

Interactive comment on “Seasonal and diurnal variation in CO fluxes from an agricultural bioenergy crop” by M. Pihlatie et al.

Anonymous Referee #1

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General comments:

The paper is well written and is the first to show the use of the eddy covariance technique for CO flux measurements. It is interesting to see a CO flux dataset with a high temporal resolution over 9 months, which is novel. Quite some studies have shown a diurnal CO cycle before but, as they say, this study is the first one to study the change in diurnal cycle over several months. The paper gives a good overview of how CO fluxes can be predicted or modeled by showing correlation matrices for many different variables, and by showing how the correlation matrices are changing over the season. This is for example very useful information for climate and carbon transport models and therefore a valuable dataset.

However, the interpretation of the dataset is at some points weak. With the Eddy Co-

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variance measurements, they try to answer process level based questions, which their dataset is not fully suitable for. For example, the measured EC CO fluxes are a net result (sum) of several processes (uptake by soil, production by soil, production by dead organic matter and production by living plants) and each of these uptake and emission processes have their own dependencies on environmental variables. While not being able to separate these sources (and their mechanisms and dependencies), still process level based questions are tried to be answered by use of best fitting correlation matrices, and by use of several assumptions (for example the assumption of stable soil CO uptake). When the paper wants to focus on process level based questions, this approach and its considerations and restrictions should be discussed in more detail. Also some other parts of the dataset interpretation and dataset explanation need some more work. Furthermore, by discussing the limitations of the interpretation part, they can determine interesting (process level) research topics/setups for future CO flux studies, thereby contributing ideas for future CO flux studies.

In general, I would consider this a nice dataset which should be published. However, as said, the interpretation of this dataset is at some points weak and needs to be worked on. The points which should be revised or rewritten are more elaborately described in the 'specific comments' section.

Specific comments:

The terms photodegradation, thermal degradation and abiotic degradation are not always used with care. With the EC method it is hard to separate different uptake and emission processes. Based on correlation coefficients, they conclude that radiation is the main driving factor of CO emission. This conclusion cannot be made based on their data. Radiation has many indirect effects such as on temperature, biological activity, etc. From their data it is hard to conclude whether direct photodegradation, or indirect effects of radiation (such as indirect photodegradation (fragmentation of organic matter) or thermal degradation) are the main cause of the CO production. In some places in the paper, this is well acknowledged (page 11, line 4-8). In other places

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this is neglected and the statistical correlation to radiation is given as a proof for direct photodegradation being the main cause. The difference between direct and indirect photodegradation should be explained, and conclusions on this subject should be formulated more carefully.

They make an important (risky) assumption by saying that biological soil CO uptake is constant, based on the paper of Conrad & Seiler (1985). However, other CO flux studies have observed the typical biological temperature response wherein biological activity increases with temperature (for example: Ingersoll 1973, Whalen & Reeburgh (2001), others). Also, especially in cold ecosystems, a small temperature change usually influences biological rates significantly. The assumed stable soil CO uptake assumption in this ecosystem seems unlikely. With the current dataset, this assumption cannot be validated or falsified. So, the authors should reconsider this assumption and think of the consequences if there is a typical temperature response, for example as found by Whalen & Reeburgh (2001), with a Q10 of 2.0. How would this influence their main conclusions? Either this possibility should be discussed, or the 'stable production' assumption should be removed from their manuscript.

Concerning possible biological CO production mechanisms, they hypothesize that CO emissions are not driven by microbial activity. While it is likely that the observed CO emissions are not driven by microbial activity, the used argumentation might be misleading: they base it on the poor correlation between FCO and FN₂O, and the poor correlation between FCO and RESP. However, FCO is a net flux (sum) of uptake and production, while RESP and FN₂O are solely production fluxes. This makes the validity of the correlation questionable. Also, in case the CO production is caused by biological as well as by abiotic sources, would this not result in the same poor correlation between FCO and FN₂O? With the current dataset, it is difficult to determine whether biological fluxes are present, but saying that the poor correlation indicates the absence of biological sources might be misleading. In previous studies, what are the magnitudes of the reported biological fluxes? In this ecosystem, would they have the same magni-

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tude? Are they also expected in autumn when vegetation is less active/dormant/etc? If the authors believe that biological fluxes play a (small) role, it would be good to spend some sentences on the assumed mechanisms and maybe indicate the magnitude of the observed biological fluxes in other studies as a comparison.

In the discussion (page 11, line 21-25), they use the high C to N ratio to confirm their theory that photodegradation is the main cause. However, this argument is not explained. Why does a high C to N ratio confirm the hypothesis?

The comparison to other variables (NEE; heat and energy flux) is stated as a goal in the last paragraph of the introduction, but it is not well explained what is expected. Also, in general the comparison is held very small, especially for the N₂O fluxes. The results are only shown in a table but not discussed, and the N₂O fluxes are hardly mentioned in the Discussion and forgotten in the Conclusion. For the N₂O results, the reader is referred to another paper. If the authors think that the N₂O story is an important part, since they state their interest in the introduction, they should show some N₂O results, interpret these results, and spend some text on why they expect a correlation. Does a N₂O figure maybe fit in Figure 1? Or, if they prefer to refer to the other paper, please then describe the N₂O fluxes (magnitude and diurnal variation) briefly in this paper.

Looking at figure 2 and 3, they correctly conclude that not all CO fluxes can be initiated by radiation, since CO fluxes are already increasing when the sun is still down (in autumn), and they refer to the possibility of thermal degradation. With the assumption of stable soil CO uptake during the dark hours, and with the idea of thermal degradation being responsible for the increasing CO production during the morning hours in the dark, is it possible to (roughly) estimate how much thermal degradation is contributing to total CO fluxes? Can this be extrapolated to the day? And does this estimate change when there is no stable soil CO uptake assumed?

The sampling line material is made from PTFE, which is reported to be inert. However, was the whole sampling set up made of PTFE (from inlet to instrument)? Other mate-

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rials are known to be possible strong CO emitters, and previous CO flux studies have found CO producing material in setups during blank tests (Schade 1999, van Asperen 2015). Has the setup been tested for internal CO production and have blank tests been performed? If so, please mention this. If there is internal CO emission, it probably won't influence your results largely due to the large sampling flow, but if possible, it would be the best to quantify this.

On Page 6, line 26-27, you estimate that the site is a net sink of CO for the 9 months, which is nicely shown in Table 1. Concerning that you seem to have a good idea of which environmental variables are important per time of the season, and that you are the first one to show a dataset with such high temporal resolution for 9 months, is it possible to give an estimate of the net CO fluxes for the other 3 months, so you can give an annual estimate? Such as done in Table 6 or 7, in the paper of Ingersoll (Soil's potential as a sink for atmospheric carbon monoxide, 1973). That would be an interesting addition.

Technical corrections:

General: - Please check your references, for example, Lee (2012) and Zahniser (2009) are missing in the reference list, but maybe there are more. - For many units in different places in the manuscript (ha⁻¹, m⁻²), the 'superscript' is not used. - The hyphen is not used consistently throughout the manuscript - Different places in the manuscript: Please use the same term for G (ground heat flux or soil heat flux)

Text: - Page 2, line 2: of a strong greenhouse gas → of the strong greenhouse gas.
- Page 2, line 12-17: quite some references are named in the different places in the manuscript, which are not named in this part. It might be nice if the references which are coming back in table 3, also come back here in the right place. - Page 2, line 18: Here is stated that CO emissions are thermal or UV-induced. However, it is not only by UV, also by visible radiation, as shown by Lee (2012). Lee (2012) is mentioned on page 9, line 21, but does not appear in reference list. - Page 2, line 22: Most of the reported

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CO flux measurements are either short-term field experiments from.... it seems that the author wants to make a point here that no CO measurements are made so far in this cropland boreal ecosystem, or are only made short-term. But neither of the points is clearly made. Is this the first measurement in this ecosystem? Or the first long term? And can there be an indication for which percentage of land use/Finland/boreal zone this ecosystem is representative for? (see paper Ingersoll, 1973, table 6,7) - Page 3, line 20: The footprint length is given, and the size of the field is given. I assume the author wants to implicate that the footprint is homogeneous in all directions, but this is not stated. Clarify for the reader. - Page 3, line 20: A 6.3 ha field is introduced, is this the same field as meant in the rest of §2.1, before this sentence? Not clear formulated. - Page 4, line 16: No white space between '(see Fig.1c)' and 'Considering'. - Page 4, line 15-20: it is stated that in the majority of the measurements is representative for the RCG canopy. What happened to the minority of the data when it is not? Can this be mentioned? - Page 4, line 25: Reference Zahniser is also missing. - Page 4, line 24-26: Unclear and incorrect sentences, please rephrase. - Page 5, line 7: PTFE lines were used, which are under most conditions inert. However, other parts of the used material might not be. Has there been a blank measurement? If so, this should be mentioned. - Page 5, line 11: The measurement position of G and Tsoil etc is not named in the 'Material and Methods', although partly named later in the manuscript. - Page 5, line 21: a verb is missing. Do you mean: LGR-CWQCL measurements were corrected for..... - Page 5, line 22: unclear sentence. Do you mean: the same applied to the AR-CWQCL measurements after software update in July 2011. - Page 6, line 15: while the length of periods were → while the lengths of the periods were. - Page 6, line 25: to the mid-June→ to mid-June. - Page 7, line 15: near constant CO uptake, is the value you found similar to other studies? - Page 7, line 25-26: please mentioned '(days 110-145)' after 'during the spring'. - Page 8, line 1: Here the discussion suddenly jumps from CO to CO₂, maybe introduce this a little clearer. - Page 8, line 1: Have LAI and GAI been introduced before? - Page 8, line 1-5: N₂O is not discussed at all here. - Page 9, line 2-4: different type of ecosystems are compared here, but they lay

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in different climate zone. Does this comparison make sense then? - Page 11, line 1: suggestion: we expect that radiation→ we expect that the effects of radiation. - Page 11, line 3: T soil at a depth of 2.5cm→ this should also be in materials and methods. - Page 11, line 24: mean and stdev are mentioned, however, the mean value is missing. - Page 11, line 26: the early summer emission→ the early summer CO emission.

Tables & Figures: In Table 3, a nice overview is given of previous CO studies. The fluxes which are reported here, are that daily averages? Several of these studies have also measured a daily cycle. Can the magnitude of these results be indicated? That might make the overview more complete.

In Figure 4, NEE is mentioned. I assume this measured by the EC measurements of CO₂? Maybe mention the trace gas in the caption, also in other places in the manuscript when mentioning NEE.

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