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Biogeosciences Discussions

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

Anonymous Referee #3

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General comments

The study deals with a less studied ecosystem, subarctic wetlands, where measurements were made over two consecutive years. Wetlands cover huge areas in boreal and subarctic regions, and thus they can have a significant impact on the global greenhouse budget, as well as on the emissions of biogenic volatile compounds. The specific constraints for plant growth in in these environments are the short growing season and low mean temperatures, and therefore species adapted to these regions differ in many respects from temperate plant communities. However, only very few and rough estimates have been presented on the actual quantities of emissions, and on their seasonal variability in subarctic regions. This manuscript addresses a few important, basic questions, namely the emission dependence on temperature and the correlation with assimilation capacity, and provides some novel data which are discussed in the light of how to implement these in emission models.

Here the isoprenoid emissions of two dominant sedge species, Eriophorum angustifolium and Carex rostrata, were measured, and the relationships between emissions and several environmental and developmental factors were analyzed. The measurements showed that both species were prominent isoprene emitters, and no other compounds were seen in emission spectrum at any time of the growing season. This is expected, since earlier enclosure measurements (including several, to some extent undefined plant species) and also ecosystem scale measurements have shown that isoprene is the most prominent compound emitted from wetland ecosystems. As has been seen in other cool environments, the emissions were only detected after a threshold of ca. 100 degree days had passed from the last spring frost. A Guenther et al -form model and a mechanistically formulated model by Niinemets et al were used to predict the diurnal and seasonal course of emissions, both showing a good agreement with the measured data.

The topic fits well with the BG scope. The title, however, is somehow misleading, as only leaves from two sedge species were measured instead of a community scale measurement strategy. I suggest changing the title in order to illustrate this better. The manuscript is in general well written and easy to follow. The abstract is clear and gives a nice summary on main results. The aims are well defined, and selection of methods and plant species are adequately justified. Analytical methods are sound and reliable, although some improvement on their description is necessary. Data is presented to show that emissions can be linked with temperature history. The discussion is logical and highlights some very important aspects regarding these specific environments. Relevant literature is included and cited appropriately. Some important conclusions are obtained and potential further research guestions highlighted.

Especially the discussion on effects of the recent temperature history, and longer-term temperature fluctuations (p. 5075), is very interesting, and shows how complicated the

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plant responses to environment can be. The authors discuss very nicely how some indices, e.g. temperature averaged over several weeks can actually reflect totally other type of responses and drivers than those regulating emissions in shorter time scales. The basal emissions varied significantly along the season, and a good fit was found when the emission was plotted against the temperature history of past 48 hours. Thus, a conclusion was made that especially in cool environments, the way the temperature history is taken into account in models may greatly influence the results. However, no other fits were attempted or at least were not shown, and so the reader is left puzzling about the significance of this important result.

There was not much discussion on why the species specific relationships between assimilation and emissions are quite different. The lack of diel emission measurements from C. rostrata and the very small amount of data from E. angustifolium were obviously prohibiting a deeper analysis. The authors properly admitted this and tried to avoid doing overly bold analyses from the limited dataset available, however some discussion could perhaps be added based on the known ecological or physiological differences between these two graminoid species.

The basal emission factor is normally determined using T=30C. However, in cool regions daily maximum temperatures may even in high summer remain well below 20C, and plants very seldom experience 30C, which gives justification to the use of T=20C for normalizing the emissions. This should be stated and discussed somewhere in the manuscript. The simple model excercises showed remarkably good fit with the unfortunately rather sparse emission data available for parameterization. Both the semi-empirical and mechanistic models performed almost identically. It is not much discussed why these models are so similar, even though their structure is so different. Is your conclusion that the semi-empirical models are sufficient and that mechanistical models should not be attempted for making scenarios of emissions in future climatic conditions? I find 49% difference between measured and modeled emissions quite large (p5076 line 20). What other factors could be involved?

The main concern I have regarding the manuscript is the quantity of data: it is very limited and/or not adequately described, and thus the conclusions can be questioned unless some clarfications are given (see specific comments below). Also the model excercises with such a limited dataset are highly sensitive to over-parameterization, and thus the results should be considered as tentative.

Specific comments:

- Section 2.2-2.3: You should give more details on the selection of plants and sampling. For instance: were all sampled leaves of the same age? Carex rostrata is continuously growing new shoots throughout the growing season, and leaf developmental age may be totally different depending on when you sample, unless you standardize the sampling. This also influences the leaf area measurements and leaf N contents, and thus the calculated total annual emissions and regressions with leaf N. For a reference on Carex see Saarinen (1998) Annales Botanici Fennici 35:203-209.

How many plants were sampled? You should give the number of sampled plants somewhere, and also calculate the averages (e.g. table 1) using N of plants as a replicate instead of N of leaves; which one did you use here is not clear to me. Did you measure same plants each time? There are no error bars in figures, please add these. Using Student's t-test you need to assume that the data is normally distributed and that variances are equal; did you check for these?

Why are A/Ci curves not shown, although they were measured? Instead only Asat values are given in Fig 1. What were the CO2, T and PAR at Asat measurement, were they constant throughout the measured periods? How well does the 2-3 min measurement time reflect a steady-state photosynthesis? Please give more details on measurements and show some more data.

- Section 2.3.: What were the blanks? Did you sample the incoming air? In the PTR-MS analyses you should name the mass you used for isoprene detection; was it M69? The M69 can also contain some other compounds, such as MBO. Did you calibrate the

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PTR-MS with standard isoprene?

- Section 3: To me the emission measurements seem to differ quite a lot between years, in spite of seemingly similar temperature and assimilation. You claim that in year 2006 the onset of emissions was seen during the measurement campaign. But why this was not the case in year 2005? How do you interpret the huge emission peak from E. angustifolium in the first emission measurement in 2005 (Fig 1c)? It is unfortunate that in 2006 it seems like you stopped measuring too early in order to see the emission decline, since the emissions are almost at maximum in August, whereas in the previous year the August emissions were considerably lower. Did higher temperatures in 2006 cause this difference?

From Fig 2e it seems that in 2005 the correlation with respiration was not similar as in 2006, as the outliers are all from 2005. Any reason for that? Fig 5 could include the measured values as dots so that a comparison between model and data would be easier.

Interactive comment on Biogeosciences Discuss., 5, 5061, 2008.