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Biogeosciences Discussions

Interactive comment on "Nonlinear controls on evapotranspiration in Arctic coastal wetlands" *by* A. K. Liljedahl et al.

Anonymous Referee #1

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Review of Liljedahl et al. (BG-2011-201)

This paper reports multi-year eddy covariance measurements of surface evaporation, hydrologic conditions, and vegetation characteristics for two coastal wet tundra ecosystems at Barrow, Alaska. This topic is important because the surface energy partitioning and moisture fluxes of these systems is expected to be sensitive to climate changes, with potentially large feedbacks on biogeochemical (CO2, CH4) fluxes. The paper is clearly written and it does a particularly good job of detailing the site and vegetation characteristics. The paper appears to be appropriate for the special issue because of the water table manipulation at one of the sites. My overall assessment is that the paper could be published with revisions (see below) that are important but that nevertheless would not be very difficult or time consuming to fix.

Major comments

1. This paper makes a major point (e.g., p 6309, line 24ff) that it represents one of the few multi-year eddy covariance studies in the Arctic. This is a good point, but the paper should do a better job of explaining what new insights come from these long-term data as well as specifically comparing them to the previous, short-term measurements. As a first example, the seasonal and interannual changes in moisture at the sites are interesting (Fig 2), but why not include a series of plots on the same time axis showing how energy partitioning or ET varied? This would show seasonal and interannual responses, as well as the effect of the water table manipulation. In fact, most of the data in the results and tables appear to be means (see comment #2 below), which brush over the details we would see by comparing seasons within years and years against other years. If you do not have space to go into detail on seasonal patterns here, perhaps just explain better how you are using the long-term data only for seasonal variations (e.g. in VPD) at this time.

As a second example, although the paper compares its results to plant physiological data from the literature, it does not put the actual ET and hydrologic results into context against other arctic eddy flux measurements. For instance, the authors say it is striking that late summer LE/Rn was so similar at 35-38% among dry and wet soils (p 6320, lines 15ff). Interestingly, the ARCSS Flux Study found the same value (38%) as the mean of a variety of tundra vegetation types in Alaska, including inland wet sedge tundra (McFadden et al. 1998, JGR). How do the authors interpret this in terms of what their long-term measurements tell us... Is energy partitioning relatively constant over the summer season such that long-term measurements see the same values as a short-term campaign? Or are there compensating variations during the season such that the seasonal mean turns out to be close to a short-term, mid-summer measurement? Does it suggest that there is little year-to-year variation in partitioning, given that the flux measurements represent several different years yet have a similar value? Does it suggest that there are relatively small spatial differences in partitioning (i.e.,

from Barrow to inland on the N Slope) so long as you are comparing wet tundra sites? The same type of questions could be asked about surface conductances, McNaughton-Jarvis omega, P-T alpha, etc. in comparison to both inland and coastal Alaskan wet tundra (above reference and McFadden et al. 2003, Ecology). Further short-term eddy covariance measurements that report the same ET variables used in this paper include Soegaard et al. (2001 Theor. Appl. Clim.) and the review by Eugster et al. (2000, Glob. Ch. Biol.). The key point is to specifically compare your results to the literature values from short-term eddy flux mmts (at least the relevant AK wet tundra sites), see where they are the same or different, and interpret what new understanding we should take away from this.

2. The paper needs to provide more details about the time periods of the flux measurements and what the aggregated values represent. First, the methods say that 5 years and 3 years of data were collected (p 6314, line 3ff), but the results show summer data from mid-June to August. A table should be added after the current Table 1 showing the beginning and ending date of the flux measurements used in this paper for each study year and site. In addition, the methods should explain what percent of the time valid flux measurements were available each year, and if there were any long gaps in data coverage for certain sites/years. Was any type of gap-filling used for any of the variables that are being reported here (for day-long gaps; I see that short gaps were interpolated at least for LE)? Second, it is not clear what the aggregated values represent, such as those in Results Sec 5.2 and Tables 2 and 3. Are all of these means or something else, like medians? What time window was used in constructing the means? Are you using only daily means or sums? Were there gaps and was there any type of filtering or data selection used, such as fair weather days, or were "wet canopy" or storm periods excluded, etc?

Minor comments

3. Please include another sentence or two in the methods to explain what is happening in the water table manipulation and how you would expect the experimental conditions

to differ from the other site, or from an un-manipulated site?

4. p. 6315, line 20: Why were negative latent heat fluxes assigned a value of zero? Negative LE would occur with dew deposition or frost, which both occur frequently in the Arctic. I could see doing this if you were comparing against another type of measurement that does not capture downward water vapor fluxes (such as sap flow of shrubs), and I could see why you might simply screen negative values out of your analysis (if you want only actively evaporating or clear conditions), but it's not clear why you would change them to zero.

5. p. 6320, line 15: Please use a symbol for "net radiation" such as Rn.

6. p. 6322, line 12: Change to "....SOIL moisture,..."

7. Sec. 6.1. Please be specific about which seasons of the year are expected to be impacted by climate change in which ways. Predicted warming is not greatest in summer, but the results in this paper deal only with the summer season.

8. Sec. 7, last sentence. I think it would be more accurate to turn this sentence around to say that a major caveat of your conclusion is that ET would remain relatively constant only if there are no major changes in vegetation or microtopography (thaw effects could cause changes in drainage that could be greater than the variability observed here).

Interactive comment on Biogeosciences Discuss., 8, 6307, 2011.