

## ***Interactive comment on* “The exchange of carbon dioxide between wet arctic tundra and the atmosphere at the Lena River Delta, Northern Siberia” by L. Kutzbach et al.**

### **Anonymous Referee #2**

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The manuscript reports on the CO<sub>2</sub> exchange between wet arctic tundra and the atmosphere as measured by the eddy covariance method. The authors present a thorough description of the measurement system, data processing and the obtained results. The paper is well written and the subject fits to the scope of Biogeosciences. Even though such CO<sub>2</sub> flux measurements have become very popular during last years, the studies in the arctic ecosystems are still rare. I would recommend publication of this manuscript after minor revision.

1. (p. 1985, ln. 8-12) Last sentence of the Conclusion considers the influence of CH<sub>4</sub> on the carbon balance. This should really be presented in Discussion. Actually,

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it would be great to have here the actual CH<sub>4</sub> emission rate instead of the expression "2-3% of carbon released by Reco". At least it would be more informative to present the emission as percentage of the annual/seasonal NEE estimate.

2. (p. 1973, ln. 4-5) Leaf area index (LAI) for the site is presented in Discussion. However, as LAI is such an important parameter describing the ecosystem in question, I would rather see it in the site description. Is the presented LAI total or projected? The authors also present LAI for mosses. Does that mean the moss coverage with a maximum value of 1 or is it something more complicated?

3. Presenting the years 2003 and 2004 in reverse order in Figures 7 and 8 is well justified in order to present the whole growing season cycle. However, it would be fair to mark the change of the year somehow in the figure (for example thin/ dashed line). This could also be stated more clearly in the figure legend in order to understand it without reading the whole paper.

4. (p. 1969, ln.11) "During this time, due to the release of large amounts of latent heat, the soil temperatures remained for a long time at 0degC, whereas Reco decreased steadily. This indicates the importance of the contribution of above-surface biomass to overall ecosystem respiration at the tundra site." I can't see straight away sufficient ground for this conclusion. 1. Reco is the modeled respiration and on the short term it mostly indicates the changes in surface temperature, not the actual respiration rate. Is this conclusion based on modeled Reco or observed NEE? 2. Does the "contribution of above-surface biomass" mean decrease in the autotrophic or in the heterotrophic respiration (of the new litter)? 3. Isn't it possible that this decrease is due to gradual decrease in the activity in deeper soil layers - even though the soil temperatures are close to 0 degC.

5. References: In text, the citations to Lloyd (2001) and Sturm et al (2001) are missing the a/b -extension (p. 1963, ln. 12; p. 1980, ln. 20; p. 1983, ln. 2)

6. (p. 1976, ln. 19-22) "...phenological events such as bud break in spring...are con-

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trolled by...also the photoperiod...". Does the photoperiod have a significant influence on bud break or other phenological events in spring/ early summer? Is there a reference for that?

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**BGD**

4, S1206–S1208, 2007

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