

Interactive comment on “UV-induced carbon monoxide emission from living vegetation” by D. Bruhn et al.

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Received and published: 5 September 2013

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Anonymous Referee #2 Received and published: 31 July 2013 The paper presents data on the influence of UV radiation on CO emissions from living vegetation. The short paper has a simple message, but for such a short paper I found it extremely difficult to read (and review) due to two issues:

1) The presentation is very short and in many cases necessary details are not given (see below).

- Based on previous revisions, we were invited by editor to resubmit the contributions

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in a short paper format following significant reductions in content. The manuscript is formatted according to journal guidelines, and we believe that revision to a general paper will add little to the communication. Rather, we attempt to clarify unclear text, but maintaining the short format as previously advised. .

2) The constant mixing-up of presentation of results and discussion in individual paragraphs makes it very difficult to grasp the full picture. I strongly recommend separating the observations (results) from the interpretation (discussion). In this way, it is also possible to present and discuss the flux chamber and leaf chamber results together, which would make the paper much better readable. - It is an option when submitting to BGD that results and discussion is mixed. This option was agreed upon by the authors and not previously affected by internal and external reviewers, nor by editor. Therefore, we have decided to maintain the current format.

The introduction is very short and would benefit from some more motivation on the CO budget and UV related emissions of trace gases in general.

- We have given an introduction to why it is important to investigate UV-induced CO emission from living vegetation, which is the topic of the manuscript.

The discussion on the relevance for the global scale is very rudimentary, it is not what you expect from this title of the section. So what is the relevance? It is not even mentioned in this section whether it could be relevant or not.

- We have changed the title of this section to clarify that we do not attempt to scale up the observations, but rather indicate that an unknown component is missing in the global CO budget, namely the UV-induced emission from live plants. .

The OH effect is presented poorly.

- The intention was to emphasize that during measurements we did not control OH and that at a global scale, the presence of OH does not eliminate the importance of investigating UV-induced CO emission from living vegetation. This has been clarified

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in the text by omitting the confusing parts.

Introduction: a) The relevance of CO emissions from vegetation to the global CO budget is not made clear, because even a short review of the global CO budget is missing. Please provide the total CO source strength, and cite some papers on the global CO cycle, e.g. Novelli, P. C., Masarie, K. A., Lang, P. M., Hall, B. D., Myers, R. C., and Elkins, J.W.: Reanalysis of tropospheric CO trends: Effects of the 1997-1998 wildfires, *J. Geophys. Res.*, 108, 4464, doi:4410.1029/2002JD00303, 2003. Or others, Khalil & Rasmussen, or a global CO model study.

We have now cited IPCC 2001 for the global net burden of CO.

- b) some references are not entirely adequate: It would be better to cite Logan, J. A., Prather, M. J., Wofsy, S. C., and McElroy, M. B.: Tropospheric chemistry: A global perspective, *J. Geophys. Res.*, 86, 7210-7254, 1981. instead of the soil modeling paper by Potter in the first sentence. Also, Schade, 1996b is not the best paper to cite to highlight that CO is "an important trace gas in the atmosphere". Please check other references!

- This is true; we have now changed a few references to more adequate ones!

c) It would be useful to mention as additional motivation that in recent years several studies have found that UV irradiation of organic matter leads to emission of a number of atmospheric compounds (e.g. CH₄, CO, VOC, H₂, CH₃Cl).

- We have already cited a number of papers on UV radiation induced CO emission from dead leaves. The motivation was that there is a need for data on UV radiation induced CO emission from living vegetation.

2) Materials and methods: 2.1 I find it strange to refer to a flux chamber setup as "ecosystem scale experiment". (also in abstract and throughout the paper). It oversells the chamber results, why not call it flux chamber on a natural grass field.

- In the revised version we now refer to the chamber results from the ecosystem of the C4766

natural grass field as "natural grass field".

2.2.1/2 The UV radiation was not excluded in the chamber experiments, but reduced to 32% (and 17% for the leaf scale exp). It could still have an effect. Is this taken into account? See comment later.

- Very good point! - See response later.

2.2.3 This description is not clear to me at all. Give equation and if it is complicated give an example.

- We are very puzzled by this comment. What equation is it that Reviewer #2 wishes we give? Is it because we wrongfully wrote extrapolations? The rate is simply derived from the observed slope and treatment effects from the change in slopes. - This is now corrected.

3) Results and discussion I do not understand what Fig 1a shows.

- Fig 1a shows measured rates of leaf CO emission of the cut grass land vegetation. - We have now described this more clearly in revised version. - However, Fig 1a is now Fig 1c.

I thought the leaf scale data are shown in Fig2, but these values are different.

- Fig 2 shows measured rates of leaf CO emission of the other species examined in order to gauge the inerspecific variation. - We have now described this more clearly in revised version.

Maybe I overlooked it, but please describe clearly. 3.1.1

Give values for the uptake rate in the dark.

- The value for uptake in the dark is given in Fig 1b. - The value is now also in the text.

What is the relevance of the following statement: "The measured uptake rate of CO in the dark can be approximated as the product of the CO diffusion coefficient of the

top soil and the CO concentration profile in the top soil profile (Potter et al., 1996) according to Fick's first law. Therefore, CO uptake in the grassland is in agreement with the expectation of an active microbial community in the grassland oxidizing the CO (Potter et al., 1996; King and Weber, 2007)." It seems trivial and out of place here. Would you have expected something else?

- To some extent we agree, but a previous reviewer was puzzled by the negative rate, i.e. uptake. No, we did not expect something else. Thus we have included the most obvious explanation.

3.1.2. It would be much clearer to move the discussion to the end when all results have been presented (also for other sections)

- We have decided to maintain the brief format, see comment above.

3.1.3. As mentioned above, the second chamber is not almost completely UV-opaque, but transmits 32% of UV radiation. I see from Fig 1 that the gross flux for full sun is the difference between the full sun and dark net fluxes, but this is not the case for the "Sun excl UV" gross flux.

- Many thanks for pointing out the problems in Fig 1, which were due to calculation errors not. This has now been corrected, but is not related to the 32% UV transmission. For this issue, see below.

Has the 32% UV fraction been taken into account? Please specify clearly how the gross rates are derived.

- Good point! - Gross rates were derived by taking dark rates into account. No, we did not take the 32% UV fraction into account. We simply showed what we measured knowing that the "UV-opaque" was not complete. - However, we have now included numbers taking the 32% UV radiation in account in the revised version.

The last paragraph of 3.1.3 is more a general conclusion and should be separated from presentation of the results.

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- Agreed

3.2.2 Refer to Fig 3, not 4.

- Correct. Thank you for noticing.

Could you compare the slopes in Fig 3 with the ones from the literature quantitatively?

- It is not clear what is meant by this comment?

Regarding the origin, maybe it is useful to relate the discussion on the origin of CO emissions briefly to the suggested origin of other trace gases that are emitted under UV in a short discussion (similar or different mechanisms?).

- True, - we have now discussed this in short.

3.2.3 Why not use the standard notation of the parameters in the Arrhenius equation from textbooks, rather than α and β ? This would also facilitate the comparison to published studies. Replace "subtracted" by "minus" α needs a unit.

- Because we acknowledge that we simply use an exponential regression without the often ignored assumptions about the Arrhenius equation that the rates are maximum rates. At best, Arrhenius equation can be used in a single enzyme function with no limitations to the process. - We have replaced "subtracted" with "minus". - β may be termed a temperature coefficient or temperature sensitivity. Thus in the type of equation used for the regression here, β may have the unit "°C⁻¹" if at all needed.

3.3 first paragraph: have you really shown that emissions from excised leaves are the same than from attached leaves? Is this the conclusion from Fig 1a, which was not clear to me?

- It is not clear exactly what part of the first paragraph of 3.3 that this comment refers to?

second paragraph: The discussion of the role of OH is very cryptic and inconclusive

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although the effect is rather straightforward to estimate. In the chamber experiments in a natural atmosphere, yes, the OH sink will be under operation, and OH levels should not be much different from ambient air levels since both H₂O and UV levels should be similar. BUT: The lifetime of CO versus oxidation by OH is of the order of weeks to months under natural conditions, so the effect is negligible in the experiments with natural sunlight. It could affect lab measurements with artificial sunlight in principle, but only if the UV component is extremely high. I do not understand what the OH effect has to do with the carrier gas flow rate. If the flow rate is higher, then the residence time of air in the chamber decreases, thus also the OH effect, but on the other hand also the emitted CO from the plants. - We intended to emphasize that during measurements we did not control OH and that at a global scale, the presence of OH does not eliminate the importance of investigating UV-induced CO emission from living vegetation. - We agree with the discussion given by Reviewer #2 and believe the discussion in the revised paper appears less cryptic.

Interactive comment on Biogeosciences Discuss., 10, 9373, 2013.

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