

## ***Interactive comment on “Leaf isoprene emission in a subarctic wetland sedge community” by A. Ekberg et al.***

**Anonymous Referee #2**

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In this paper some measurements of isoprene emission from subarctic vegetation are reported. An analysis of model performances with emissions by this ecosystem is attempted. The data are interesting but some physiological conclusions are not really well supported. The data set is very poor (e.g. Fig. 1 “...means of duplicate or triplicate samples from single leaves”; Fig. 3, only one sample). I appreciate the enormous difficulty in running field measurements, especially in extreme conditions, but the data set is really too small to allow any realistic statistical analysis and, consequently, any well-supported physiological consideration. Probably also model testing suffers from such a poor data-set.

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## Specific comments:

**Abstract and results:** I doubt that basal emission rate ex Guenther et al. was measured at 20°C. It would be better to rename the emission rate and to state that this is different from Guenther's parameterization.

**Introduction:** the statements about the importance of temperature (5063-12) do not really support the arguable opening statement that it is important to study isoprene emission from subarctic vegetation. Beside showing calculations of ecosystem emission of isoprene, I would like to see an introduction (and discussion) a comparison of fluxes of isoprene by this subarctic ecosystem with tropical and temperate ecosystems. 5064-23: Why the relationship between photosynthesis and isoprene should be different in sedges and in other plant species? Please explain.

**Materials and Methods:** Photosynthetic gas exchange: Asat (see Figures and results) is not defined here! If measurements were carried out fortnightly (5066-4) why only few data are shown e.g. in Fig. 1? 5066-14: "Leaf temperature...as above" redundant, delete. "...and chamber humidity was controlled at ambient level". What does it mean? If it is ambient why do you need to control it? A/Ci measurements, each step requiring 2-3 min? Generally this is not a sufficient time to allow for stabilization of photosynthesis (or, even worse, of stomata). 5066-24: "Chamber conditions...as above". Redundant, delete. 5070-5: Unclear how epsilon was calculated. Please explain (an explanation is only drafted in Fig. 4 legend). 5070-13: Is this another goal of the paper?

**Results:** 5071-25 and following. Too many data are not shown here and throughout the paper. See further comments below. 5072: I like the way results about parameterization are presented. Clearly the coefficients of the Guenther's variables are different. The discussion should focus on how different they are from those found in other ecosystems? And why are they different?

**Discussion:** 5074-1:5: "...basal rates (at 30°C)..." see above, about Guenther's basal emission. It is also misleading to compare measurements at two different tempera-

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tures and conclude that rates are similar. 5074-13: “For cool ...a lag ...”. The lag time between photosynthesis and isoprene induction is larger at cool temperature but it is observed at all temperatures (as better specified at the end of this page). The reason why the parameter cumulative sum of diurnal mean temperatures above 0°C was used is unclear. Why the delay of isoprene emission was only attributed to this index? Is not conceivable that also leaf ontogeny may be delayed and that the effect be an indirect one? Similarly, why isoprene was plotted only against the temperature of the previous 48 h? Why no attempt was made to see if the relationship with instantaneous temperature was similarly good? On one hand the authors stress the differences of their ecosystem from other well-studied ecosystems, but on the other hand they apply a-critically findings obtained in other ecosystems and that may not be valid in their case. The meaning of showing Asat (not defined in M&M, see above) is unclear. To compare with isoprene emission, photosynthesis should be measured under the same conditions (including temperature). This is not Asat almost surely, please clarify and rephrase. 5076-19:27: These sentences are unclear to me. 5077-5078: The part dealing with the uncoupling of photosynthesis with N is interesting but, again, why the low N effect could not be due to the prevalent temperature limitation of photosynthesis under the subarctic conditions? The authors conclude that “It is not possible to draw conclusions...”, and I unfortunately concur with this statement which demonstrates that the data are mostly preliminary, but a more articulated and informative discussion would improve the tone of the paper.

Conclusions: 5079-1:5: This is the statement that I wanted to see since the beginning to compare this subarctic ecosystem with other better known ecosystems. Thus emission rates by sedges are not that high, confirming a diffuse feeling. Again the 48 h temperature effect is reported as a main finding but it would be important to show that this is a better index compared e.g. with instantaneous temperatures.

There are several small language errors over the paper, e.g. cryo-focused (5076-14); to be dominated (5070-20); rigid = rigorous (5077-4).

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