements, meteorology and thereby transport. We understand the concerns of referee #2 but we would like to keep the paragraph at this position because it allows discussion of the first two figures, i.e. the site's local surroundings and the introduction of the difference between "model" and "true" topography.

(p.12917): "the anthropogenic and biospheric contributions": As the atmospheric measurements of CO2 and CH4 mole fractions provide only information on the total fluxes, to specifically address the terrestrial component need extra measurements or hypothesis. The separation of terrestrial versus anthropogenic fluxes is not really discussed in the manuscript. For example CO data are not considered. Could you please elaborate your strategy on this issue ?

CO measurements could indeed provide additional insights into the separation between anthropogenic and biospheric signals. However, CO was not measured at all sites and the main aim of this study was to provide a characterization of the network with respect to local meteorological conditions and regional surface sensitivity without addressing in detail the influence of anthropogenic versus biospheric fluxes. This will be covered by a forthcoming publication which will make specific use of the CO measurements to estimate the anthropogenic contribution to CO2. We will change the sentence to: "In order to derive regional signals of the trace gas concentrations at each site, a background concentration was estimated and subtracted from the measurements."

(p.12917): "....an accuracy of $\sim$4 ppbv": is it also true for the HORIBA instrument ? Do you have the same accuracy with HORIBA and PICARRO analyzers ? By

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the way if the CO data are not discussed in the analysis I suggest to remove this details.

Indeed, the HORIBA measurement device does not measure with such precision or accuracy. Here, is intended to briefly document the new measurement devices at all sites and to mention the availability of these measurements. We concede that there is no need for this information if we do not discuss the CO measurements, and will remove this information as suggested.

(p.12920): Land cover dataset: "...to evaluate the sensitivity of the measurement sites to different land cover types (LCT)". As mentioned in the metrics description the comparison of LCTs is made with the hypothesis of "equal surface flux strengths" (p.12922). However I would appreciate that you make it clearer in the discussions about the sensitivity to the different LCTs and to local emissions. I suggest at least to systematically use the term 'potential' sensitivity since it is of course dependent of the intensity of surface fluxes, which also may vary a lot in space and time (both at seasonal at diurnal scales).

The expected concentration signal is indeed a product of sensitivity times flux, but the sensitivity is independent of the actual flux and is not a "potential" sensitivity. However, we agree that the way the sensitivity C_{LCT} is currently discussed in Section 3.3.2 is prone to misunderstanding. We will change the text so that it is clear that the influence or contribution of a certain land cover type is potential. We will change all instances where the influence or contribution is discussed to "potential influence" or "potential contribution." Planned changes:

(P. 12922) With equal surface flux strengths, each LCT would contribute the respective fraction C_{LCT} of the observed signal.

(P. 12931, starting line 23) "Potential monthly land cover type (LCT) contributions C_{LCT} vary little throughout the year, and on average reflect the typical land cover for Switzer-

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land and Central Europe (Fig. 10). At all sites, the arriving air parcels spent about $\sim$30 % of the time directly above forest LCTs, and spent about $\sim$50–60 % of the time above crop and grassland LCTs combined. For example in Fig. 10**A**, air parcels, which were observed during the month of March 2013 at Beromünster and were in contact with the surface (< 100 m a.m.g.), spent approximately 20 % of the time over the evergreen forest LCT, 10 % of the time over the deciduous forest LCT, 20 % over the grassland LCT, and 30 % over the crop LCT."

(P. 12933) Beromünster exhibits high sensitivity to grasslands, which, along with being located in an intense dairy farming area (Hiller et al., 2014), would potentially increase influence of agricultural methane emissions.

(P. 12933) Similar to Beromünster, Früebüel exhibits high sensitivity to grasslands (Fig. 10), and therefore the influence of methane emissions may be increased (Hiller et al., 2014).

(p.12926): "...respired $\triangle$CO2": you cannot exclude a contribution from combustion sources. CO variation could be interesting as a tracer of combustion. By the way the CH4 shows the same signal so it cannot be attributed only to respired CO2.

We agree that it is speculative to attribute the strong measured signals to respiration fluxes. Indeed, CH4 shows the same behavior, and the observed variability in both CH4 and CO2 is therefore likely due to ABL dynamics. The suggestion makes the need obvious to combine the following sentence and reads: "The nighttime increase of more than 60 ppmv suggests rapid accumulation of ΔCO_2 in the shallow nocturnal boundary layer, and although nocturnal regional advection of ΔCO_2 may also contribute to the nighttime enrichment (Eugster and Siegrist, 2000), low windspeeds at night suggest that the surface influence is limited to a few tens of kilometers from the site."

Results: (p.12929): high cattle density: the attribution of the summertime CH4 high values both at Beromünster and Früebüel, to this specific source appears

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very speculative.

We agree that it is somewhat (not very) speculative without full simulation of methane concentrations, but it fits nonetheless with the surroundings. Reference is made to Hiller et al. (2014), who state that the majority ($> 80\,\%$) of all Swiss methane emissions comes from ruminant agriculture, and the two sites Beromünster and Früebüel are both located in regions of high cattle density (see Figure 2a in Hiller et al. 2014). Therefore, we think that it is reasonable to assume that the observations correspond to the measurement site's location.

(p.12930): Gimmiz: I agree that the high values and variations observed at this station are "difficult to understand". I am also surprised that you do not use the CO observations as a tracer of anthropogenic sources versus biogenic ones.

Because we intend to remove all reference to the CO measurements, we will revise this. Revised text: "The large diurnal variation in both of the observed regional signals at Gimmiz is suprising and suggests strong accumulation at nighttime in a stable nocturnal boundary layer."

Figure 3: I suggest to remove Goggle aerial phot which do not bring any additional information.

The aerial photograph helps to visualize the idea conveyed on page 12925, starting on line 5. We think it is justified to highlight the challenge of modeling fluxes and resulting concentration signals in highly fragmented landscapes. This challenge is best noted with an aerial photograph. Therefore, we would like to keep the aerial plots.

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