Biogeosciences Discuss., 9, C4567–C4570, 2012
www.biogeosciences-discuss.net/9/C4567/2012/

Biogeosciences Discussions

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

D. Bruhn et al.

dabr@kt.dtu.dk

Received and published: 8 October 2012

Received and published: 24 August 2012 In general, this is a sound manuscript that presents new information on UV-induced CO emission by living plant tissue and sand. The authors fill in a gap left by past studies that either addressed non-living plant material (e.g., Schade and Crutzen) or that did not incorporate UV at all. The authors show that UV matters in all cases, which is hardly surprising. Nonetheless, the results have merit in that the provide an indication of the potential magnitude of the UV impact. My concern about this paper is based on the extrapolation to global impact. The authors are specifically concerned about global budgets, which is fine, but one would think they would therefore be a bit more guarded in their extrapolations.

I am particularly concerned at the sampling time, September-October, only represents

one window on UV impact. The authors assume that UV effects are consistent overtime without presenting any justification for such an assumption.

âĂć We believe the process per se may be photolysis, why the extrapolation of UV effects only would be compromised by a change in the source. Granted, the wax composition of leaves may change with time, why the source may change with time. However, this potential temporarily change in wax composition is largely covered in our experiments indirectly by scaling the results averaged across a range of different plant species. A brief discussion of this is inserted in the revised version.

They also have only a limited survey of plant material, and so again, it is hard to accept the global extrapolation.

âĂć We agree that the number of species tested might be in the lower end. But, as discussed in the manuscript, we believe that the process of CO emission is a non-biological photo-degradation rather than a biotic process linked to e.g. plant physiological status. Thus, we do not see the number of species tested as a major concern, given the relatively low between species variation in CO emission rates under UV (see fig 3). This is further discussed in the revised version, where we also emphasize the facts that current numbers for CO adopted by the IPCC are derived from fewer plant species. âĂć In this context, it is also noteworthy, that current global scale CO numbers accounted for by IPCC (CO emission by live plants due to visible light (Seiler et al., see Tarr et al. 1995 for discussion) are based on fewer species, and the yet unaccounted for (by IPCC) numbers of CO emission by dead plant/litter due to visible light and UV are based on the same number of species as we have used.

Additional factors the authors don't consider include the effects of excision and leaf water status.

âĂć We have tested that there is no significant effect of excision. Granted, we admit that we did no test the effects of a directly measured water status. However, we did meause the CO emission of dried material (litter status) and found as others (see ms for cited references) that the CO emission is about one order of magnitude higher in dried leaves compared to that in fresh leaves. We did not include a discussion of those of our results as we focused on the omitted sources in the literature, living plant material. This will be further emphasized in the revised manuscript.

Thus, while the methods are good and the resulting data has merit as an indication of the magnitude of UV effects, I see little reason accept the extrapolations, at least not without some significant caveats.

âĂć Apparently the two reviewers have opposing opinions on this issue. However, given our responses given above. in combination with the positive statement by reviewer #1: ‘More importantly, the careful extrapolation method the authors present in combination with their observations could be valuable for future research’. We believe the up-scaling should be considered robust and maintained in the manuscript.

A few other points: p. 4, l. 22: : : drop "own"; it’s hard to believe that the production of CO from the chamber was zero, but I’ll accept your report of an undetectable blank value; in my own experience, just about everything emits some CO

âĂć OK, we drop “own”.

p. 4, l. 11: : : is this it? just September and October?

âĂć Yes, this is it.

p. 5: : : so the Walz chamber emitted CO, but the field chamber didn’t? how was the field chamber blank actually measured? was the chamber placed over some sort of inert surface and exposed to various light regimes

âĂć Yes, that was how it was done and we could not detect CO emission. This is further explained in the methods section.

p. 7, l. 20: : : the authors should note that a range of global uptake values have been reported, the KR ’90 number is just one estimate of several.

âĂć True, but KR ’90 number is related to components of the total budget not studied/scaled in this paper.

p. 8: : : did you consider a clipping or biomass removal experiment to manipulate sources of CO?

âĂć Not at the time. However, that would be a great addition to future experiments.

p. 9, l. 25: : : this sentence doesn’t make sense

âĂć OK, perhaps our English is insufficient. We just want to state the potential of an undiscovered source (UV-induced CO emission from living plant material) in the authors discussion of their results. The language has been corrected in the revised manuscript, hopefully clarifying this.

p. 10, top: : : so what depth intervals are you suggesting are responsible for net CO uptake? are you saying that it occurs primarily in deeper soils? How do you reconcile this with various reports that indicate otherwise?

âĂć We are only saying that when using the Walz chamber, no soil profile is present! Further, we speculate that the deeper the soil profile, the greater volume for potential CO oxidizing microorganisms. We do not speculate on the exact depth of the “most active layer” in the soil profile. This has been further clarified in the revised manuscript. Everywhere: English usage needs to be improved âĂć OK, the text has been revised for linguistic corrections..

Interactive comment on Biogeosciences Discuss., 9, 8449, 2012.