

Response to interactive comments of reviewer 2 (bg-2018-205-RC2)

We provide the reviewer's comments and critique in blue and provide our response in black. All pages and lines are from the revised manuscript unless otherwise stated.

The paper is a very important contribution to the discussion on climate mitigation in agriculture even though it covers data only from one site. The site Oensingen has been monitored since 2003 including eddy covariance and soil carbon stocks inventories. Most important for the net C balance of the site is the import of C via organic amendments and the export with harvest. These fluxes are often ignored or insufficiently quantified. In contrast this study presents the full balance including all fluxes. It is written very well and provides a unique data set and valuable conclusions on cover crops and organic fertilisation effect on the C balance of croplands.

Thank you very much for this positive assessment.

There are only two major aspects that should be considered to improve the paper:
1.) Cover crops are presented (e.g. in table 2 and Fig. 4) similar to other crop types even though they are not similar, since they are only grown during autumn and winter season. This should be made clear to the reader at any point (e.g. by adding "winter season" in figure legends).

We added: "Please note that cover crops were only grown during autumn and winter" to the corresponding figure and table legends. (Figure 4, Table 2 and Figure D1)

Also on page 14, in l. 12 I recommend to rephrase to: "Their relatively large C loss to the atmosphere was thus a result of the winter growing season and not of the crop type". On p. 14 l. 3: Do not merge peas and cover crops in one sentence since they are not comparable. P. 15 l. 14: Rephrase to "During the winter season with cover crops there was always a net C loss. This loss. . .". Add on p. 10 in l. 15 "of net CO₂ loss during winter season". I would also recommend changing the order of crop types in all relevant figures and tables and putting cover crops separately at the end.

We made all the textual changes as suggested by the reviewer:

"Their relatively large C loss to the atmosphere was thus a result of the winter growing season, not of the crop type and was strongly compensated by the application of solid manure." (P. 14, l. 12-14)

"Peas assimilated less C from the atmosphere than the ecosystem released at the same time, and very little was exported from the field during harvest. Also during winter seasons with cover crops, more CO₂ was lost to the atmosphere than was taken up by the ecosystem." (P. 14, l. 2-5)

"During the winter seasons with cover crops, there was always a net C loss." (P. 15, l. 4)

"Cover crops were only growing in the autumn resulting in a relatively weak CO₂ uptake followed by a relatively long period of net CO₂ loss during winter season." (P. 10, l. 12-13)

We also changed the order of the crop species in Figure 4 and C1 and Table 2 so that cover crops appear at the end.

2.) There is a discussion on organic fertilisers to compensate soil C losses in agriculture since they can be so called external C inputs (see Powlson, Whitmore et al. European Journal of Soil Science 2011). If the organic fertiliser is made of biomass that is mainly produced outside the field side where it is applied, we do not gain a climate mitigation effect. Thus, it would be important to estimate from straw and yield export of the Oensingen site how much of this exported C is fed to animals or mixed with manure and afterwards returned to the field site. Is it a closed cycle? You recommend increasing the use of organic fertilisers (e.g. p. 18. L. 18). However, you do not mention how much organic fertiliser is produced from biomass from the site or maybe available at regional scale. Also the recommendation for more compost application as organic amendments (p.17. l.5) should be encompassed with data on how much compost is regionally available.

As also mentioned in the responses to the other reviewer's comments, in general, we would like to focus on the relevance of C for the soil fertility. We rearranged the text to focus more on this aspect. However the potential to compensate C losses to the atmosphere is of course also interesting. We agree that importing manure does not necessarily result in an overall CO₂ sequestration because it might be missing somewhere else. This is a very interesting point, however, it would require a complete life cycle assessment which goes beyond the scope of this study. We do not have all the numbers to assess the life cycle of all C adequately. This is a highly interesting research topic on its own and would require an additional study, beyond the scope of this paper. However we provided some information to estimate the potential.

Compost is not a limiting factor in the region. For example, the composting plant Ricoter, which is located relatively close to the field site (in Aarberg, at 45 km distance) produces 200 000 m³ per year recycled soil substrate from compost (approximately 160 000 t). Recycled soil substrate consists to a major part of compost mixed with low-organic substance soils from construction sites. Several other composting plants exist close to the field site. In total, there exist 368 composting plants in Switzerland. At some of the sites, it is even the policy to offer the recycled soil substrate freely to farmers who want to ameliorate their fields (e.g. Allmig near Baar, Switzerland). It is thus more a question on the carbon footprint of transporting such recycled soil substrate to a given site, i.e. a full life cycle impact assessment, not primarily a question of the availability of the material.

We implemented the following textual changes:

We changed the first sentence in the section to:

"The more frequent use of solid manure could compensate at least partly the C losses of the crop field and decrease or prevent the loss of soil fertility." (P. 15, l. 23-24)

We deleted the sentence:

“However, Switzerland’s nationally determined contribution (NDC) to the reduction in greenhouse gas emissions assumes zero emissions from non-forest lands like croplands (NDC, 2017)” (in first submission version, P. 15, 31-32)

We added the following paragraph:

“Switzerland’s nationally determined contribution (NDC) to the reduction in greenhouse gas emissions lists zero emissions from non-forest lands like croplands (NDC, 2017). Therefore, the C losses should be reduced from a climate change point of view. The use of organic fertilisers could help get closer to the set goal. In the case of CH-Oe2, the grains, peas and potatoes were not used to feed animals on the same farm. However, straw produced on the field at a rate of $78 \text{ g C m}^{-2} \text{ year}^{-1}$ (1013 gC m^{-2} in total during the 13 years of measurements) is used on the farm. If this straw would have been added back to the field (either directly or included in solid manure), it could have compensated a fraction of the C losses over the 13 years. Ammann et al. (2007) studied the C exchange of the neighboring grassland managed by the same farm. Intensive management of the grassland fertilised with a mixture of solid and liquid manure from the same farm resulted in a significant uptake of C. Because the grassland was a C sink it could have been considered to apply the manure to CH-Oe2 instead to counteract the higher C loss of the arable field. Therefore, we assume that there is a potential to decrease the field’s C losses substantially by increasing the application of the farm’s own solid manure. In order to determine if the application of manure would improve the greenhouse gas budget of the cropland as listed by Switzerland’s NDC, it would require a complete life cycle assessment which goes beyond the scope of this study (P. 17, l. 9-21)

Minor remarks

P. 1, l.2: “intensively managed”

We changed this as suggested. (P. 1, l. 2-3)

P. 1, l.7: Mention here that NEE derived NBP is not equal to the net C balance since non CO₂ fluxes (DOC etc.) have to be included but are often very small.

We added the information by changing the sentence on P.1, l. 11-12 to: “Net biome production, representing the overall C budget (assuming carbon leaching to groundwater to be negligible),...”

P.2, l.27-30 can be removed since they only contain large lists of existing studies (blowing the reference list) without referring to the content and finding of these studies.

To some of the studies we refer to also later, and hence in the introduction we feel that being inclusive in the list of existing studies that we considered in our own work is not a bad thing. The Biogeosciences guidelines do not specify an upper limit for references, hence we decided to keep the list of references in the introduction.

P.5, l. 1: The type and company that produced the camera is no required information to understand or potentially reproduce this study. Please remove it.

We removed the type and company name of the camera.

P.5, l.25: Please provide the physically plausible range that was used as quality criteria (-50 to +50?).

The plausible range is already mentioned right at the end of this sentence in brackets: "Fluxes were rejected from further analyses when they were outside a physically plausible range ($\pm 50 \mu\text{mol m}^{-2} \text{s}^{-1}$)" (P.6, l. 13; in first submission version P. 5, l. 26)

P5, l. 31: How was CO₂ storage measured or calculated? Please explain.

We added: "CO₂ storage in the air layer below the flux measurement height was calculated according to Aubinet et al. (2001) within EddyPro." (P. 6, l. 17-18)

P.7, l. 1: How was the u* filter determined? Which criteria were used to keep it dynamic?

We added the following sentence:

"The u* threshold was automatically determined for each bare soil period and growing period separately within REddyProc by determining the saturation of NEE with u*." (P. 6, l. 23-25)

P.7, l. 28: Provide the depth for which bulk density was determined.

We added: "Additionally, bulk density of the soil was determined for a 4-cm deep core within the top 12 cm of the soil..." (P. 7, l. 18-19)

P. 7, l. 30: Explain why soil sampling was interfered by liquid manure application and what could be the consequences for the obtained soil C and N data.

Sampling dates before and after the application of liquid manure were chosen to test if it would change the soil C and N significantly. We added this information to the text: "Sampling dates before and after the application of liquid manure were chosen to test if it changed the soil C and N significantly." (P. 7, l. 21-22)

On P. 11, l. 2-3 (P. 11, l. 20-21 of first submission version), we already stated that the application of liquid manure did not result in a significant change in C and N in the soil.

P. 8 Eq. 1: Add "+N" with N comprising all non CO₂ losses as mentioned on the next page. Is there any estimation of the DOC export flux for the site in order to show that it is small and can be ignored?

We added the term I_{other} comprising all other pathways to Eq. 1.

We added the following text:

"The term I_{other} can be relevant in rice paddies, where methane fluxes are important (Nishimura et al., 2008) and at sites, where substantial losses via volatile organic

compounds (VOC) or dissolved organic carbon losses (DOC) have to be taken into account. At the CH-Oe2 site, however, neither of these fluxes is of relevant magnitude, and I_{other} can be neglected. While VOC emissions (methanol) had been investigated at the nearby CH-Oe1 grassland site (Brunner et al., 2007) and were found to be very small compared to CO_2 fluxes, no estimates were done for DOC at CH-Oe2 so far. A dye tracer experiment by Alaoui and Goetz (2008) at CH-Oe2, however, indicated that the high clay content actually limits the leakage to lower soil layers well beyond the ploughing depth, hence we do not account for potential DOC losses. “ (P. 8, l. 24-31)

P. 9, l. 8: Soil-Plant-Atmosphere Crop Model

We changed this as suggested. (P. 9, l. 6)

P. 10, l. 3: Did the season starts with sawing or with January? How is it defined?

We defined the crop season on P. 3, l. 32 (P. 5, l. 6 of first submission version). Additionally to this, we added “...(defined from sowing of one crop to the sowing of the following crop)...” to P. 10, l. 1.

P. 10, l. 8: Voluntary growth???

Voluntary regrowth is the term that is usually used for the practice to let the recently harvested crop regrow on the field before ploughing them under in the fall. The term is also an important aspect in ICOS ancillary data (e.g. <http://www.icos-belgium.be/files/ICOS%20ancillary%20data%20workshop%20-%20Croplands.pdf>). We thus decided to keep this terminology.

Fig. 3: Please add the estimated soil carbon density for the topsoil for the 2004 sampling in this figure.

We changed this as suggested.

P. 12, l. 2-3: Please provide uncertainties for the relative C loss numbers.

We added the requested uncertainties: “Depending on the measurement method, the field lost 15.7 ± 4.0 % (based on NBPcum and soil C stock of top 30 cm in 2004) to 18.5 ± 5.3 % (based on soil C stocks in 2004 and 2017) of C over the 13 years. “ (P. 11, l. 6-8)

P. 12, l. 6: The number in brackets “(138. . .” is unclear what is it referring to. Please also consider to compare your mean NBP with data from Kutsch et al 2010 and Schulze et al 2009 or data from cropland soil inventories provided by Ciais et al. 2010, GCB.

We changed the sentence to clarify that the number in brackets corresponds to the annual net biome production:

“Ceschia et al. (2010) studied the annual NBP ($138 \pm 239 \text{ g C m}^{-2} \text{ year}^{-1}$, they call it net ecosystem C budget) and the annual changes in soil C stocks of the top 30 cm ($2.4 \pm 4.7 \text{ \% year}^{-1}$) ...” (P. 12, l.1-2)

We also added the results of the other three publications as suggested by the reviewer:

“Kutsch et al. (2010) determined an average annual NBP of $95 \pm 87 \text{ g C m}^{-2}$ for 5 crop rotation sites and 2 monoculture sites.” (P. 12, l. 5-6)

“On the other hand, a study using on a process-based model and soil C inventories (Ciais et al., 2010) and a study using a combination of ecosystem measurements combined with atmospheric greenhouse gas measurements and an inversion model (Schulze et al., 2009) found an average annual source of 8.3 ± 13 to $13 \pm 33 \text{ g C m}^{-2} \text{ year}^{-1}$ and $10 \pm 9 \text{ g C m}^{-2} \text{ year}^{-1}$, respectively for croplands. In our study, the ...” (P. 12, l. 9-12)

P. 13, l. 9: More work on the uncertainties of the flux components would very much improve the paper. Could you provide estimates for the uncertainties instead of only stating “they can be assumed to be similar”?

We added an estimate of the uncertainties of NBP based on Eq. 1 in the appendix (P. 19, l. 4-21) and add a summary sentence to the main text:

“An uncertainty estimate of NBP calculated with Eq. 1 can be found in Appendix A. In total, the uncertainty adds up to a maximum uncertainty of approximately $\pm 25 \text{ \%}$ of NBPcum. Buysse et al. (2017) listed in detail the uncertainties involved in the different NBP terms, which added up to an uncertainty of 220 g C m^{-2} over the 12 years of their study (at NBP = 990 g C m^{-2}) corresponding to an uncertainty of 22 \% .” (P. 12, l. 16-19)

P. 15, l. 4: “was again comparable” – comparable to what?

We reworded this sentence the following way: “For peas, NEE was comparable to NBP because the export with the harvest was much smaller than for all other harvested crops.” (P. 14, l. 29-30)

P. 15 l. 32: “promised” instead of “assumes” ?!

We prefer the term “lists”. (P. 17, l. 9)

Tab. 3: This is a table which I do not understand. First, C_{org} is no nutrient and should be removed from this table. Where do the amounts of solid manure and manure come from? Is it annual? If yes, the unit should be $\text{Mg ha}^{-1} \text{ a}^{-1}$.

We removed C_{org} from the table. We changed the caption and the units to make clear that these are annual values. It is already stated in the caption that the amounts for manure are based on table D1, which is based on lab analysis of manure at our field site.

P. 18: I. 7: Please take into account the uncertainty of the numbers and do not provide them with one digit (since they cannot be determined with such a high accuracy).

We changed this as suggested. (P. 18, I. 7)