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Biogeosciences Discussions

Interactive comment on "Soil and plant contributions to the methane flux balance of a subalpine forest under high ultraviolet irradiance" *by* D. R. Bowling et al.

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This is the first manuscript I am aware of where the hypothesis of biogenic methane production under aerobic conditions is tested with state-of-the-art micrometeorological methods. As with any empirical study it is clear that if the authors do not find significant fluxes at their site this does not automatically imply that this is generally and everywhere the case. I think the authors provide a sound assessment of the issue.

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General Remarks

This is a very carefully done manuscript, and I appreciate very much how the authors lay open their uncertainties and do not hide methodical shortcomings behind sophisticated wording. There are only a few minor points I can comment on and which I would like the authors to consider in their revision.

The major shortcoming of the manuscript is the need for substantial corrections in UVA irradiance measurements, which we accept but which I'd like to mention as a reviewer because this important measurement obviously has not received the exceedingly detailed care as the methane analyzer received. I however can agree with the authors how they approximate the total UVA radiation and do not think that their conclusions are seriously affected by this shortcoming.

Main comments

1.

First of all I found the authors' Figure 3 very interesting, but was not sure I agree with their interpretation presented on page 4775, lines 26–28 ("there were no clear changes in CH_4 associated with precipitation events in the present study, even those later in the season that increased soil moisture"). I have a different view on this graph for discussion and drew a green line into the panel showing CH_4 concentration (see Fig. 1). To me it appears that within 1–2 days after precipitation events the baseline of the concentration tends to increase to higher values. I marked the distict precipitation events with green letters from P1 to P6, and added a question mark where I could imagine a similar response of the CH_4 baseline to the period with scattered showers (days 195–197) that also seem to increase the baseline after a certain time delay. From

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the details given about volumetric soil moisture content measurements I cannot rule out that their measurements are not perfectly representative of the overall soil moisture in the footprint area of the measurements. Footprint areas of mean concentrations as presented in this study tend to be way larger than footprint areas of fluxes, therefore it is not unlikely that a mismatch in scales confounds the comparison of soil moisture content with CH_4 concentration. If one only relates precipitation events with this CH_4 concentration baseline, I would argue that there is a relationship, and the lag would be somewhere between 1 and 2 days.

2.

The sign convention used by the authors is inconsistent. Although clearly specified, it is physically ambiguous to use positive sign for increasing height above ground put the opposite definition of the sign of concentration differences. This confuses the reader in that he must change the signs of all gradients reported in the manuscript. The conventional definition is that an exchange coefficient K_c is a positive entity, and that if distance z is measured positively from the ground surface, then the sign of the concentration differences must be positive if the concentration increases with height (not decreases with height). This leads to the typical flux-gradient equation

$$F_c = -K_c \cdot \frac{\partial \bar{c}}{\partial z} \tag{1}$$

with a physcially consistent definition of signs. I would like the authors to change their conflicting sign convention (e.g. page 4774, lines 13–14, and page 4778). z was defined to be positive e.g. on page 4774, line 3. Consequently, the signs in Equations (1)–(4) must be changed.

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3.

There is no such thing as an approximate accuracy (page 4771, line 14). I would remove this bracket information after measurement trueness.

4.

I do not really agree with the interpretation on page 4779, lines 10–12 ("CO_2 within the canopy during the daytime was lowest at the middle canopy inlet (Fig. 7c), which illustrates a potential problem with analysis of small within-canopy gradients"). Can the authors rule out that the CO_2 used for photosynthesis is not coming from lower layers in the canopy? Only looking at the gradient does not reveal any such potential problem to my eye. I rather think that some ideas and concepts about "counter-gradient fluxes" or small gradients are based on neglect of larger-scale gradients (e.g. from ambient air above canopy to mid-canopy; or from soil surface to mid-canopy). In a turbulent medium the exchange is not primarily diffusive (which your statement implies), but mixing can occur over larger distances. I suggest a moderate rewording here.

5.

On page 4783, line 16 it is argued that a CH_4 emission source, if present, would be detected. From a purely logical standpoint I would think one must add "or it would have to be smaller than what the method currently can resolve".

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Details

In general: do not mix the notations of using negative exponents in units with the notation of using a slash to denote denominators (this applies to the unit nmol mol^{-1}/m which should be nmol mol^{-1} m^{-1} instead, in text and figures)

Figure 2: the tick marks are drawn every 4 nm, which appears to be somewhat unconventional. It would suffice to draw ticks every 10 nm.

On page 4772, line 26 you start including CO without having mentioned this in the introduction. Because you are referring to CO at other places as well it would be welcome to have a short statement in the introduction that prepares the reader to this linkage with CO that you are discussing later in the paper.

P. 4770/ l.10: add “model” before the model number

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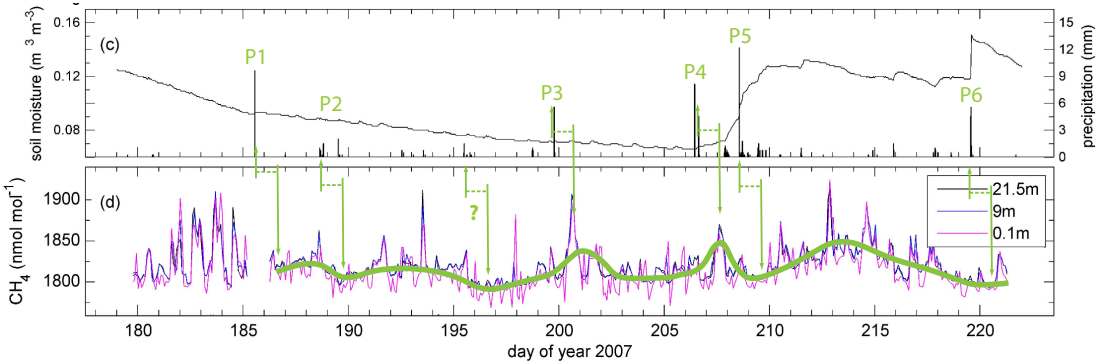

Fig. 1. Alternative interpretation of methane concentrations presented in Figure 3 of the manuscript by Bowling et al. (2009). Green items were added by the reviewer. See text for details.

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