

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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We thank Prof. Dr. Eugster for his thoughtful comments and very careful review. Here we respond to those comments which led to suggestions for changes in the manuscript.

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.

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Reply: We agree. Unfortunately the studies highlighting the role of UV were not published when we did the field work, and we didn't measure UV ourselves. We were fairly fortunate to have access to the NEUBrew dataset, especially so since their site was only 1 km away!

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 CH₄ 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 CH₄ 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 distinct precipitation events with green letters from P1 to P6, and added a question mark where I could imagine a similar response of the CH₄ baseline to the period with scattered showers (days 195–197) that also seem to increase the baseline after a certain time delay. From 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 CH₄ concentration. If one only relates precipitation events with this CH₄ concentration baseline, I would argue that there is a relationship, and the lag would be somewhere between 1 and 2 days.

Reply: Thanks for the effort to look so carefully at this. Indeed there may be some change following rain. We've modified the text to acknowledge the possible relationship: “It is well-known that CH₄ production occurs in anaerobic soil conditions. There were no abrupt changes in CH₄ associated with precipitation events in the present study, although there is some indication of a baseline methane increase 1-2 days following many precipitation events (Figure 2). Rainstorms later in the season that in-

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creased soil moisture (days 208-210) did not substantially increase the CH₄ baseline.”

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

(equation $F_c = -K(dc/dz)$)

with a physically 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.

Reply: Thanks for catching that error. We changed the signs in equations 1-3 (there's no eq. 4).

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

Reply: We've removed the “~”.

4. I do not really agree with the interpretation on page 4779, lines 10–12 (“CO₂ 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 CO₂ 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

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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.

Reply: We agree, the gradient itself does not rule out the possibility that CO₂ from low in the canopy is used for photosynthesis. However, these measurements were made on a flux tower, and NEE showed strong uptake of atmospheric CO₂ during the daytime. That measured flux direction (downward) was consistent with the larger-scale gradient (from ambient air above canopy to mid-canopy). Our point here is to make the reader question whether or not it's ok to use small mid-canopy gradients to infer a flux direction, and we don't think it's ok to do so. We've been pretty open about this. This is why we also focus on the larger-scale gradient with the “daytime excess” concept, and this paragraph is where we provide the rationale.

5. On page 4783, line 16 it is argued that a CH₄ 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”.

Reply: We've added a new phrase to the end of the sentence: “Clear evidence of net photosynthesis in the canopy was obtained, and this provides some confidence that a foliar CH₄ emission source, if present, would be detected (or smaller than our method can resolve).”

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)

Reply: Changed as suggested.

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.

Reply: Changed as suggested.

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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.

Reply: We added this sentence near the end of the introduction “Measurements of CO₂ and CO are used to provide some understanding of expected CH₄ patterns within the forest and of transport of urban air masses.”

P. 4770/ I.10: add “model” before the model number Reply: Done.

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