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

Anonymous Referee #2

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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 they 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 over

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time without presenting any justification for such an assumption. They also have only a limited survey of plant material, and so again, it is hard to accept the global extrapolation. Additional factors the authors don't consider include the effects of excision and leaf water status.

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.

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

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

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

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.

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

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

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?

Everywhere: English usage needs to be improved

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