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

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Overall, this paper presents important and useful results. The dependence of CO emission on UV radiation intensity is worthwhile data.

One scientiïňĄc issue not fully addressed is the background CO emission of the plexiglass containers during solar irradiation. A control experiment without plant matter should be carried out.

âĂć Please see p. 9376, l. 15-18: 'Blank chamber emissions of CO were examined by placing the chamber over an inert surface (PTFE foil) under different light regimes. The analysis revealed no detectable CO emissions from the chamber itself.'

Another issue is that the experimental section does not describe how the excised

leaves were handled. Where they kept hydrated by placing the stem in water?

âĂć Please see p. 9380, l. 19-24: 'We did not test the effects of a directly measured water status. However, we did measure the CO emission of dried material (litter) and found as others (Tarr et al. 1995, Yonemura et al. 1999, Derendorp et al. 2011) that the CO emission is about one order of magnitude larger in dried leaves compared to that of fresh leaves. Hence, more knowledge is also needed in order to evaluate how a higher degree of leaf desiccation may increase rates of CO emission.'

âĂć Excised leaves were kept unhydrated on a desktop for ca. 10-30 min before measurements.

âĂć Please confer Tarr et al 1995 who could not find an effect of excision on photo-induced CO emission in living leaves.

On page 5, it is unclear if the value 4437 is for the net emission or the dark uptake.

âĂć 'The photo-induced gross release rate of CO can be calculated as the difference between the rates measured in natural sun-light and the rate in darkness, equal to 4437 $\pm$ 464 nmol CO m-2 h-1 for the grassland. ' – so 4437 is neither 'net emission or the dark uptake', but rather the the calculated photo-induced gross release rate of CO.

âĂć This will be reworded to avoid confusion.

There is some confusion in the discussion about UV vs visible light. The authors indicate that UV was not included in estimates, but some studies cited did utilize full spectrum sunlight, which included solar UV. How is it that data from those studies does not accurately account for UV radiation?

âĂć It is unclear, exactly what is meant by this comment:

âĂć If the comment refers to that 'UV was not included in estimates', then it is true that there is yet no global estimate of UV-induced CO emission from living plant material.

âĂć If the comment refers to that 'some studies cited did utilize full spectrum sunlight',

then we wish to point to that we wrote at p. 9379:

‘3.2.1. Natural sunlight Freshly excised green leaves of six different plant species exhibited rates of net (i.e. after subtracting rates from dark measurements) CO release ranging from 965 to 2396 nmol CO m-2 h-1 (mean 1740 nmol CO m-2 h-1) when exposed to natural sunlight (Figure 2). These rates are of the same magnitude as the gross rates (i.e. incl. dark rates) reported by Tarr et al. (1995) by green leaves, 1800 nmol CO m-2 h-1 in response to simulated sunlight (650 W m-2 UV-B + UV-A + PAR), and those by Yonemura et al. (1999) by green leaves, 1300 to 1550 nmol CO m-2 h-1 in response to 490 W m-2 PAR (without UV). In comparison, Seiler et al. (1978) reported a mean photo-induced CO production by living plants of 386 nmol m-2 h-1 in response to 50 W m-2 PAR (without UV).’

Thus by this we did indeed acknowledge that Tarr et al. (1995) measured green leaves in response to simulated sunlight (650 W m-2 UV-B + UV-A + PAR).

âĂć Further at p. 9378 we wrote: ‘To date no other study has explicitly examined the potential of CO emission from living vegetation in response to the full natural spectrum of sunlight’, which remains true as only Tarr et al. 1995 measured photo-induced (incl UV) from living leaves. However, Tarr et al 1995 did not use ‘natural spectrum of sunlight’ but rather artificial light.

The regression line presented in Figure 4 seems to include all data points for both species. However, the two species seem to have very different behaviors. These data should not be pooled for the regression.

âĂć This can be changed.

One typographical error was noted in the references: "Zapp" should be "Zepp". The authors should carefully check all references to be sure no other errors are present.

âĂć This will be corrected.

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